

Cv12-H03474-8-0293409

293409

# AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

FEBRUARY 1971 301.05 VOLUME 36 NUMBER 1

Am 35

Vol. 36 Nos. 1-3

FEB. - JUN

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The *American Sociological Review* is published at 49, Sheridan Avenue, Albany, New York, bi-monthly in February, April, June, August, October, and December.

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Annual membership dues of the Association, including subscription: Student, \$8; Associate, \$15; Associate Foreign, \$8; Active, \$20; Fellow, \$30.

Application for membership and payment of dues should be made to the Executive Office.

Subscription rate for non-members, \$15.00; institutions and libraries, \$20.00; non-member students, \$7.00. Single issues, \$4.00. New Subscriptions and Renewals will be entered on a calendar year basis only.

Change of address: Six weeks' advance notice to the Executive Office, and old address as well as new, are necessary for change of subscriber's address.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply missing copies when losses have been sustained in transit and when the reserve stock will permit.

Second class postage paid at Albany, N. Y.

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Fellows and Active and Student members of the Association may submit manuscripts of 100 to 300 typed pages for publication in the *ASA Arnold and Caroline Rose Monograph Series in Sociology* to the Series Editor, Shekion Stryker, Department of Sociology, Indiana University, Bloomington, Indiana 47401.

## ITEMS

■ **R. Farley** is Associate Professor of Sociology at the University of Michigan. His investigation of the structure of black families is part of an ongoing research project concerning the demography of blacks in the United States; earlier studies pertained to population growth and residential segregation. Later his attention will be centered upon racial differences in socioeconomic status. **A. I. Hermalin** is Research Associate in The Population Studies Center, University of Michigan. His participation in this study of family stability grew out of his family research on sibling homogeneity. Although this work continues, his current research centers on the effect of family planning on fertility in Taiwan and the relation of fertility patterns to changes in the age and marital structure.

■ **R. E. Mitchell** (Professor, Department of Urban and Regional Planning and Director, Survey Data Center, Florida State University) has recently completed a book-length manuscript on: "Housing, Urban Growth, and Economic Development," a comparative study of the social and economic aspects of housing in the developing world. He is currently writing on the significance of balanced growth patterns, with special emphasis on the stresses created by rapid urbanization and industrialization in Southeast Asia; he is also engaged in a study on public support for new legislation in Florida. Other current activities touch on population and family-planning issues in Asia, as well as secondary schools and their pupils in Hong Kong.

■ **H. Schuman** is Associate Professor of Sociology at the University of Michigan, where he also directs the Detroit Area Study. He is currently analyzing both national and DAS data on black and white racial attitudes and experiences. His long-run interests include continuing research on the validity of sample interview surveys, along with a concern to expand survey methodology in both experimental and observational directions. **G. Lenski** is Chairman of the Sociology Department, The University of North Carolina at Chapel Hill. He writes that projected work includes a study of trends in stratification in North Carolina from colonial times to the present and an examination of experiments with income equalization in eastern Europe and their consequences from 1917 to the present.

■ **G. A. Hillery, Jr.**, Professor of Sociology at The University of Iowa, is currently collecting data to test hypotheses on freedom and social organization, the main title of his article in this issue. His basic problem, he states, is in operationalizing freedom, conflict, and commitment; in measuring their relation to such variables as anomie and alienation; and in establishing parameters of these measures among total institutions and intentional communities. On the basis of this work, he intends to expand the study of freedom and conflict to more normal communal and formal organizations.

■ **E. O. Hoiberg** is a doctoral candidate at The University of Nebraska, with major interests in urban sociology. **J. S. Cloyd** is Associate Professor of Sociology at The University of Nebraska; his research specialties include role theory and the sociology of the small group; his teaching specialties include urban sociology and collective behavior. Hoiberg and Cloyd are currently engaged in a study of residential location and social contacts. They

are also exploring the use of status contour analysis as a device for longitudinal analysis of changes in community residential structure.

■ **Joseph Woelfel**, University of Illinois, is currently engaged in theoretical research on attitude formation and change; **Archibald O. Haller**, University of Wisconsin, is continuing his study of stratification and status mobility.

■ **L. D. Haber** is Director of the Division of Disability Studies, in the Office of Research and Statistics, Social Security Administration. He is currently involved in a series of studies on the epidemiology of disability, including factors affecting the ability to cope with impaired capacities, and work adjustments among the recently disabled. His interests focus on the application of social research to social policy problems. **R. T. Smith** is Associate Professor in the Department of Behavioral Sciences, at Johns Hopkins School of Public Health. He has been engaged in studies of long-term illness and disability, as well as social mobility factors associated with the etiology of chronic diseases. He is currently involved in health manpower research, with a major focus on organizational change and innovation, and in a study of professional career mobility.

■ The article by **A. F. Blum** (Assistant Professor of Sociology at New York University) and **P. McHugh** (Associate Professor of Sociology, City University of New York) is part of a wider investigation into the relationship between theory and practice. This work includes studies in deviant behavior, social stratification, the rise of mathematical theory, positivism, and the analysis of films.

■ **K. G. Jöreskog** (Educational Testing Service) has recently published (Biometrika, [1970], 57, 239-251) a general model for the analysis of covariance structures which, e.g., includes "path analysis" and factor analysis as special cases. Other work relevant to sociologists is a general method for estimating a linear structural equation system (available from author) which allows for both errors in equations (residuals, disturbances) and errors in variables (errors of measurement). **R. L. Linn** and **C. E. Werts** (ETS) have been considering applications of Jöreskog's general model, especially with regard to growth and its determinants in the context of panel data.

■ **Keith F. Otterbein**, Associate Professor of Anthropology at SUNY, Buffalo, is engaged in the anthropological study of war; Caribbean family organization is his other research speciality.

■ None of our editorial procedures are sacred and all are subject to revision. Recently the exclusion of comments on the ASA presidential address has been questioned. Why exclude such comments? The answer lies in the nature of the presidential address and the conditions of its appearance in this journal. In his address, the President *ex cathedra* is free to express his personal opinions on various topics and to depart in other ways from the requirements of a research article. However, ASR does not make "equal space" available for the expression of counter opinions, by reason of our very nature (we are not a journal of opinion). Since this position may strike some persons as dogmatic, we have referred the whole matter to the ASA Publications Committee,

# AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

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## NEW EDITOR of American Sociological Review

After June 30, 1971, all manuscripts submitted for review should be sent to:

**JAMES F. SHORT**

Department of Sociology, Washington State University, Pullman, Wash. 99163

## ITEMS

■ **James F. Short** has been appointed Editor of ASR for the period 1972-1974. As of July 1, inquiries about the REVIEW should be sent to Short (Department of Sociology, Washington State University, Pullman, Washington 99163), except insofar as they deal with Vol. 36, 1971. **Dennis Wrong** has been appointed Editor of the new book review journal, and correspondence about books and the like should be sent to him after June 1, 1971. It is a matter of both pride and pleasure to report these forthcoming changes; in the meantime, we will do our best to keep the faith and preserve the heritage.

■ In this issue, we publish several letters on book reviews and thereby depart from standing policy. We recognize that the new working rule is subject to abuse and that irate authors may use it for therapy. However, we are willing to experiment with the new pattern and to exercise control, as required, to assure its purpose of maintaining a critical dialogue on recent literature.

■ **H. H. Hyman** (Wesleyan U.) has recently completed a monograph on the principles and potentialities of secondary analysis, and is now developing a training program for advanced undergraduates at Wesleyan in the secondary analysis of sample surveys. **C. R. Wright** (U. of Pa.), who specializes in research on the sociology of communications, is currently exploring the use of secondary analysis of survey data to extend his earlier research on response to communication. The present article is a continuation of work reported in ASR in 1958.

■ **D. L. Featherman**, Assistant Professor of Rural Sociology at the University of Wisconsin, Madison, is currently engaged in studies of motivational and cognitive effects on status attainment processes in the general population and in population subgroups. He has embarked on a replication and extension of Blau and Duncan's survey of the American occupational structure.

■ **K. H. Thompson** is Assistant Professor of Political Science at the University of Southern California. His examination of upward social mobility and political orientation in the United States is part of a cross-national study of the effects of selected social structural variables on political attitudes and behavior.

■ **M. Rosenberg** (NIMH) and **R. G. Simmons** (U. of Minn.), whose study of children's attitudes is reported in this issue, are completing a monograph on factors affecting the self-esteem of black and white children in an urban setting. Dr. Simmons is also extending a concern with self-image to the realm of kidney transplant patients and their families.

■ **C. J. Lammers**, Professor of Sociology, University of Leiden, is currently working at a general introduction to the field of comparative organizational sociology; engaged in a study concerning strategy and tactics of university authorities with respect to student opposition; making an analysis of participation by the Leiden faculty in the public debate on university reform; and partaking in a research project concerning growth and development of hospital organization in the Netherlands.

■ At the present time **R. Centers**, Professor of Psychology, UCLA, is concentrating on interpersonal attraction and interpersonal power. In addition to studies of attraction, studies of power distribution and power balance in intersexual dyads of various levels of intimacy are now in progress.

**B. H. Raven**, Professor of Psychology, UCLA, has a long-standing interest in the bases of social power in interpersonal situations, which he has examined in group laboratory experiments. He is currently applying the same analysis to social power in field settings. **A. Rodrigues** (Pontificia Universidade Catolica Rio de Janeiro, Brazil) principal research activity revolves around cognitive balance phenomena.

■ **E. A. Holdaway** is Associate Professor of Education at The University of Alberta. His current research activities include the examination of staffing differentiation in educational organizations and career patterns of teachers. He is also investigating the organizational structure of two-year colleges in Alberta and British Columbia. **T. A. Blowers** is a doctoral student at The University of Alberta; he is presently interested in approaches to supervisory staffing in school systems and the problems of inner city areas and their schools.

■ **L. G. Carr** is a doctoral candidate at the University of North Carolina at Chapel Hill. His study of the Srole scale is part of a substantive study of the effects of civil rights activity on attitudes.

■ **M. W. Meyer**, Assistant Professor at Cornell University, is continuing his study of organizational structure. He plans to collect data on city, county, and state finance departments to test causal assumptions implicit in most comparative research. **N. P. Hummon** is currently in his last year of doctoral study in the Department of Sociology, Cornell University. His research interests include methodology, mathematical sociology, and organization theory. **P. M. Blau**, Professor of Sociology, Columbia University, is continuing his comparative study of organization. He is revising his theory and is testing that revision on different types of organizations: government agencies, department stores, universities and colleges, and hospitals.

K. S.

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*The American Sociological Review* is published at 49 Sheridan Avenue, Albany, New York, bi-monthly in February, April, June, August, October, and December.

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Annual membership dues of the Association, including subscription: Student, \$8; Associate, \$15; Associate Foreign, \$8; Active, \$20; Fellow, \$30.

Application for membership and payment of dues should be made to the Executive Office.

Subscription rate for non-members, \$15.00; institutions and libraries, \$20.00; non-member students, \$7.00. Single issues, \$4.00.

New Subscriptions and Renewals will be entered on a calendar year basis only.

Change of address: Six weeks' advance notice to the Executive Office, and old address as well as new, are necessary for change of subscriber's address.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply missing copies when losses have been sustained in transit and when the reserve stock will permit.

Second class postage paid at Albany, N. Y.

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## NEW EDITOR of American Sociological Review

After June 30, 1971, all manuscripts submitted for review should be sent to:

**JAMES F. SHORT**

Department of Sociology, Washington State University, Pullman, Wash. 99163

## ITEMS

■ **J. C. McKinney** is Vice-Provost and Dean of the Graduate School at Duke University. He is continuing his long-term work on both the process of typification and theoretical aspects of social systems analysis, in addition to recent work on trends in graduate education. **L. B. Bourque** is Assistant Professor of Sociology at California State College, Los Angeles. Her primary interests are in methodology and social psychology, and her current research is on social psychological aspects of aging.

■ **R. D. Gastil** is currently a Fellow at the Battelle Seattle Research Center. His interests include studies in social indicators and international and military affairs. His work on homicide is part of a larger effort to analyze American society in terms of regional variation.

■ **S. Spilerman** is an Associate Professor of Sociology at The University of Wisconsin (Madison), effective September 1, 1971. During the academic year 1971-72, he will be at the Russell Sage Foundation to conduct research on lynchings in the South, 1882-1935. He writes that he is seeking a pica typewriter so that he may make his own ASR tables.

■ **V. Jeffries** is an Assistant Professor of Sociology, San Fernando Valley State College. He is currently engaged in research on the structure of ideological systems, cultural values and solidarity, and majority group reactions to protest movements. **R. H. Turner** is Professor of Sociology, University of California at Los Angeles. He is currently investigating the state of theory in collective behavior. **R. T. Morris** is Professor of Sociology, University of California at Los Angeles; his current research and writing include studies of the professionalization process and majority group reactions to protest movements.

■ **G. E. Stephan** is affiliated with Western Washington State College. His general research interest lies in the historical geography of populations and their social institutions. He is currently engaged in testing the generality of the size-density relationship, broached in this issue of ASR.

■ The article by **M. L. Kohn** (Chief of the Laboratory of Socio-environmental Studies of NIMH) reflects his long-standing interest in the effect of a person's position in the social structure on his values and orientation. In his previous work, he has focused on social class position, introducing occupational conditions primarily to explain class effects. The analysis presented here shifts the focus to occupation.

■ **J. Brewer** is an Associate Professor of Sociology at Wesleyan University in Middletown, Connecticut. His article on the flow of communications reflects his continuing interest in the processes of bureaucratization and in problems of organizational re-

search design. He is presently engaged in research on the structure of elite roles within professions and the administration of autonomous professional organizations.

■ **B. F. Meeker** is Staff Associate to the Committee on Basic Research in Education, administered by the National Research Council. Her analysis of exchange theory grows out of a recently completed series of laboratory experiments dealing with social exchange processes. She is presently developing models which will relate the results of those experiments to exchange theory. **J. D. Martin** is an Associate Professor of Sociology at Lakehead University, Thunder Bay, Ontario, effective July 1, 1971; **L. N. Gray** is Assistant Professor of Sociology at Washington State University. Both have an interest in sociological methodology.

■ **D. F. Alwin** and **C. W. Mueller** are graduate students in sociology at The University of Wisconsin, Madison. **R. A. Rehberg**, who supplies a rejoinder to Alwin and Mueller's criticism, is an Associate Professor at SUNY, Binghamton.

■ **T. G. Harvey** is an Assistant Professor of Political Science at The University of Western Ontario. He is currently engaged in research on social correlates of attitudes toward freedom of speech in a Canadian community. **D. L. Phillips** has recently joined the faculty of the University of Amsterdam, the Netherlands, where he will continue to concern himself with meaning and interpretation in sociological investigations. **K. J. Clancy** earned his Ph.D. from New York University in February 1971, and is presently Vice-President and Senior Associate Research Director of Batten, Barton, Durstine & Osborn, Inc. He is continuing his research on bias in survey studies. **J. DeFronzo** is a Lecturer in Sociology at Indiana University, Fort Wayne. He is engaged in research on problems of the inner city.

■ **L. G. Vargo** is a graduate student and Instructor in Sociology at the University of Colorado. He specializes in models of social processes. **D. G. Schwartz** is a Senior Scientist in the Department of Applied Science at Brookhaven National Laboratory, with specialties in kinetics and thermodynamics. He is currently engaged in research on kinetic models of population (human and cell) dynamics. **S. Schwartz** is affiliated with SUNY at Stony Brook. **L. S. Mayer** is an Assistant Professor at Ohio State University, with a joint appointment in political science and statistics. He is also Assistant Director of the Behavioral Sciences Laboratory at Ohio State. **S. Labovitz**, University of Southern California, will move to the University of Calgary, Alberta, Canada, in September, 1971. He is currently engaged in research on the measurement of social norms, conflict in formal organizations, situational risk and suicide, and the effects of technology on social change.

K. S.

# AMERICAN SOCIOLOGICAL REVIEW

FEBRUARY, 1971

VOLUME 36, NUMBER 1

## FAMILY STABILITY: A COMPARISON OF TRENDS BETWEEN BLACKS AND WHITES \*

REYNOLDS FARLEY AND ALBERT I. HERMALIN

*University of Michigan*

*American Sociological Review* 1971, Vol. 36 (February):1-17

*From the 1890s to the present, writers have commented upon the instability of Negro family life. Most have observed that discrimination in the job and housing markets have made it difficult for black men to support their wives and children. As a result, desertion occurs commonly. Family stability has been of interest because of the belief that children who grow up apart from their parents will be adversely affected. Indeed, some investigations imply that being raised in a home which did not have both parents is linked to lower rates of achievement in school, higher rates of delinquency and lower occupational status.*

*While commentators have discussed family stability, there has been little consensus as to how this concept should be measured. Moreover, there are only a few demographic indicators available for operationalizing this concept, particularly if one desires to study long-term trends or to compare blacks and whites. The major portion of this paper examines Negro and white trends on a number of indicators related to a specific definition of family stability.*

*This study concludes that (1) the majority of both blacks and whites are in the statuses indicative of family stability. Contrary to the images which are sometimes portrayed, most black families are husband-wife families, and the majority of black children live with both parents. (2) In every comparison, the proportion of people in the status indicative of family stability is greater among whites than among blacks. (3) In recent years there have been changes in family status, although most of them have been small. Some changes suggest a trend toward greater stability while others indicate a trend in the opposite direction.*

### A. INTRODUCTION

THE Moynihan Report generated interest in the family structure of blacks in the United States (U.S. Department of Labor, 1965). Moynihan claimed that among blacks, particularly among those at lower socioeconomic levels, there was a trend away from family stability. He argued that the increasing breakup of families was unfortunate since it meant that a larger proportion of young blacks grew up without the helpful influence of both parents. This in turn, Moy-

nihan argued, was a major reason why blacks were making only limited gains during the prosperous 1960s. The Moynihan Report thus implies that there is a measurable aspect of family structure, i.e., stability, whose level differs for whites and blacks, which has been changing over time, at least for blacks, and which has a discernible influence on life chances. This paper analyzes the four components of this argument, with major emphasis on the first three.

We seek first to clarify what is meant by family stability by reviewing some of the socio-demographic indicators that have been used to measure the concept. We then present time series for whites and blacks so that the relative levels for the four types of indicators and changes over time can be observed. The final section briefly reviews some

\* This paper was presented at the 1970 meetings of the Population Association of America in Atlanta, Georgia, April 17, 1970. Support for this study was provided by the Social and Rehabilitation Service and the Social Security Administration, Grant Number CRD-458. The authors thank Mrs. Beverly Duncan for her comments.

of the evidence relating indicators of stability to life chances.

For many years sociologists have commented about family stability, but there is no consensus as to how it is measured or how trends over time can be assessed. DuBois studied black men in Philadelphia in the 1890s and found that many of them could not afford to marry and that some of those who married sent their wives or children to the South because they could not maintain their own households (DuBois, 1967:164-196). Frazier (1939: Chaps. xv and xvi) observed blacks who lived in Northern cities during the 1920s and 1930s and believed that desertion and illegitimacy were common—findings corroborated by the Drake and Cayton study of Chicago (1962: Chap. 20). More recently, the black ghettos of Washington have been studied by Liebow (1967) and Hannerz (1969). They report that many marriages are terminated and that premarital child-bearing occurs frequently. Consequently black children are often raised in households which do not contain both parents.

These observations suggest that the marital status of adults, their living arrangements, and the type of households into which children are born and in which they are reared, all have a bearing on family stability. Thus, one might derive the following definition of stability: A stable family system is one in which adults marry and live with their spouses, in their own households and in which children are born into and raised in such a household.

On several counts, however, such an overarching definition of family stability is not too useful. Data that would enable households in the United States to be classified as "stable" or "unstable" according to this composite definition are not available. Even if such a classification were available, it is possible that the trends of the separate components are not all alike and that some differ from the trend in the composite measure. This information would be lost by reliance on one composite measure. Lastly, insofar as stability has some affect on life chances, it is of theoretical and practical interest to determine which components of stability are most related to educational and occupational outcomes and to social and

psychological variables deemed to influence these outcomes.

For these reasons we concluded that it would be useful to analyze separately the various dimensions of stability. Demographic tabulations were examined to obtain indicators of these dimensions. This search revealed four types of indicators for which measures were available for two or more points in time: (1) the current marital status of adults; (2) the distribution of families by type and the proportion of men and women, respectively, who headed families; (3) the number and proportion of illegitimate births and the rate at which unmarried women bore children; (4) the family living arrangements of young children.

Subsequent sections of this paper summarize trends in each measure of family stability. Trends among blacks are described and then contrasted with the trends among whites. This paper analyzes data for the national population. It is probable that the trends in certain localities or among people with certain social or economic characteristics are quite different from the national trends.

#### B. CURRENT MARITAL STATUS

Adults cannot maintain a stable family, according to our definition, unless they marry and live with their spouse. Thus changes in marital status may be one indicator of trends in family stability (Glick, 1969:158-166). Censuses since 1890 have provided marital status data, and more recently the Census Bureau has annually collected this information. Adults have been asked if they are single, that is, never married; or if they are currently married, widowed or divorced. Table 1 presents tabulations of marital status for years between 1890 and 1969. These data have been standardized for age to eliminate confounding effects of changes in age structure.

The proportion of blacks who report themselves as single has fluctuated between 24% and 28% for males and 16% and 20% for women. For both sexes, the low point was reached in 1950, reflecting the high marriage rates of the post-World War II period (Glick, 1957: Chap. 6). Since 1950, the proportion

## FAMILY STABILITY: BLACKS AND WHITES

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Table 1. Distribution of Negroes (N) and Whites (W), Age 15 and Over, by Current Marital Status, 1890-1969.<sup>a</sup>

Year	Men								Women							
	Single		Married		Widowed		Divorced		Single		Married		Widowed		Divorced	
	N	W	N	W	N	W	N	W	N	W	N	W	N	W	N	W
1890	26%	31%	67%	63%	7%	6%	..	..	18%	23%	56%	60%	25%	17%	1%	..
1900	27	32	63	61	9	7	1%	..	19	24	55	57	25	18	1	1%
1910	25	31	65	61	9	7	1	1%	17	23	57	60	25	16	1	1
1920	25	31	65	62	9	6	1	1	17	23	57	60	25	16	1	1
1930	25	29	64	64	9	6	2	1	16	23	56	61	26	15	2	1
1940 <sup>b</sup>	28	30	64	64	7	5	1	1	18	23	56	61	24	14	2	2
1950 <sup>b</sup>	24	25	67	69	7	4	2	2	16	19	61	66	20	13	3	2
1960 <sup>b</sup>	26	23	66	72	5	3	3	2	18	17	61	68	17	12	4	3
1969	26	22	64	73	7	3	3	2	20	17	58	68	17	12	5	3

<sup>a</sup>These data have been standardized for age using the distributions of the total male and female populations in 1960 as the standard.

<sup>b</sup>Data for these years refer to nonwhites.

Sources: U.S., Census Office, Eleventh Census: 1890, Report on Population of the United States, Part I, Table 82.

\_\_\_\_\_, Twelfth Census of the United States: 1900, Population, Vol. II, Table 29.

U.S., Bureau of the Census, Thirteenth Census of the United States: 1910, Population, Vol. I, p. 517.

\_\_\_\_\_, Fourteenth Census of the United States: 1920, Population, Vol. II, p. 388.

\_\_\_\_\_, Fifteenth Census of the United States: 1930, Population, Vol. II, pp. 843-844.

\_\_\_\_\_, Sixteenth Census of the United States: 1940, Population, Vol. IV, Part 1, Table 6.

\_\_\_\_\_, Census of Population: 1950, P-C1, Table 104.

\_\_\_\_\_, Census of Population: 1960, PC(1)-1D, Table 176.

\_\_\_\_\_, Current Population Reports, Series P-20, No. 198 (March 25, 1970), Table 1.

single among blacks has increased chiefly because fewer blacks are marrying at young ages. In contrast, the proportion never married among whites continued to decline in the 1950s and 1960s (Rele, 1965:219-234). As a result, the proportion single is now greater among Negroes than whites, the reverse of the pre-World War II situation.

Divorce has become more frequently reported as a current marital status among blacks but widowhood has decreased. Most of these changes have occurred since World War II. Identical trends are apparent among

whites. The proportions widowed and divorced are somewhat higher for blacks than for whites at most observation dates.

The net outcome of changes in the proportions single, widowed and divorced has been a near constant proportion among Negroes who are currently married. Among whites, the outcome was different, for there has been a rise in the proportion of both men and women who report they are currently married. Consequently, the proportion currently married is, at present, considerably higher among whites than among blacks. (For a

cohort analysis of these changes see Price, 1969:228-232.)

Not all currently married persons live with their spouse. Some may be living apart because of desertion, others because they anticipate obtaining a divorce or because they work in different places (U.S. Bureau of the Census, 1945:3). Since 1940 tabulations have been provided showing marital status by presence of spouse. Table 2 indicates the proportion of currently married adults who were living with their spouse.

Among blacks, there was a rise during the 1940s in this proportion. However, since 1947, when this percentage apparently peaked, there has been a decline in the proportion of currently married blacks who live with their spouse. In 1969, the percentage of currently married Negro females who lived with their spouse was lower than at any earlier date for which data are available. Among whites, almost all who were currently married—about 96%—lived with their

spouse, and there has been little change in this figure since 1940.<sup>1</sup>

With these data we can also answer the question: what has been the overall change, in the last three decades, in the proportion of adults who are married and live with their spouse? Table 2 presents pertinent data; that is, it shows the age standardized proportion of all adults who were both married and living with their spouse.

Among blacks, the proportion of both men and women who were married with spouse-present rose between 1940 and the mid-1950s, chiefly a result of higher proportions married. Since the mid-1950s, the proportion married-spouse-present declined because a smaller proportion of currently married non-whites live with their spouse and because a

<sup>1</sup> Changes in the proportion of married women living with their husbands do not result from rises in the proportion of men in military service (U.S. Bureau of the Census, 1958: Table 1; 1970a: Table 1).

Table 2. Percentage of Currently Married Adults and of All Adults Who Had a Spouse Present, 1940-1968.<sup>a</sup>

Year	Currently Married Adults				All Adults			
	Men		Women		Men		Women	
	Non-whites	Whites	Non-whites	Whites	Non-whites	Whites	Non-whites	Whites
1940	88%	96%	86%	96%	56%	61%	48%	59%
1947	91	97	87	96	60	66	52	62
1950	86	96	82	96	58	66	50	63
1953	90	97	85	95	62	68	50	64
1957	87	97	83	96	59	69	50	65
1960	85	96	80	95	57	69	49	65
1964 <sup>b</sup>	88	97	78	96	59	70	50	65
1969 <sup>b</sup>	86	97	77	96	56	71	45	65

<sup>a</sup> These data have been standardized for age using the distributions of the total male and female populations in 1960 as the standard. They refer to the population age 15 and over.

<sup>b</sup> Data for these years refer to Negroes.

Sources: U.S. Bureau of the Census, Sixteenth Census of the United States: 1940, Population, Vol. IV, Part 1, Tables 6 and 9.

\_\_\_\_\_, Census of Population: 1950, P-C1, Table 104.

\_\_\_\_\_, Current Population Reports, Series P-20, Numbers 10, 50, 81, 105, 135 and 198.

decreasing proportion of nonwhite women are currently married.

Among whites there has been a rise in the proportion of both men and women who were married-spouse-present, a change which has resulted primarily from an increase in the proportion married.

When figures for 1969 and 1940 are compared, we see no difference in the percentage of Negro men in the marital status associated with family stability; that is, no change in the proportion of married-spouse-present. Among black females, however, there has been a decrease in this proportion, while among whites of both sexes, the proportion married-spouse-present has increased. As a consequence of these trends, the racial differential with regard to this one indicator of family stability is greater now than in the 1940s or 1950s.

#### C. FAMILY TYPES AND FAMILY HEADSHIP

In discussing the breakdown of Negro family life, Moynihan observed that the proportion of families headed by women had been rising. Table 3 shows the distribution of families by type for 1930 to 1969. Families, as defined by the Census Bureau,<sup>2</sup> include two or more people who share the same household and who are related to each other by blood, marriage or adoption (U.S. Bureau of the Census, 1963: xi). Families need not include a married couple, although the majority—87% in 1969—do (U.S. Bureau of the Census, 1970a: Table 2). Families have been categorized into three types based upon information obtained from respondents about family composition and family headship. First, there are families headed by a man whose wife is present; next, there are other families headed by a man; and finally, there are families headed by a woman. If both a man and his wife were present within the household, Census Bureau rules did not permit the designation of wife as family head.

The majority of both nonwhite and white families are headed by a man who lives with his wife. There are, however, substantial racial differences both in the relative number

of families of each type and in recent trends. Among nonwhites, the proportion of families in the modal category (husband-wife) declined from 80% in 1930 to 69% in 1969, while there was a corresponding rise in the proportion of families headed by a woman. As a result, more than 25% of the nonwhite families were headed by females in 1969. Among whites, however, the proportion of husband-wife families rose from 86% in 1930 to 89% in the late 1960s. The proportion of white families headed by a woman was about 10% at each date.

If no additional information were available about families, we might conclude that on this dimension there was a trend away from family stability among blacks. However, we can also determine the percentage of men and women who headed families of different types. A primary family, according to Census Bureau terminology, is one in which the family head is also head of the household. We can ascertain what proportion of ever-married men headed husband-wife primary families—the type of family which corresponds to our definition of a stable family—for a series of dates between 1930 and 1969. Ever-married men who did not head such families either were no longer married at the time of the enumeration or did not head their own households. The data shown below have been standardized for age and adjusted so that consistent definitions of the family were used.<sup>3</sup>

% OF EVER-MARRIED MEN (AGE 15 AND OVER) WHO HEADED HUSBAND-WIFE PRIMARY FAMILIES<sup>4</sup>

Date	Nonwhites	Whites
1930	73%	82%
1940	67	81
1950	66	83
1960	72	87
1969	74	90

Between 1930 and 1940, the proportion of ever-married nonwhite men who headed husband-wife primary families fell from 73% to

<sup>3</sup> In 1930 and 1940, the concept "primary family" was not used. Families, as defined at those dates, closely correspond to primary families as defined after 1947 once the one-person families have been excluded from tabulations.

<sup>4</sup> These data have been standardized for age using the age distribution of total men 15 and over in 1960 as the standard. See Table 3 for sources.

<sup>2</sup> Prior to 1947, a different definition of the family was used. Statistics presented in this paper for years before 1947 refer to families as defined since 1947. Specifically, they exclude one-person families.

Table 3. Types of Families By Color, 1930 to 1969.<sup>a</sup>

Year	Families Headed by Nonwhites			Families Headed by Whites		
	Husband-Wife	Other Male Head	Female Head	Husband-Wife	Other Male Head	Female Head
1930 <sup>b</sup>	80%	5%	15%	86%	5%	9%
1940	77	5	18	86	4	10
1950	77	4	19	88	3	9
1953	79	3	18	88	3	9
1957	75	3	22	88	3	9
1960	75	4	21	89	3	8
1964	73	4	23	89	2	9
1969	69	4	27	89	2	9

<sup>a</sup>Adjustments have been made so that all these figures are based on the definition of the family used by the Bureau of the Census since 1947.

<sup>b</sup>Data for 1930 refer to blacks.

Sources: U.S. Bureau of the Census, Fifteenth Census of the United States: 1930, Population, Vol. VI, Table 20.

\_\_\_\_\_, Sixteenth Census of the United States: 1940, Population, Families--Types of Families, Tables 3, 17, and 19.

\_\_\_\_\_, Census of Population: 1960, PC(2)-4A, Table 5.

\_\_\_\_\_, Current Population Survey, Series P-20, Numbers 33, 53, 83 and T39, Series P-23, No. 29.

67%, a change which may reflect the economic constraints of the Depression. During the following decade there was little change at the national level in the proportion of black men who headed husband-wife families. Since 1950, there has been a rise in this proportion, and by 1969 this percentage was at least as great as at any date since 1930.

Among white men, there was little fluctuation in the proportion who headed husband-wife primary families until 1950. Since that year, the proportion heading families has gone up among white just as it has among nonwhites.

These data appear to present a paradox. An increasing proportion of nonwhite families are headed by women (see Table 3), and this suggests decreasing family stability. A growing proportion of nonwhite men are heading husband-wife primary families, and this may be indicative of greater familial stability. The explanation for this finding lies in trends in family headship by nonwhite

women. Apparently there has been an increase since 1930 in the proportion of women—be they nonwhite or white—who head their own families. Since the mid-1950s, the percentage of nonwhite women who head their own families has risen sharply. The rise in family headship has been much more rapid among nonwhite women than among nonwhite men and, as a consequence, the proportion of nonwhite families headed by women has increased.

It is impossible to put together a lengthy time series concerning family headship by women. We can, however, determine much about recent changes in the family or household status of adult men and women. Table 4 presents this information for 1950 to 1969.

Among nonwhite women there has been a decline in the proportion who live outside regular households or who live as relatives, other than wives, of family heads. There has been an increase in the proportion of women who head their own households whether it



be a household containing no relative, i.e., primary individual, or a household containing relatives, i.e., primary family. These shifts have offset each other and there has been little change in the proportion of non-white women who live as wives of primary family heads. Among white women, there has similarly been a decline in the proportion of women who live outside regular households or as relatives, other than wives, of family heads. However, there has been an increase in the proportion of white women who are wives of primary family heads along with

some increase in the proportion who head their own households.

Among males of both color groups there have been rises in the proportion who head their own households; in particular, the proportion of men heading husband-wife primary families was greater in 1969 than in 1950.

These trends, discussed in this section, do not point unambiguously to greater or lesser stability of black families. They indicate that during the last thirty years a growing proportion of adults have been able to head

Table 4. Persons 25 to 64 Years Old by Household Status and Color, 1950, 1960, and 1969.<sup>a</sup>

Household Status	Nonwhites			Whites		
	1950	1960	1969	1950	1960	1969
<b>Females:</b>						
Heads of Primary Families	13%	16%	22%	6%	6%	7%
Primary Individuals	7	8	9	4	6	7
Wives of Heads of Primary Families	55	58	57	74	78	79
Children of Primary Family Heads	5	5	3	6	4	2
Other Relative of Family Heads	11	8	6	6	4	3
Secondary Individuals	5	3	2	2	1	1
Not in Household <sup>b</sup>	4	2	1	2	1	1
Total	100%	100%	100%	100%	100%	100%
<b>Males:</b>						
Heads of Husband-Wife Primary Families	62%	66%	66%	77%	82%	84%
Heads of Other Primary Families	3	3	3	2	2	2
Primary Individuals	6	7	10	3	4	5
Children of Primary Family Heads	5	6	6	6	5	4
Other Relative of Family Head	9	7	6	5	3	3
Secondary Individuals	7	5	4	2	2	1
Not in Household <sup>b</sup>	8	6	5	5	2	1
Total	100%	100%	100%	100%	100%	100%

<sup>a</sup>These data have been standardized for age using the age distribution of the total population 25 to 64 in 1960 as a standard.

<sup>b</sup>The "not-in-household" population includes persons who lived in group quarters, in institutions and, in 1950, people who lived in quasi-households. The 1969 figures exclude individuals who lived in military barracks on Armed Forces bases within the United States. See sources for further discussion of problems of comparability.

Sources: U.S. Bureau of the Census, Census of Population: 1950, P-E, No. 2D, Tables 1 and 2.

\_\_\_\_\_, Census of Population: 1960, PC(1)-1B, Table 45; PC(2)-4B, Table 2.

\_\_\_\_\_, Current Population Reports, Series P-20, No. 198, Table 2.

their own families and their own households. We can speculate that general improvements in income and welfare (Merriam, 1968:721-804) and the greater availability of housing (U. S. Bureau of the Census, 1969b: Table 1084; 1969c, Tables 365 and 367) have facilitated this change. Among blacks, the family headship rate has increased more rapidly among women than among men, although the proportion of Negro men who head families has risen. This may reflect the fact that the income of black women has gone up more rapidly than that of black men (U.S. Department of Labor, 1967: 13; U.S. Bureau of the Census, 1970b; Tables A-1, A-2 and A-8). Rises in income may simultaneously permit more men to head families and more women to head their own families instead of living with relatives, in-laws, or husbands they deem unsatisfactory.

#### D. ILLEGITIMACY

Illegitimacy influences family stability in a variety of ways. If a woman becomes premaritally pregnant, her chances for a stable marriage may be lessened. If a child is born to an unmarried woman, his chances to grow up in a home with both parents are dimin-

ished (Bowerman *et al.*, 1963; Vincent, 1961: Chap. vii).

Illegitimacy data in the United States are not of high quality. At present only 34 states ascertain legitimacy status (U.S. National Center for Health Statistics, 1969:3-7), a decline from the 45 states which determined this in 1940 (U.S. Bureau of the Census, 1943:10-11). On the basis of data from reporting states, the National Center for Health Statistics annually develops national estimates of legitimate and illegitimate births. Few studies have investigated the accuracy with which legitimacy is reported (see Clague and Ventura, 1968:72-81 and Berkov, 1968:473-498), or whether there are racial or socioeconomic variations in reporting. Therefore, interpretations of changes in illegitimacy must be made cautiously and with awareness of weaknesses in the data. The available figures suggest, however, that illegitimacy has become more common among both nonwhites and whites. The number of nonwhite illegitimate births rose from 60,000 in 1940 to 137,000 in 1957 and then to 189,000 in 1968. Among whites, the increase was from 42,000 in 1940 to 73,000 in 1957 and then more than a doubling to 155,000 in 1968. At each date the number of

Table 5. Number (in Thousands) of Legitimate and Illegitimate Births and Percentage Illegitimate, by Color, 1940-1968.<sup>a</sup>

Date	Nonwhite		White		Percentage Illegitimate	
	Legitimate	Illegitimate	Legitimate	Illegitimate	Non-white	White
1940	300	60	2,157	42	16.8%	1.9%
1943	335	65	2,658	45	16.3	1.7
1947	390	79	3,283	64	16.8	1.9
1950	430	94	3,052	56	18.0	1.8
1953	465	110	3,331	58	19.1	1.7
1957	523	137	3,575	73	20.7	2.0
1960	535	147	3,543	83	21.6	2.3
1964	513	166	3,274	115	24.5	3.4
1968	417	189	2,775	155	31.2	5.3

<sup>a</sup>These estimates include a correction for the underregistration of births.

Sources: U.S. National Center for Health Statistics, Vital Statistics of the United States: 1967, Tables 1-2, 1-19, and 1-24.

\_\_\_\_\_, Monthly Vital Statistics Report, Vol. 18, No. 11, "Advance Report, Final Natality Statistics, 1968" (January 30, 1970), Tables 1 and 10.

illegitimate births was greater among nonwhites than among whites.

The trend in illegitimate births differs markedly from that of legitimate births. Among both color groups, the annual number of legitimate births peaked in the late 1950s and declined since then. Consequently, the proportion of illegitimate births has increased. Among nonwhites the gain has been from 17% illegitimate in 1940 to 31% in 1963; among whites, from 2% to 5% (these proportions are given in Table 5).

#### *Legitimate and Illegitimate Fertility Rates*

The rise in the proportion of births recorded as illegitimate merits further study, for it could result from decreases in childbearing by married women or a rise in childbearing by the unmarried. To investigate these possibilities, we computed age-specific legitimate and illegitimate fertility rates for 1940 to 1967. There is a question of how these rates should be computed. Should illegitimate births be related to *single* women or to all women *who are not currently married* in order to compute an illegitimate birth rate? There is evidence that widowed and divorced women bear some illegitimate children but most illegitimate children are borne by single women (Vincent, 1961:56; Kiser *et al.*, 1968:133). We computed illegitimate birth rates twice, first including only single women in the denominator, and second using all women who were not currently married in the denominator. The latter rates are shown in this paper. That is, the illegitimate birth rates were computed by relating illegitimate births to women who were not currently married, while the legitimate birth rates related legitimate births to currently married women (for other sets of illegitimate birth rates see Clague and Ventura, 1968: Table 2; Kiser *et al.*, 1968: Table 8.4).

Between 1940 and 1960, the legitimate birth rates of both whites and nonwhites rose, but since 1960 these rates have decreased. Pre-1960 increases in legitimate fertility were greater among nonwhites than among whites, but the post-1960 declines have been of about the same magnitude among both color groups. These rates are shown in Table 6.

The fertility rates of unmarried women

have moved in a somewhat different direction. Among both groups, the 1940 to 1960 span was one in which these birth rates rose. Illegitimate fertility rates increased much more rapidly than legitimate birth rates. Since the mid-1960s, nonwhite and white illegitimate birth rates have apparently moved in opposite directions. Among nonwhites, there have been decreases in the rate of illegitimate childbearing,<sup>5</sup> but among whites, these rates have continued their climb. The rate at which unmarried nonwhite women bore children seemingly peaked during the early 1960s and has declined slightly since then.

Despite these changes, rises in the proportion of births illegitimate seem particularly sharp since 1960. This occurrence may be for many reasons. We used the components-of-difference-between-two-proportions methodology (Kitagawa, 1964; Grabill *et al.*, 1958:429-438) to determine demographic reasons for recent changes in this proportion. Four components of change were considered: fluctuations in legitimate birth rates, in illegitimate birth rates, changes in the age distribution of women 15 to 44, and changes in the proportion of women who were married. We determined what proportion of births would have been illegitimate in 1967 if three of the four components had remained at their 1960 level and if one component had assumed its 1967 value. In this manner we estimated the independent effects of each component of change. Results of this analysis are shown below:

	COMPONENTS OF CHANGE IN % OF BIRTHS RECORDED AS ILLEGIT.: 1960-1967	
	Nonwhite	White
% illegit. 1960	21.6%	2.3%
% illegit. 1967	28.7	4.9
Total change	+7.1	+2.6
Change due to:		
Change in legit. birth rate	+5.0	+0.8
Change in illegit. birth rate	-1.1	+0.7
Change in age distrib. of women	+1.2	+0.3
Change in % married	+1.2	+0.2
Interaction of factors	+0.8	+0.6

<sup>5</sup> The nonwhite illegitimate fertility rates computed by relating illegitimate births to single women also show a decline during the 1960s.

Table 6. Estimates of Legitimate and Illegitimate Fertility Rates (per 1,000) by Age and Color, 1940 to 1967.

Age	Legitimate Fertility Rates <sup>a</sup>					Illegitimate Fertility Rates <sup>a</sup>				
	1940 <sup>b</sup>	1950 <sup>b</sup>	1960	1964 <sup>c</sup>	1967 <sup>c</sup>	1940	1950	1960	1964	1967
<b>Nonwhites:</b>										
15-29	392	467	586	597	539	49	70	85	79	87
20-24	232	295	379	323	282	54	104	160	162	133
25-29	128	173	239	205	170	36	89	147	168	123
30-34	90	112	145	124	101	26	62	101	130	98
35-44	48	49	56	47	37	11	21	33	34	30
Est. Gen. Fert. Rate	130	155	186	163	142	40	70	97	97	92
<b>Whites:</b>										
15-29	388	421	473	460	433	4	5	7	8	10
20-24	253	291	350	304	245	6	10	19	21	24
25-29	164	197	221	191	164	4	9	18	24	23
30-34	102	119	121	104	86	3	6	11	15	15
35-44	39	41	40	33	26	1	2	4	5	5
Est. Gen. Fert. Rate	124	144	153	135	118	4	6	10	11	13

<sup>a</sup> Legitimate fertility rates related legitimate births to currently married women. Illegitimate fertility rates related illegitimate births to women who were not currently married, i.e., single, widowed, and divorced women.

<sup>b</sup> Rates for 1940 and 1950 are based on births corrected for underregistration and population corrected for net census undercount. The completeness of birth registration obviated a correction for either underregistration of births or census undercount for dates after 1950.

<sup>c</sup> Births for 1950, 1960, 1964 and 1967 are average number of births occurring annually in a three-year period centered on the year in question. Numbers of women in 1964 and 1967 are average numbers of women in the three-year period centered on the year in question as indicated by the Current Population Reports. Estimates of women for 1940, 1950 and 1960 are from decennial censuses.

Sources: U.S. Bureau of the Census, Census of Population: 1950, P-C1, Table 102.

\_\_\_\_\_, Census of Population: 1960, PC-1D, Table 176.

\_\_\_\_\_, Current Population Reports, Series P-20, Nos. 135, 144, 157, 170, and 187.

U.S. National Office of Vital Statistics, Vital Statistics of the United States: 1940, Vol. II, Table 3.

\_\_\_\_\_, Vital Statistics of the United States: 1950, Vol. I, Tables 6.5 and 6.51.

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Robert D. Grove and Alice M. Hetzel, Vital Statistics Rates in the United States: 1940-1960, U.S. Public Health Service, Publication No. 1677 (1968), Table 28.

Ansley J. Coale, "The Population of the United States in 1950 Classified by Age, Sex, and Color--a Revision of Census Figures," Journal of the American Statistical Association, L (March, 1955), p. 29.

These figures show the contributions of specific causes to change in the proportion illegitimate. For instance, they indicate that if there had been no change from 1960 to 1967 in the age distribution of nonwhite women, if the proportion married remained fixed, if the illegitimate birth rates were constant, and if only the legitimate birth rate had changed, the proportion of nonwhite children recorded as illegitimate would have increased 5 percentage points. This reflects the drop in legitimate fertility rates. If only the proportion of nonwhite women married had changed, the proportion of births illegitimate would have increased 1.2 percentage points.

The figures shown above demonstrate that even if there had been no change in fertility rates, the proportion of births illegitimate would have gone up during the 1960s because of changes in age distribution and changes in the proportion of women married. Between 1960 and 1967, the age composition shifted so that it became more favorable to illegitimate fertility, and the proportion of currently married women, 15 to 44, declined. In addition, among both color groups, there was a substantial decline in legitimate fertility rates, and this had the independent effect of increasing the proportion of illegitimate births. Among nonwhites, changes in the illegitimate birth rate had the effect of reducing the proportion of illegitimate births, but among whites the change in the illegitimate birth rate tended to raise the proportion of illegitimate births.

We conclude that since 1960 there has apparently been a rise in the proportion of illegitimate births. This is chiefly due to changes in legitimate birth rates, the age structure, and marital status. It is not primarily a consequence of more frequent childbearing by unmarried women; in fact, illegitimate fertility rates of nonwhites have fallen.

#### *The Frequency of Premarital Conceptions*

Thus far we have discussed legitimate and illegitimate births. An illegitimate birth is the outcome of a lengthy process involving various decision points. To bear such a child, a woman must become pregnant outside of wedlock and then bring the pregnancy to term. A woman who becomes pregnant might suffer a miscarriage, obtain an abortion, or

marry, and any of these would prevent her from bearing an illegitimate child. If we desire to study trends in illegitimate fertility, we should examine changes in the frequency of these events. We do not have satisfactory time series data for these, but the Census Bureau does provide information from which we can infer the number of premarital conceptions. In 1959 and 1965 the Current Population Survey (U.S. Bureau of the Census, 1961; 1969a) asked national samples of ever-married women the birth dates of each of their children and then later asked the women when they first married. This permits us to estimate the number of women who had a child before, or very shortly after, their first marriage. Again we note that we are uncertain of the quality of these data. Some women may incorrectly report the birth dates of their offspring, or they may report the date of their most recent marriage rather than their first marriage. (Discussions of the quality of data are contained in the sources cited and in U.S. Bureau of the Census, 1966b:xi-xv; 1968:xiii-xiv.)

Table 7 presents data pertaining to premarital births and premarital conceptions. Figures are shown for marriage cohorts—that is, for groups of women who first married in the designated years such as 1940 to 1949. The first row of figures, for each color group, indicates the proportion of women whose reported dates of marriage and birth of first child imply that they bore a child prior to marriage. For example, from 1960 to 1964, 28% of the nonwhite and 4% of the white women who first married had a child before marriage. As the illegitimate birth rates of Table 6 suggest, the proportion of women bearing a child premaritally is greater among nonwhites than among whites. Changes over time indicate there was a rise between the 1940s and 1950s in the proportion of nonwhite women who had a child prior to marriage but there was no further increase in the 1960s. Among whites there apparently has been an increase in the proportion of women who bear a child before marriage.

The second rows of the panels in Table 7 show the proportion of women who reported their first child was born within seven months of the date of their first marriage. Some of this childbearing occurred before

Table 7. Cumulative Percentage of Women Who First Married in Specified Years Who Bore a First Child before Marriage or Shortly after Marriage.

Months Married	First Marriage Cohorts						
	1900- 1909	1910- 1919	1920- 1929	1930- 1939	1940- 1949	1950- 1959	1960- 1964
<b>Nonwhites:</b>							
Before Marriage	..	..	..	..	18%	28%	28%
By 7th Month after Marriage	10%	19%	19%	24%	32	46	48
By 12th Month after Marriage	29	36	34	39	46	61	61
<b>Whites:</b>							
Before Marriage	..	..	..	..	2%	3%	4%
By 7th Month after Marriage	6%	7%	6%	7%	8	13	17
By 12th Month after Marriage	30	31	27	23	28	37	42

Sources: U.S. Bureau of the Census, Current Population Reports, Series P-20, No. 108, Tables 16 and 17; Series P-20, No. 186, Tables 17 and 18.

marriage and some after, but for women who married before 1940 we cannot distinguish when these first births occurred. Figures in these rows point to a trend toward more childbearing either before or shortly after marriage. Among nonwhites, the proportion having a child within seven months of marriage rose from 10% among women who first married in 1900 to 1909, to 48% among those married in the 1960s. These figures imply that a growing proportion of women—both nonwhite and white—became premaritally pregnant.

We must be careful not to attribute these trends to a single cause. The frequency of premarital pregnancy is a function of the incidence of premarital sex, the age groups involved, the use of birth control or abortion and health conditions (such as the prevalence of venereal diseases). Changes in any or all of these factors may account for the observed trends (Grabill, *et al.*, 1958: 217; Kiser *et al.*, 1968:145; Farley, 1970: 206-245).

The last row of figures in each color group in Table 7 shows the proportion of women who had a child within twelve months of their first marriage. These demonstrate

that an increasing proportion of couples have a child within a year after marriage.

#### *Premarital Conceptions and Rapid Marriage*

Statistics from the recent Current Population Survey were used with vital registration data to estimate what proportion of women who became premaritally pregnant, in a given period, married shortly after they became pregnant. The vital statistics system indicates the number of illegitimate births occurring in each period. A recent study estimated that 90% of the illegitimate births were borne by single women (Kiser *et al.*, 1968:133). Thus we estimated that 90% of the illegitimate births of each period were premaritally conceived. The Current Population Survey of March, 1965 showed the number of women who first married and then bore a child within seven months of marriage. This gave us an estimate of the number of births which were premaritally conceived but borne to married women. Using these data, we estimated the proportion of premarital pregnancies which led to marriage within seven months. These are rough indicators of trends. We could not ascertain

the incidence of abortion, miscarriage, or misreporting of dates. We have, however, adjusted for the underregistration of births and for mortality occurring to women between the time of their marriage and the survey date. The estimates are shown below:

ESTIMATES OF % PRE-  
MARITAL PREGNANCIES  
RESULTING IN RAPID  
MARRIAGE: 1940 TO 1964

Dates	Nonwhites	Whites
1940-49	23%	55%
1950-59	19	63
1960-64	18	60

These figures demonstrate one important reason why nonwhite illegitimacy rates exceed those of whites: premarital pregnancies are less likely to lead to marriage among nonwhites. In 1960-64, apparently fewer than one in five nonwhite women who became premaritally pregnant married within seven months but three in five white women married. This implies that racial differences in premarital conception rates are smaller than racial differences in illegitimate birth rates.

These figures also suggest why the proportion of illegitimate births increased more rapidly among nonwhites than among whites during the 1950s. The likelihood that a premarital conception would lead to marriage decreased among nonwhites but rose among whites between the 1940s and the following decade. Since the 1950s, however, there appears to be little change in the proportion of women who marry shortly after they became pregnant.

### Summary

The data imply that rates of childbearing by unmarried women are higher now than they were 30 years ago. These rates have generally risen throughout this period although in recent years the illegitimate fertility rate among nonwhite women has declined. Higher illegitimate birth rates mean that a larger proportion of children are born to parents who are not married and that a growing proportion of young women face the burdens of pregnancy and early childrearing without the assistance of a husband. In addition,

data concerning the timing of conceptions suggest that a growing proportion of women become pregnant before they marry. Changes on these indicators suggest a trend away from family stability. The directions of the changes appear to be the same among both races. (For a discussion of factors producing changes in illegitimacy rates see Vincent, 1961; Kiser *et al.*, 1968:144-146; Kumar, 1969:92-108.)

### E. CHANGES IN THE LIVING ARRANGEMENTS OF YOUNG CHILDREN

Whether children live with both of their parents may depend upon their own legitimacy status as well as the stability of their parents' marriage and family. This indicator is of interest since growing up with both parents may influence the child's chances for educational attainment and occupational achievement.

Demographic data of two types describe change in the proportion of children who live with one or both parents. First, figures are available from the Census of 1910 and from censuses since 1940 showing the proportion of children under five years of age who lived in households with their mothers. Trends in these proportions are shown below:

% CHILDREN UNDER FIVE WHO LIVED  
WITH THEIR MOTHERS

Date	Nonwhites	Whites
1910	86.9%	96.5%
1940	85.2	96.7
1950	85.6	98.2
1960	86.2	99.1

These figures reveal that a high proportion of young children of both races live with their mothers. There appears to be no change from 1910 to 1960 in the proportion of nonwhite children who had their mother present, but among whites there was a modest rise. At each date, the proportion with their mothers was higher among whites than among nonwhites.

The Census of 1960 and the Current Population Surveys provide the second source of information. They show detailed information about the living arrangements of persons. In particular, they indicate the proportion of children under age 18 living in

households with both parents. Table 8 gives data for two years, 1960 and 1968.

We observe from this table that the proportion of children who lived with both parents was greater among children 0 to 5 than among children 14 to 17. As children grew older, their likelihood of living with both parents decreased.

There are substantial racial differences in the living arrangements of children. In 1968, 60% of the nonwhite children lived with both parents but among whites, 90% of the children lived with both parents. The proportion of youngsters who lived with only their mother was about four times as large among nonwhites as among whites. In that year, 10% of the nonwhite children and 2% of the white children lived in households with neither parent.

Changes in the living arrangements of young nonwhite children do not point unambiguously to a trend toward or away from family stability. On the one hand, fewer

nonwhite children live apart from both parents. This proportion dropped from 12% in 1960 to 10% in 1968 and may be indicative of greater family stability. On the other hand, fewer nonwhite children are living with both parents—a decline from 66% to 60%—and an increasing proportion are living with their mother only. This may result from the rise in the proportion of illegitimate births.

Rather similar changes are evident in the living arrangements of white youngsters. The proportion living with neither parent has fallen and the proportion with only their mother has increased. However, there has been no change in the proportion of white children who live with both parents.

#### F. FINDINGS

This examination of demographic indices leads to specific conclusions about family composition and changes in family stability.

Table 8. Children by Presence of Parents, Whites and Nonwhites, 1960 and 1968.

Age by Color	Percentage of Children in 1960 Living with:				Percentage of Children in 1968 Living with:			
	Both Parents	Mother Only	Father Only	Neither Parent	Both Parents	Mother Only	Father Only	Neither Parent
Nonwhites:								
Under 6	69%	19%	1%	11%	61%	25%	1%	13%
6 to 9	68	20	2	10	61	30	2	7
10 to 13	66	21	2	11	61	28	2	9
14 to 17	59	21	3	17	58	28	3	11
Total <sup>a</sup>	66	20	2	12	60	28	2	10
Whites:								
Under 6	93%	5%	1%	1%	92%	6%	..	2%
6 to 9	92	5	1	2	91	7	1%	1
10 to 13	90	7	1	2	89	8	1	2
14 to 17	82	9	2	7	85	10	2	3
Total <sup>a</sup>	90	6	1	3	90	7	1	2

<sup>a</sup>The total figures have been standardized for age using the age distribution of the total population under 18 in 1960 as the standard.

Sources: U.S. Bureau of the Census, Census of Population: 1960, PC(2)-4B, Tables 1, 2, and 19.

\_\_\_\_\_, Current Population Reports, Series P-20, No. 187, Table 4.



First, among blacks and whites the majority of both adults and children are in the statuses indicative of family stability. Most adults are currently married and most of them live with their spouses. Most families are husband-wife families. The majority of children are legitimate and live with both parents. The focus upon family disruption in the black community sometimes suggests that the majority of black families are broken. The tables in this paper refute such notions.

Second, there are racial differences in each of the indicators and, in every case, a higher proportion of whites than Negroes are in the status indicative of family stability.

Third, it is impossible to draw unequivocal conclusions about overall trends in family stability among either race. We can, however, describe changes in particular dimensions of family stability. Among whites, the proportion of adults who were married and lived with a spouse has gradually increased but among Negroes this has not happened. The proportion of men of both races who headed their own families has gone up, but only among blacks has there been a pronounced rise in the proportion of women who head families. Among both races the percentage of births recorded as illegitimate has risen, but only among blacks has there been a decline in the proportion of children who live with both parents.

#### G. THE RELATION OF FAMILY INSTABILITY TO LIFE CHANCES

One implication of many discussions concerning Negro family instability is that this is a substantial factor accounting for much of the Negro-white differential in life chances, by which we refer to differential levels of education, occupation, and income. There has also been recognition that racial differentials in family stability are themselves a result of differential access to life chances, particularly the inability of black men to obtain sufficiently remunerative employment (Moynihan, 1965:745-770). Effective public programs to reduce the impairments in Negro family structure and thereby to improve life chances for Negroes would require detailed knowledge of (1) the nature and degree of difference between blacks and whites in family stability, and (2) the interrelations between family stability and life chances. The

major focus of this paper has been to assemble and examine data bearing on the first point. In this section we review, much less exhaustively, a number of points bearing on the second issue.

Analysis of the relation between family stability and life chances must be examined separately for children and adults since both may be residing in other than stable families. This distinction also introduces the possibility of studying whether the family stability status of a child affects his stability as an adult. Attention must also be given to ascertaining the causal or temporal ordering of the stability and life chance variables.

In an earlier section we considered adult marital status and living arrangements as indicators of family stability. At this point, we may ask if there is any association between marital status and life chances, keeping in mind that the temporal order of the relevant variables is difficult to establish from available data. For many men schooling is completed before marriage, but for a significant number this is not the case, and for many of these the advent of marriage may affect the educational level attained. In the case of occupation, it is even more clear that this is a factor which can both affect marital status and, in turn, be affected by marital status.

In Blau and Duncan's (1967:337-340) study it was found that men who were married with spouses present had somewhat higher occupational status than single, divorced or married men with spouses absent. This was true even after taking into account family background, education, and the occupational status of the respondent's first job. The differences, however, were modest, indicating that "the net effect of marital status is by no means pronounced" (Blau and Duncan, 1967:340. For further analysis of these data see Duncan, *et al.*, 1968:251-255).

The sample used by Blau and Duncan was not sufficient to allow a separate analysis of whites and blacks. To see if the general conclusions stated above hold equally for each race, we utilized the data from the Census of 1960, examining the marital status differentials in education, occupation and income (U.S. Bureau of the Census, 1966a: Tables 4, 5, and 6). Men of both races who

were married-spouse-present or who were single tended to have greater educational attainment than men in other marital statuses. Individuals who were married but did not live with their wives had the lowest educational attainment among both races. There were very small marital status differences in occupational prestige, though men who were married-spouse-present had the highest levels of prestige as measured by the Duncan socioeconomic index. Marital status differences in personal income—not family income—were more sizable, and men of both races who were married-spouse-present had larger average incomes than men who were single, married-spouse-absent, widowed or divorced. (For additional discussion see Carter and Glick, 1970:169–221.)

The general impression from these data is that for both whites and Negroes, currently married men have higher scores on these indicators of socioeconomic status than do men in other marital status. There were, however, exceptions, and with regard to some indicators of status single or divorced men outranked the currently married. Thus the census data corroborate the conclusions of Blau and Duncan. They reveal quite clearly that differences among marital status categories are much smaller than differences between races.

Much of the concern about Negro family instability centers on the effect such an environment may have upon children. Moynihan reviewed a number of studies which indicate lower intelligence score performance and greater delinquency among black children who were not living with both parents (U.S. Department of Labor, 1965).

Insight into the relation of family stability to education and occupational status may be gained from the data utilized by Blau and Duncan. Negro adult males from intact homes (defined as living with both parents most of the time up to the age of 16) had 1.6 more grades of schooling than black males who grew up in a family headed by a woman; non-Negro males from intact homes had 0.9 more grades of schooling (Duncan and Duncan, 1969:277). A multivariate analysis which regressed education on number of siblings, family type, education of family head, and occupation of family head indicated that an intact family was associated

with from .4 to .8 years of schooling for various birth cohorts of nonwhites and from .5 to 1.5 years of schooling for white birth cohorts (B. Duncan, 1967:366).

Turning to occupation, growing up in an intact home was associated with an advantage of 4 to 5 points on Duncan's socioeconomic index for both Negro and non-Negro males (Duncan and Duncan, 1969:277). Males from intact homes appeared to translate more effectively their educational attainment into occupational achievement than males from a family headed by a female (Duncan and Duncan, 1969:282). For black males, there was also an advantageous gain from first occupation to current occupational level for those from an intact home (Duncan and Duncan, 1969:282).

An important by-product of the foregoing research is the finding that there is little difference in marital status distributions for adult males according to the stability of their family of origin. It appears that children from a broken home are no more likely than others, as adults, to be in family arrangements characterized in this paper as unstable (Duncan and Duncan, 1969:275).

This review of the relation of family stability to such life chance variables as educational and occupational level is admittedly sketchy and conclusions must be drawn cautiously. The evidence suggests that family instability is associated with somewhat lower levels of the life chance variables examined. At the same time, it appears that in nearly all cases the differentials are modest; often the effect of the instability factor is much less important than other factors examined in the works cited, and by itself the family stability variable does not go far in accounting for differences between the races. One implication of this is that programs designed to strengthen black family structure in the hope of thereby improving the socioeconomic status of blacks may be less effective than alternate strategies. At any rate, the potential gains from different approaches must be carefully appraised if scarce resources are to be used to maximum advantage.

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# SOME SOCIAL IMPLICATIONS OF HIGH DENSITY HOUSING \*

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*American Sociological Review* 1971, Vol. 36 (February):18-29

*The existing literature on the social, personal, and health consequences of high density housing is unclear regarding the significance that can be attached to the physical features of housing. The present study, which is based on interview information collected in Hong Kong, is able to control for deprivations and stresses related to "poor housing," and it is also able to distinguish superficial from more severe measures of personal strain. High densities are seen to have very little effect on individuals and families, although there is a suggestion that congestion is a potentially significant stress.*

SINCE man occupies space, it is reasonable to assume that much of human behavior is affected by and oriented to spatial features of the physical environment. Ecologists and regional scientists have studied the macro-features of space for entire communities and regions, but very little research attention has been given to the patterning effects of space in small areas, especially within the household. The present paper discusses the effects that high density and other features of the dwelling unit (the micro-environment) have for the emotional health and family role relations of people living under widely divergent housing conditions in urban Hong Kong, the city that has probably the highest residential densities ever known in the world.

Although there is a social science literature on housing and habitation (Van Den Broek, 1964), very little attention has been given to the design features of housing. As Sommers (1969) noted, even studies of small groups typically fail to note that the spatial dimensions of experiments are potentially significant variables that must be systematically considered in the design of experiments.

\* This paper is based on research conducted for the Urban Family Life Survey, a joint project of the Department of Social Welfare and the Council of Social Services, both of Hong Kong. Acknowledgment is also given for the assistance provided by the Program Area Committee on Housing and Health of the American Public Health Association, the Environmental Control Administration of the United States Department of Health, Education, and Welfare; the Institute for International Studies and the Survey Research Center of the University of California, Berkeley; The Chinese University of Hong Kong; and my fellow participants in the United Nations Ad Hoc Meeting of Experts on the Social Aspects of Housing in Urban Areas.

The physical features of housing have special significance to many fields in sociology, especially to family sociologists. Much of the family's life as a family is spent within the family's dwelling unit. Therefore, in studying the requirements for "adequate" family and personal functioning, it might be asked how housing affects families and their members. What objectives is housing supposed to accomplish? What features of the household as a physical unit best help to accomplish these objectives?

## PRIOR STUDIES

There have been very few well designed studies concerning the personal and family effects attributable to housing in general and the physical features of housing in particular, especially densities. For example, Loring (1956, 1964, 1967) has suggested that "over density" is the only housing and neighborhood feature that is associated with his measures of social disorganization. In an ecological analysis of elderly patients in mental hospitals, Gruenberg (1954) found that center-city high density areas account for a disproportionate number of first admissions for psychoses. One French study argued that the habitable space per person in a dwelling unit has a strong effect on levels of tension among the members of the family in the dwelling unit (P. and M. Chombard de Lauwe, 1959). According to Madge (1968), "the current expert consensus in European countries is that the lower limit of mental health is 170 square feet of floor space per person," although the American Public Health Association (1950) set the desirable standard at twice this figure in 1950.

Several studies have shown the significance of space and density for the desire to move (Lansing, 1966; Lansing *et al.*, 1969), although neither this desire nor its frustration has been shown to adversely affect family functioning and emotional health. Studies of language behavior are also suggestive in this regard, for Bernstein (1968) has suggested that levels of congestion within the household affect children's language behavior and their perceptions of their environment. In so far as language behavior and these perceptions reflect and affect communications channels and communication disorders, congestion could possibly have an indirect effect on emotional health and family functioning. Other indirect evidence regarding the potentially negative effects of high densities is suggested by Calhoun (1962a, 1962b, 1963) in his experimental studies of rats, as well as by other studies of high density animal populations (cf. Hall, 1966:15-22). The research studies of Hall (1966) and Sommers (1969) regarding proximics and personal space are also highly suggestive of the potential significance of density.

To date, most of the negative findings pertaining to the family and personal significance of high density have been produced by public health researchers, especially those who are concerned with infectious diseases. These diseases involve host-parasite relationships, and it is reasonable to assume that these relationships will most likely develop in small undivided rooms housing large numbers of people. However, there have been a number of studies that have questioned the independent significance of housing and density for the development of infectious diseases. Brownlee (de Groot *et al.*, 1970), for example, has noted that there is a relationship between density and mortality but that the strength of this relationship has been declining over the years. Similarly, Martin (1957; n.d.) has suggested that direct attacks on either partner to the host-parasite relationship is more likely than giving attention to densities to affect rates of infectious diseases.

A number of studies have also indicated that high density is associated with many other deprivations and stresses (Mitchell, 1969a, 1969b), and that both infectious and noninfectious diseases tend to cluster to-

gether. Various diseases and maladies, including psychoneurosis, were found to be clustered together in Baltimore rather than to be evenly distributed throughout the population (Downs and Simon, 1954), and similar findings have been found elsewhere—for example, in St. Paul (Buell, 1952; Lemkau, 1970). Schmitt's (1966) ecological analysis of census tracts in Honolulu found that overcrowding had no impact on various measures of morbidity and illness, once education and income were statistically controlled. Similarly, Guerrin and Borgatta (1965) found that illiteracy is the best predictor of morbidity.

Other studies concerned more directly with personal and family functioning have argued against the independent significance that might be attached to high density and poor housing. Wilmer *et al.*'s (1962) multiwave panel study of new residents in a Baltimore public housing project raises serious questions about the benefits of improved housing, although this study did not touch directly on densities and physical design. Foote *et al.* (1960) found that people are willing to trade off less space in order to add or maintain mechanical equipment in the household, and the rapid growth of the mobile home industry suggests that people are willing to sacrifice space for apparent savings in housing costs.

These ambiguous findings regarding the personal, family, and social significance of housing have begun to arouse the concern of public health personnel and urban planners. Even back in 1943, one expert (Anderson, 1943) noted that "of the many newer aspects of environmental sanitation, the standards of housing seem to rest on an especially insecure epidemiological foundation." Both the World Health Organization (WHO) and the American Public Health Association (APHA) have encouraged research on the health implications of high density housing (WHO, 1964a, 1964b, 1966, 1968). Health is broadly defined by WHO's constitution in 1946 to be "a state of complete physical, mental and social well-being and not merely the absence of disease or infirmity." Health includes the adequate functioning of families, however "adequacy" is defined.

These more general social concerns were

covered in a number of position papers recently prepared for the APHA. In one, Lemkau (1970) reported that his review of the existing research indicates that "estimates of the power of emotional factors in influencing the healthfulness of housing are so rarely based on sound data, or any data at all, that the relationship of them to health is possible only in the most extreme conditions." Wilner and Baer (1970) arrive at a similar conclusion: "There is no body of convincing evidence that crowding in a dwelling unit contributes materially to mental disorder or to emotional instability. Nor is there evidence as yet that crowding (or other housing deficits) interferes with a promotive style of life; that because of crowding, family roles and rituals cannot satisfactorily be carried out; or that the development of infants and children is severely impaired." Similar conclusions were reached by de Groot *et al.* (1970) as well as by Mitchell (1970a).<sup>1</sup>

Various suggestions have been given regarding possible reasons why density does not have its anticipated effects. In comparing rat and human responses to density, Mitchell (1971a) has distinguished density from congestion (or intensity). Congestion refers to the simultaneous demands for the use of very limited resources. Wilner and Baer (1970) make a similar distinction involving measures of space, people, and time. Loring (1967) has suggested "that a lot of health problems stem not from mere physical crowding, but from activity overcrowding, role over-density, and possible subsequent withdrawal into psychosocial isolation."

Research to date suggests several lines of development for future studies. (1) More attention should be given to clarifying the meaning of density and the physical features of housing in order to specify what stimuli in the housing situation are noxious and stressful. (2) Past studies may not have given adequate attention to the effects of density because there was not a sufficiently wide variation in the density situations considered. Extremely high densities, such as those existing in Hong Kong, must be considered. (3) Since high densities and poor housing tend to be highly associated with

other deprivations and stresses, these potentially confounding influences must be statistically controlled before the independent effects of housing can be ascertained. And (4) better measures of deleterious individual and family effects potentially arising from housing must be used. In the present context, measures pertaining to noninfectious diseases are needed, and these measures should distinguish superficial from deeper, more impairing consequences.

The present research speaks to all these issues, especially (2), (3), and (4). Consideration is given to effects that various housing features have for attitudes toward one's housing, for levels of emotional strain felt by residents, and for effects upon several kinds of family and nonfamily relationships.

#### SOURCES OF DATA

The information referred to here is drawn from three large-scale sample surveys conducted in Hong Kong, as well as the pre-listing operations conducted in preparation for two of these samples.<sup>2</sup> One of these studies was based on a sample consisting of 3,966 people from the urban areas of the Colony. Individuals 18 years of age and over were the ultimate sampling units in this multistage sampling plan. In contrast, the second study was concerned with families in the same urban areas. Of these families, 561 husband-wife pairs were interviewed, in addition to 2,631 other people who were married and living with their spouse, but whose spouse was not interviewed. University students conducted these interviews during the summer and early fall of 1967.

A split-interview schedule was used in both of these studies, although two-thirds of the questions were asked of both halves of the sample. Some of the information that is reported here refers to only one-half of each sample.

The third study consists of a 10% sample of all Forms 3 and 5 pupils in the Colony. Self-administered questionnaires were used to obtain information for this project.

<sup>1</sup> These papers were prepared for the First International Conference on Health Research in Housing and Its Environment.

<sup>2</sup> Information on housing also was collected by the author in large-scale sample surveys conducted in Bangkok, Taipei, Singapore, and the six major cities in Western Malaysia. For comparative information on housing situations in these different countries, see Mitchell (1969b, 1970).

## MAJOR FINDINGS

Hong Kong has extremely high ground densities and densities within the individual dwelling units.<sup>3</sup> Our concern here is only with the latter. The median size dwelling unit in the urbanized areas of the colony has 400 square feet, and the median square feet per person is 43. Thirty-nine percent of the Hong Kong respondents report that they share their dwelling unit with nonkinsmen; 28% sleep three or more to a bed; 13% sleep four or more to a bed. As noted elsewhere, high density dwelling units could be labeled as "poor housing," for they are the most likely not to have tap water, flush toilets, and cross ventilation. They are also most likely to have only one room per unit, to have ten or more people in the unit, and to have two or more unrelated families sharing the same unit (Mitchell, 1969b, 1970). Housing deprivations tend to accumulate, and, furthermore, the poorest people in the Colony suffer the most from these housing deprivations, as well as from a number of other deprivations.

## ATTITUDES TOWARD HOUSING

As noted earlier, American studies have shown that people are aware of and respond to a perceived lack of space (Lansing, 1966; Lansing *et al.*, 1969; Foote *et al.*, 1960; Rossi, 1955), although no satisfactory connection has been made between these attitudes and other more disturbing features of personal and family functioning. People in Hong Kong are similarly responsive to a lack of space.

Six different attitudinal items relating to housing were measured, but only two will be mentioned here. Both were asked only of the sample of married people.

First, to tap the dissatisfaction about the amount of space people have, the married people were asked: "How do you feel about the amount of space you have? Twenty-nine percent responded that they have "somewhat less space" than they need, and another 13%

said they have "much less space" than they need.

Second, three separate questions were asked to measure feelings about a lack of privacy.<sup>4</sup> Since those who were bothered in one of these three ways were also bothered in the other ways,<sup>5</sup> the three items were combined to form a single index.<sup>6</sup>

Complaints about a lack of space and a lack of privacy rise in response to high densities. Although density itself, not the number of people or the number of rooms in a dwelling unit, affects the level of these complaints, the complaint about space is also strongly influenced by the social composition of the dwelling unit; those who share their dwelling unit with another household, regardless of the dwelling unit's density, are most likely to make this complaint (Table 1). It is the number of households, not the number of people in the dwelling unit, that is significant, a point that will be discussed again later.

## EMOTIONAL STRAIN

A large number of indicators of positive emotional health were developed for this study. (The study was concerned generally with the sources of emotional strain created by the processes of urbanization and industrialization in Southeast Asian cities.) Three general types of emotional strain were devel-

<sup>4</sup> These questions were preceded by the general query: "Now I would like to know how often you are troubled by the following: Is it many times, sometimes, hardly ever, or never?" In answering the question: "Do you have to hide or avoid other people whenever you want to do something in private," 17% said "sometimes" or "many times"; 16% gave the same answer in response to the question: "Do you feel that you cannot discuss anything with your spouse without others listening?"; and 13% gave one of these two responses to the question: "Do you feel that you cannot avoid seeing certain persons no matter how hard you try because you live so close to them."

<sup>5</sup> For example, 68% of those bothered many times or sometimes about not being able to discuss things privately with their spouse also complained to the same extent about not being able to avoid certain persons, no matter how hard they try. In contrast, among those who were never bothered by a lack of privacy with regard to their spouse, only 5% were bothered by not being able to avoid those who lived close to them.

<sup>6</sup> A score of two is assigned to the answer categories "many times" and "sometimes"; a score of one is assigned to "hardly ever"; and a score of zero is assigned to "never."

<sup>3</sup> Urban net residential densities vary from 415 to 466 people per acre. This density is equivalent to between 265,000 and 298,000 people per square mile. According to information provided by the Colonial Outline Planning Team in 1969, densities in the Shek Kip Mei Resettlement Estates were 4,100 per acre (Mitchell 1970).

Table 1. Percentage of Respondents with Less Than Needed Space.<sup>a</sup>

Sq. Ft. per Person	Not Sharing Unit with Another Household	Sharing Unit with One or More House- holds
16 or under	67 (127)	86 (66)
17 to 27	50 (157)	75 (79)
28 to 42	35 (98)	52 (117)
43 to 66	26 (94)	45 (106)
67 and over	17 (157)	40 (95)

<sup>a</sup>Figures based on family sample.

oped. *First*, there are two indicators of somewhat superficial levels of strain: happiness and worry. Happiness was measured by the question: "In general, which of the following statements do you think best describes how happy you are these days?" Worry was measured by the question: "Some people worry about things a great deal, whereas some worry a little. How much would you say you worry?" Four answer-categories were listed for each question. *Second*, two separate and more severe levels of strain were measured by obtaining information about standard psychosomatic symptoms. Eleven symptoms were used in an index of emotional illness,<sup>7</sup> and six symptoms were used in an index of hostility.<sup>8</sup> These indices are explored in con-

<sup>7</sup> The eleven symptoms used were: You feel shortness of breath when you are not exercising or working hard; you feel your heart beating hard; you feel spells of dizziness; you have nightmares; you tend to lose weight when you have something important bothering you; your hands sweat so that you feel damp and clammy; some days, even for several weeks or several months, you cannot take care of things because you cannot "get going"; you have trouble getting to sleep or staying asleep; you are troubled by headaches; you are bothered by feeling nervous, fidgety, and tense; you lose your appetite.

In constructing the index, a score of 1 was assigned to each symptom that occurred "many times" or "sometimes." Other definite answers were scored zero. Those not providing information were excluded from the index. People with a score of 4 or more were considered to score high on emotional strain.

<sup>8</sup> The six symptoms were: Some people make you dislike them so that whatever they want you to do, you just want to do the opposite; your

siderable detail in the forthcoming publications of the Urban Family Life Survey. *Third*, two separate indices of role or behavioral impairment and withdrawal were developed. One refers to withdrawal from family roles, and the other refers to withdrawal from work roles. Since densities and other physical features of housing do not affect these measures of behavioral impairment, these two indices will not be considered in the present paper.

Both measures of superficial strain respond to high density. However, since low-income and lowly educated people have the highest levels of unhappiness and worry, and since these poor people are most likely to live in high-density dwelling units, these confounding influences must be statistically controlled. When this is done for the general population of individuals, densities affect these superficial strains for only people in low-income families (Table 2).

Given the conditional significance of density for these superficial levels of strain, it is not surprising that the more severe levels of emotional strain do not respond to high densities.

Although density does not have an independent effect on severe forms of strain or, as noted earlier, on role impairment, it is possible that some individual components entering into the definition of density have a potentially significant influence on emotional strain. To test this possibility, separate tabulations were run involving the number of square feet in the dwelling unit, the number of people living in it, the number of square feet per person, and the number of people per bed. All of these were tabulated against the several forms of emotional strain introduced earlier. Partial correlations also were prepared, but these indicate that these individual components are insignificant. When considered together in a multiple correlation, they account for less than 5% of the varia-

stomach is full of fire, but you don't let others know about it; you are bothered by easily losing your temper; you feel that if you don't try your best to control yourself, you may say or do something harmful to others; you feel that you are going to explode; you find some people so unreasonable that it is hard to talk to them.

The same weighting scheme is used for this index as for the index of emotional strain. Those who score 2 or more on the index are considered to be high on hostility.



Table 2. Percentage of Respondents Worrying a Great Deal or Somewhat, by Family Income.<sup>a</sup>

Sq. Ft. per Person	\$1 to \$359 (1st Quartile)	\$360 to \$509 <sup>b</sup> (2nd Quartile)	\$510 to \$849 (3rd Quartile)	\$850 and Over (4th Quartile)
22 or under	79 (121)	65 (139)	56 (73)	36 (25)
23 to 34	69 (103)	57 (101)	55 (85)	38 (29)
35 to 50	58 (69)	58 (96)	57 (102)	37 (43)
51 to 99	60 (53)	48 (86)	41 (88)	43 (111)
100 and over	42 (79)	50 (48)	49 (49)	39 (150)

<sup>a</sup>Figures are based on one half of sample.

<sup>b</sup>Figures are in Hong Kong dollars. At the time, the approximate exchange rate was HK \$5.714 = US \$1.

tion in any of the strain variables in Hong Kong.

Density and crowding are sometimes measured by the number of people per room rather than by feet per person (UN, 1962). This definition suggests a concern for a lack of privacy, and for the multiple if not competing uses of the same limited space. However, the number of rooms in a dwelling unit, even when considered together with the earlier density measures, has no impact on levels of emotional strain.

In addition to these multiple and partial correlations, tabulations were prepared to ascertain whether densities affect levels of strain among occupants of particular kinds of housing. But again the results are negative. Only the superficial indicators of unhappiness and general worry respond to densities, and, as seen above, these differences are considerably reduced when family income is considered together with densities.

High densities can be architecturally arranged in several different ways. In this regard, the number of rooms in a dwelling unit was considered together with densities, but no impact on levels of emotional strain was found in Hong Kong.

In addition to the amount and organization of space, the impact of housing amenities on levels of strain was determined. To show this, an index of amenities was created. A score of one was assigned to the presence of each of the following: tap water, flush toilet, and cross ventilation. This index was then used together with densities, number of rooms, and family income to determine the

influence of each upon the superficial measures of personal happiness, the deeper measures of emotional illness, and the serious measures of withdrawal from family and work roles. Again, housing conditions seem to be irrelevant.

Therefore, we have seen that densities, the internal partitioning of a dwelling unit, and the amenities available in the dwelling unit all fail to influence levels of emotional strain. All of these features refer primarily to the physical characteristics of housing.

#### SOCIAL FEATURES OF HOUSING

In contrast to the physical features of housing, some of the social features seem to adversely affect individuals, but these several features are significant only when they are found in certain physical surroundings. These surroundings determine whether potentially destructive social environments will in fact create stresses that result in high levels of strain.

Two characteristics of the social environment are especially significant: the number of households<sup>9</sup> sharing the same dwelling unit and, related to this, the number of people in that unit. Only one feature of the

<sup>9</sup> A household may include nonrelatives, such as a servant; a family is limited to kinsmen only, but our concern in this study is with family relations, not families as groups; finally, a dwelling unit may contain several households or families. Although families and households tend to be interchangeable units, there are some exceptions, as described in Mitchell (1969a).

physical housing environment is significant: the floor level of the dwelling unit.

These housing features are singled out for special consideration because of their relevance to sustained social relations or social densities. It is assumed that sustained social relations among nonrelatives are more likely to produce interpersonal conflict and stress than are sustained social relationships among relatives. We rely on information concerning the number of households in the dwelling unit in order to distinguish intradwelling-unit social relations that are limited to kinsmen from those involving kinsmen and non-kinsmen living together.

Floor levels are significant in Hong Kong simply because it is more difficult for residents in the upper stories of multistory buildings to move about and to get away from their homes. Consequently, they are forced into close social relationships with the other members of their dwelling unit.

If close, forced social relationships are a potential source of stress, then stress should be higher when there are more people in the dwelling unit, when these people include non-family members, and when they live in upper-floor areas.<sup>10</sup>

These assumptions are supported by the Hong Kong data. First, when the number of households occupying a single dwelling unit is statistically controlled, floor level is seen to affect emotional illness for multiple family dwelling units only. Forced interaction apparently does not affect levels of emotional illness if this interaction is with one's own kinsmen, but it does if this interaction is with nonkinsmen. The number of families sharing a dwelling unit, however, does not increase the levels of emotional illness for

Table 3. Percentage of Respondents Scoring High on Index of Emotional Illness.<sup>a</sup>

Floor	One Household in Unit	Two or More Households in Unit
Ground	35 (140)	35 (69)
1st-5th	38 (597)	40 (488)
6th or higher	35 (284)	45 (99)

<sup>a</sup>Figures based on one half of sample.

those who live on the ground floor, perhaps because these people are able to move out into the street for additional social space. Only when the individual's movement is restricted is he affected by the presence of other families (Table 3).

Second, a somewhat similar pattern exists for levels of hostility. Floor level, however, is clearly most significant for this dimension of strain (Table 4). Furthermore, these strains are affected by the number of households living in a dwelling unit, not the number of people living there. It is also clear that—when considered individually—floor level, number of households in a dwelling unit, and number of people in the unit have no independent effect on levels of strain.<sup>11</sup>

The effect of architectural features on individuals is indirect in that individuals do not respond directly to housing. Instead, housing affects patterns of social relationships, and individuals respond to the system of social relationships that housing conditions have helped to create.

Given the significance of housing for social relationships, we might expect that family relationships also would respond to different housing conditions.

#### HUSBAND-WIFE RELATIONSHIPS

The arrangement and amount of space that people occupy at home has little if any effect on their intrapsychic emotional stability. But it seems reasonable to assume that

<sup>10</sup> As a counter-hypothesis, it seems likely that people living on the lower floors of multistory buildings would be exposed to much higher and longer periods of noise and that this noise would be emotionally upsetting. There are some studies that suggest that if certain home noises continue for a long period, they might be destructive to hearing (Farr, 1967). Other studies have also suggested that home noises can be severely disturbing (APHA, 1948; Council of Europe, 1964; WHO, 1968; Eisenberg, 1969). However, Lemkau (1970) has noted that, although there is evidence relating physiological disturbances to noise, it has not been adequately demonstrated that there is a relationship between this disturbance and psychological disorder. The present findings from Hong Kong lend support to Lemkau's position.

<sup>11</sup> Floor level and sharing arrangements also affect levels of concern about a lack of space in Hong Kong dwelling units.

Table 4. Percentage of Respondents Scoring High on Index of Hostility.<sup>a</sup>

Floor	One Household in Unit	Two or More Households in Unit
Ground	36 (143)	32 (69)
1st-5th	42 (598)	45 (487)
6th or higher	45 (282)	54 (103)

<sup>a</sup>Figures based on one half of sample.

the physical boundaries of social action in the home would have clear effects on interpersonal relationships among family members. If this is so, then the wide variations in physical housing arrangements in Hong Kong should reflect themselves clearly in the measures of family relationships to be presented here. Three measures will be considered: the frequency of husband-wife communications, tension in husband-wife relations, and marital happiness.

Husband-wife communications are considered because verbal communication is no doubt the most common form of interaction between people. As a form of interaction, it is the basis on which positive feelings of husbands and wives toward each other are created and sustained. Furthermore, the frequency of interaction is a determinant and indicator of the degree to which the husband-wife relationship is "group-like"; that is, communications represent a dimension of family integration.

Three questions were used to construct an index of husband-wife communication.<sup>12</sup>

<sup>12</sup>Two of these questions were preceded by a general query: "Some couples discuss more things than other couples. In general, how often do you and your spouse discuss the following topics:

Amusing or interesting incidents that happen to you?

Things you have talked about with friends and others?"

The third question was: "Some people talk with their spouse when they have problems and worries. Some would hide them in their heart when they have worries. How many of your worries and problems do you talk over with your spouse?" For the first two questions, the answer categories "many times" or "sometimes" are given a score of 2. The answer category "seldom" is scored 1, and the answer category "never" is scored zero. For the

Whereas the index of husband-wife communications measures the integrative features of marriage, our measures of tension reflect destructive and disintegrative features. Again, three items enter into the index of tension.<sup>13</sup> Finally, a rather general question was asked to tap overall marital satisfaction.<sup>14</sup>

These various dimensions of husband-wife relationships are highly associated with each other, and they are each strongly influenced by conditions found outside the family and its housing (Mitchell, 1969a). Therefore, our attention to housing is at best superficial in the present context.

With these qualifications in mind, the tabulations suggest that high densities appear to affect husband-wife communications and marital happiness, but not quarrels. That is, compressing the arena for family interaction apparently affects husband-wife relationships, especially levels of communication. However, our other studies found that the educational level of spouses has an important influence on levels of family cohesion; therefore, the educational level must be statistically controlled in assessing the significance of density for cohesion. When this is done, the effects of density on cohesion are entirely eliminated.

To see whether the division of space within a dwelling unit affects family cohesion, we introduced along with densities several other

third question, a score of 2 is given to the answers "all of them" or "most of them"; a score of 1 is given to "some of them"; and a score of zero is assigned to "just a few of them" or "don't you ever talk about your worries and problems with your spouse?" The maximum possible score is 6, and the minimum score is zero. Twenty-two percent of the husbands and 27% of the wives have a maximum score of 6.

<sup>13</sup>These questions were preceded by a general query: "How often do you and your spouse argue or disagree about: money, uses of leisure, and how each spouse treats the other." An index based on these items was scored by giving the answer categories "often" and "sometimes" a score of two; "hardly ever" is scored one; and "never" is scored zero. The index has a range of scores from zero to six. Eleven percent score high on the index (scores of four to six), and another 16% have a score of three.

<sup>14</sup>The question: "I know this is difficult to answer, but could you tell me how happy you are with your marriage?" Twenty-four percent of the men and 15% of the women were "very happy"; only 4% of the men and 13% of the women are either "somewhat" or "very unhappy."

housing features. They included the area of the unit and the number of rooms in it. Again, however, there is no meaningful or consistent pattern of effects arising from these features. Furthermore, husband-wife relationships do not appear to be adversely affected by "forced" interaction in the presence of nonkinsmen, in contrast to the effects on individuals arising from these situations. These situations refer to the combination of multiple households in the upper floors of multistory buildings.

#### PARENT-CHILD RELATIONSHIPS

In addition to information on husband-wife relationships, a number of questions were asked about parent-child relationships. Note is made here only of the degree to which high densities force children out of the home and away from the control of their parents. The simple question used for this purpose was: "How often do . . . your children . . . play outside the house and you do not know where they are?" This was asked only of parents with a child 18 years of age or younger living at home.

High densities appear to affect the amount of surveillance that parents have over their children. For the higher the density, the higher is the proportion of parents who say that they don't know where their children are playing.

Since lower-class families are less likely to exercise close control over their children, and since lower-class families are the most likely to live in high-density housing, statistical controls must be applied with regard

to the family's class position. Even when family income or the education of the parents is considered, however, high densities still facilitate low control over children (Table 5). Furthermore, densities have this predominant effect even when floor level, which indicates the potential ease of parental surveillance, is statistically controlled.

Similar perspectives on problems of space are reported for secondary school pupils with regard to the difficulty they have in finding a place to study. They are more likely to have this difficulty if they live in one of the following types of high-density housing: a resettlement estate, a room other than a flat, a bed space, a cottage, or a squatter hut. Except for those who live in a "whole house," an absence of a place to study becomes more serious as the size of the pupil's family increases. No doubt some of these young people have to study outside their homes, since studying at a crowded home may be annoying and disturbing to them, thereby adversely affecting their educational development.

Therefore, it seems likely that high densities create uncomfortable situations for members of a family in which there are children. To reduce the consequences arising from high density, children are given greater freedom to leave the home, thereby reducing the density. But this response tends to weaken the surveillance that parents have over their children, and this weakened supervision may in turn facilitate the development of many of the social problems of youth assumed to be characteristic of high-density slum communities around the world.

Table 5. Percentage of Parents Who Sometimes or Often Do Not Know Where Their Children Are, by Parents' Level of Schooling.<sup>a</sup>

Sq. Ft. per Person	None	Some Primary	Some Secondary	Secondary or More	Total
22 or less	43 (136)	31 (165)	40 (53)	† (5)	37 (359)
23 to 42	30 (137)	25 (184)	13 (89)	16 (25)	23 (435)
43 to 66	18 (44)	17 (107)	18 (66)	13 (24)	17 (241)
67 and over	8 (40)	19 (91)	8 (72)	7 (101)	11 (312)

<sup>a</sup>Figure based on the family study.

†Figure too small to show percentage.

## ENTERTAINING NON-FAMILY MEMBERS

Finally, a number of separate questions were asked regarding the family's relationships with kin and nonkin living outside the family. But again only one element in this larger picture will be discussed: whether people entertain others in their home. The question was: "When you invite people home for an ordinary meal or for relaxing and chatting, how many do you usually invite?" Forty-nine percent of the Hong Kong married people said that they never invited people to their home.

Densities affect entertaining practices, for the higher the density, the less likely a family is to entertain. This is true even when family income is statistically controlled, although the relationship is considerably reduced.

## SUMMARY AND DISCUSSION

The purpose of this paper has been to ascertain the effects, if any, that different dimensions of housing, especially density, have upon patterns of interaction among members of a family, upon levels of emotional strain manifested by individuals, and upon attitudes toward housing.

After various controls are applied, densities within individual dwelling units are seen to have only a very limited range of effects. These effects are not altered by considering other features of housing, including the number of rooms, amenities, absolute amount of area, and the number of individuals within a dwelling unit. The major effects are listed and discussed below.

1. Attitudes toward housing, especially toward the amount of space that one has and toward a lack of privacy, respond clearly to densities within dwelling units. No doubt many critics of high-density housing, along with those subjected to these densities, are very conscious of the absence of space. But this consciousness should not be confused with deeper levels of emotional strain.
2. High densities also affect two somewhat superficial manifestations of emotional strain: worry and unhappiness. It is necessary, however, to control statistically for other stresses producing these strains. When only one of these controls—the stress of poverty—is applied, these two superficial strains still respond to high densities, but they do so only for the poorest members of the community.
3. Densities do not affect deeper and more

basic levels of emotional strain and hostility.

4. Although high densities and other physical features of housing do not affect deeper levels of strain, the social features of housing have an important impact on these strains. Most importantly, the doubling-up of nonrelated households tends to create stressful situations, especially if it is difficult for the household members to easily escape each other by retreating outdoors. It is more difficult to retreat in this way when the dwelling unit is on an upper floor of a multistory building. Therefore, multistory buildings, when combined with sharing arrangements, can have negative effects on the emotional health of individuals. These effects, it is conjectured, probably arise from forced interaction among non-relatives, not from high densities or large numbers of fellow kinsmen. Large numbers of people in high density housing can be tolerated more easily if these people are one's kinsmen.
5. Although the various housing conditions have no apparent effect on patterns of husband-wife interaction, densities have a clear impact on parent-child relationships. Parents in high-density housing evidently do not discourage their children from leaving the house, thereby temporarily relieving the high densities. But this solution to high densities tends to reduce the parents' knowledge of and control over their children.
6. High density housing also discourages interaction and friendship practices among neighbors and friends.

Therefore, some of the major effects of housing pertain to the fabric of social life and social control in neighborhoods. Residents in upper floors in multistory buildings are no doubt encouraged to leave their dwelling unit if they share the unit with other households. If they do not leave, they tend to develop higher levels of emotional illness and hostility. Residents on the ground floor are able to avoid this situation because it is easier for them to use the street as an adjunct to their home. (This pattern of effects is much less common in public housing because the Hong Kong government's regulations discourage these sharing arrangements.)

Children who live in high-density housing, regardless of their floor level, also are pushed out of the house, a pattern that tends to reduce parental control over the children. And with very little home-visiting among neighbors who live in high-density dwelling units, the potential for friendly relationships among

neighbors and the development of a positive neighborly community consciousness is made more difficult. Individuals apparently can tolerate very high densities within their own family dwelling unit, but these high densities may create a street environment that is socially unhealthy to the community.

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# THE RELIGIOUS FACTOR IN DETROIT: REVIEW, REPLICATION, AND REANALYSIS \*

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American Sociological Review 1971, Vol. 36 (February):30-48

*In 1958 the Detroit Area Study dealt with "religion in the metropolitan community" and the resulting data became the basis for much of Lenski's The Religious Factor (1963). The 1966 Detroit Area Study repeated a number of the questions dealing specifically with the application of the Protestant Ethic hypothesis to Protestant-Catholic comparisons in America. One important question was successfully replicated, but other relevant questions failed to show reliable differences between Protestants and Catholics. Reanalysis of Lenski's original data using 1966 sample restrictions yields results consistent with both the single success and the several failures to replicate. Possible reasons for the largely negative outcome of the investigation are discussed: value changes over time, differences in population definitions by the two surveys, and lack of reliability for part or all of the original 1958 findings.*

THE application of Weber's Protestant Ethic thesis to the United States has two aspects that go far to explain its attractiveness to many American sociologists. On the one hand, the tracing of economic attitudes and behavior to religious values and beliefs is a nonobvious but intuitively interesting linkage. As such, it seems to provide a persuasive instance where sociology has developed insights that are not apparent to the layman. At the same time, the primary application of the hypothesis to the United States makes this more than a purely theoretical exercise. The division of the American population into one quarter Catholic and three quarters Protestant, it is argued, must be taken into account if we are to understand such nonreligious features of American life as its stratification and mobility system, economic organization, and socialization practices.

The single, most influential writing on the Protestant Ethic hypothesis is probably Gerhard Lenski's *The Religious Factor* (1963).<sup>1</sup> As a widely available and forcefully

written book, Lenski's work has been frequently cited.<sup>2</sup> No doubt a major reason for its impact is that it reports strikingly positive findings and concludes that the Protestant Ethic thesis is relevant to contemporary America, that Protestant-Catholic differences in secular attitudes are both large and manifold (more so in fact than class differences), and that these indirect effects of religion in America seem to be on the increase rather than the decrease. One other characteristic of Lenski's work that distinguishes it from most other efforts in this area is its concern to measure social psychological attitudes and values that intervene between religious identification and larger macro-social processes such as occupational mobility.<sup>3</sup>

Despite its intriguing nature, the Americanized version of the Protestant Ethic thesis, and more specifically Lenski's conclusions, have met with some criticism. At

<sup>2</sup> For example, the article on "The Sociology of Religion" in the new and highly regarded *International Encyclopedia of the Social Sciences* (1969, Volume 13, p. 412) describes *The Religious Factor* as "perhaps the most successful attempt to apply survey research to the sociology of religion," and briefly notes the positive findings of the book.

<sup>3</sup> Most other work on this topic investigate only the general connection between religious membership and socioeconomic variables such as income. See Glenn and Hyland (1967), which includes a fairly complete set of references to earlier studies. For more recent analyses emphasizing socioeconomic status, see Glockel (1969) and Warren (1970). Featherman (1969) provides one of the few analyses (other than Lenski's) which include intervening social psychological variables, though the intervention in this case is between different points in the achievement process for adults.

\* This investigation was carried out through the University of Michigan's Detroit Area Study. I am indebted to Edward O. Laumann with whom I collaborated in designing the 1966 Detroit Area Study. Many of the specific problems involved in replication and reanalysis were solved by James House and Paula Pelletier, who worked as research assistants on this part of the project. Gerhard Lenski kindly furnished advice on use of certain codes from his 1958 study.

<sup>1</sup> The 1963 Anchor edition is not simply a reprint of the original 1961 Doubleday edition but contains important changes in analysis and writing. All references in this paper will be to the 1963 edition.



the theoretical level, questions have been raised as to whether Weber really intended to argue that the Protestant Ethic was an important cause of the rise of capitalism, and, even if so, whether he expected the Protestant-Catholic distinction to continue to lead to similar economic differences in the twentieth century.<sup>4</sup> More practical questions have focused on whether such gross categories as "Catholic" and "Protestant" are analytically useful in the United States, since there are important ethnic lines within American Catholicism and both ethnic and denominational differences among American Protestants.<sup>5</sup> Finally, at the empirical level enough contradictory findings have appeared to raise some doubts about the reliability of the positive findings reported in *The Religious Factor* (see especially Greely, 1964).

The present paper will not attempt to deal with the first type of question, but will assume that whatever Weber may have intended, it is not unreasonable to determine whether there are Protestant-Catholic differences in beliefs and values relevant to economic advancement. We will deal briefly at one point with the second type of question on ethnic and denominational differences within the major religions, but that is also not our primary focus. The analysis and findings described here are mainly relevant to the third question, namely, the reliability, and therefore validity, of reported Protestant-Catholic differences, using Lenski's

earlier results as the focus of investigation. More simply, we shall present results from both a replication and a reanalysis of Lenski's findings, with a review along the ways of the original findings themselves.

Lenski's research was carried out in 1958 within the framework of the Detroit Area Study, a continuing survey research and training unit of the University of Michigan. It was appropriate that in 1966 the Detroit Area Study included as part of another research effort an attempt to replicate and extend certain small but important aspects of the investigation reported in *The Religious Factor*. However, the replication was by no means complete and exact, and several limitations must be stressed at the outset.

First, almost the entire focus of the 1966 replication was on the Weberian hypothesis developed and tested by Lenski in Chapter 3 ("Religion and Economics") of his book. The book deals with several other topics, such as "Religion and Politics," which are not relevant at all here, and with still other topics such as "Religion and Family Life," that are relevant but are tested only slightly, if at all, by the present data.

Second, this replication began more as an "extension" than as a simple replication. We hoped to duplicate Lenski's results and then to carry out—more carefully than his sample and techniques allowed—several further analyses, for example, on the relevance of ethnicity to religious differences. We did not, therefore, repeat quite all the questions needed for a full replication even of the economic aspect of the 1958 DAS survey, and we did not attempt seriously to follow his exact ordering of questions.

Third, replication is not a simple concept and can obviously never be carried out perfectly.<sup>6</sup> Apart from sampling error, which can be estimated, and real changes over time,

<sup>4</sup> The wide disagreements on this point indicate that *The Protestant Ethic and the Spirit of Capitalism* (Weber, 1930) can be read in different ways. I myself see Weber's citation of religious differences in education and business (pp. 38-40) as suggesting continuation into the twentieth century of Protestant-Catholic differences in secular values. That Weber, as a careful scholar, emphasized the complexity of any causal analysis of capitalism does not mean that he lacked a bold thesis himself. In any case, the impact of Weber's writing on the Protestant Ethic has certainly been largely based on this thesis, whether or not Weber himself would today subscribe to it.

<sup>5</sup> Glock and Stark (1965) have particularly emphasized denominational differences within Protestantism. Their dependent variables, however, are religious and moral values closely tied to the origins of American denominations, and they do not deal with values linked to the Protestant Ethic hypothesis. See Warren (1970) for a careful analysis of denominational differences in education, occupation, and income. An original review of *The Religious Factor* (Rosen, 1962) questioned the lack of attention to ethnic differences.

<sup>6</sup> The term "replication" is used here to refer to the deliberate repetition at a later point in time of part or all of a completed piece of research in order to determine whether the same results obtain. This usage is related to but not the same as that common to experimental designs (Fisher, 1951:58-60). A helpful discussion of the present usage appears in Lykken (1968). The type of replication used here would probably be termed "operational replication" by Lykken, as opposed to "constructive replication," which does not attempt to duplicate the research procedures of the original investigation but rather to operationalize the relevant constructs in other ways.

which often cannot, replication turns on many subtle factors in the operational definition of the population sampled and in the actual execution of the research process. The present effort is in many ways more exact than usually possible, because essentially the same organization (although not the same people) carried out much the same type of survey within almost exactly the same geographically and socially bounded population. (Indeed, successful replication in this case would still leave open the question of whether the findings could be extended beyond the Detroit Metropolitan area. Unsuccessful replication, however, does not carry this kind of ambiguity.) Despite the similar organizational and geographic frameworks, however, there are also important differences between the 1958 and 1966 sampling designs that will be discussed below.

#### *Sample Design in 1958 and in 1966*

Both the 1958 and 1966 data are based on samples from the metropolitan Detroit population—city and surrounding suburbs. The Detroit SMSA defined by the Bureau of the Census actually consists of three counties, but in both 1958 and 1966 thinly populated outlying parts of the counties (plus the city of Pontiac) were not included in the survey sample boundaries, primarily to reduce time and costs.<sup>7</sup> The sample area was defined to be somewhat larger in 1966 than in 1958 in order to accommodate expansion of actual suburban areas, but the percentage of the total SMSA population included in the total sample area was quite similar for the two years: 87% in 1958 and 85% in 1966. Suburban towns that had experienced recent growth were all included in the 1966 boundaries, and it does not appear likely that important parts of the “1958 population” had moved beyond the 1966 boundaries. Thus, we regard the total populations available for the two studies as essentially the same, except for whatever long-distance migration

may have taken place between the Detroit SMSA and the rest of the United States during the intervening eight years.

Although the geographically defined populations were essentially the same for the two studies, the 1966 survey screened out parts of the general population that were included in 1958. We need to indicate the likely effect of this differential screening on comparisons between the two sets of results.

The sample used in 1958 involved essentially a cross section selection of occupied dwelling units from the geographic area described above. Within each dwelling unit, all persons age 21 and over were listed and one was selected at random for interview. No type of household appears to have been screened out. Of the 750 households selected for interview, 656 resulted in complete interviews—a response rate of 87%.

The sample in 1966 drew a cross section of occupied dwelling units, but considered eligible for random selection within household, only white males, ages 21 through 64, born in the United States or Canada. A total of 1,013 cases were obtained, with a response rate of 80%.<sup>8</sup> Each of the screening factors requires brief consideration. Included in parentheses is the number of cases that were excluded from the sample as originally drawn because of the criterion discussed.

1. Exclusion of Negroes ( $N=332$ ): Lenski divided the Protestant population by race in all his calculations, and his main conclusions about the effects of religion are based on differences between white Protestants and Catholics. His decision can be questioned from a theoretical standpoint, but in terms of replication it means that an all-white sample is quite adequate for retesting his findings. One simply ignores the specific results and conclusions in *The Religious Factor* that involve the Negro subsample.
2. Exclusion of persons born outside the United States or Canada ( $N=181$ ): The effect of this exclusion should be to increase Protestant-Catholic differences, according to

<sup>7</sup> Information on the 1956 sampling was obtained from Lenski (1963:16-17) and from “Sampling Design for the 1957-58 Detroit Area Study” (1959). Neither of the above sources specified the lower age limit for the 1956 sample, referring simply to “adults.” The lower cut-off of 21 mentioned below is found in the “Interviewer’s Instruction Booklet” for the 1957-58 study. Information on the 1966 sampling appears in Schuman (1967).

<sup>8</sup> The actual number of interviews obtained was 985. Twenty-eight of these interviews were weighted double for sampling reasons explained in Schuman (1967). The number involved in the weighting is assumed to be too small to require adjustments in significance tests. A design effect due to cluster sampling of about 1.1 has also been ignored here because of its trivial size.

Lenski's 1958 results. One of his most striking conclusions is that differences between "socio-religious groups" are generally heightened when third generation Americans are studied separately.

3. *Exclusion of Women (N=268) and of Native-Born Men over 65 (N=92)*: In the chapter directly replicated by the 1966 study, certain of the tables are confined to men, and others include both sexes. There is no indication that results differ by sex where questions are relevant to both sexes. The same applies to age, except that the finding reported above on third generation effects suggests that Protestant-Catholic differences may well increase among younger people. Nonetheless, possible specification by age or sex must be allowed for and will be discussed below after presentation of major results.
4. *Differences in response rates*: The difference between the response rates for the two years is partly a function of the restriction of the 1966 sample to working age males—a particularly difficult category to reach. It may also reflect the general downward trend of response rates reported in most studies over the past two decades. Insofar as the latter is the case, the difference may reduce comparability between the obtained samples for the two years. While there is no particular reason to expect such an effect, especially given the relatively small percentage difference, the possibility cannot be ruled out. It provides a good example of the difficulty of achieving "literal replication" (Lykken, 1968).

#### RESULTS

Lenski's major conclusion concerning Protestant-Catholic differences in economic ethic is that "as a general rule, commitment to the spirit of capitalism . . . is especially frequent among white Protestants and Jews [and] is much less frequent among Catholics . . ." (Lenski, 1963:128). We will review the evidence he offers for this conclusion, presenting parallel results from the 1966 study where possible. Our initial focus will be on values and attitudes, rather than on indicators of occupational achievement or mobility, since as noted earlier Lenski's research remains valuable mainly because of its development of intervening social psychological variables. Our tables will include results for Jews when these were presented by Lenski, but our main focus will be on Protestant-Catholic comparisons. The Jewish sub-sample is quite small in both DAS surveys,

and it also involves other issues not directly tied to the Weberian debate.<sup>9</sup>

#### Work Values

After examining differences in upward mobility by religion, Lenski presents the first of a series of questions on work values and work-related attitudes. Each person in both the 1958 and the 1966 surveys was asked to rank the following in order of their importance when choosing a job:

1. High income
2. No danger of being fired
3. Working hours short, lots of free time
4. Chances for advancement
5. The work is important and gives a feeling of accomplishment.

The 1958 survey asked men and working women to answer for themselves, and wives to answer in terms of what they would want in their husbands' job; the 1966 survey, as mentioned earlier, did not include women.

Lenski considers the 5th alternative the one best representing the "Protestant Ethic" as conceived by Weber, the 4th also a partial indicator of the Ethic, and the 1st a popular but questionable approach to operationalizing the same construct. The 3rd alternative, and to a lesser extent the 2nd, are regarded as "completely in opposition to any conception of the Protestant Ethic" (Lenski, 1963:87).

Lenski notes "how strong a hold the Protestant Ethic, in the classical sense, has on all segments of the American population": nearly half of the 1958 sample ranked the 5th alternative highest, and two-thirds selected either the 5th or the 4th alternative. Our results in 1966 are almost identical, with 47% of the sample making the 5th

<sup>9</sup> Lenski does not always present exact N's with percentage findings. It is useful to record here the total N's for the two studies, even though a few "don't know" cases are usually lost for any given question.

	Protestants	Catholics	Jews	Total*
1958 Study	267	230	27	524
1966 Study	498	433	29	960

\* The 1958 sample also included 100 Negro Protestants and apparently 32 "Others" (Negro Catholics and white "other religion"), as calculated from figures in Lenski (1963:16 and 370). The 1966 sample included 53 white "other religion."

alternative first choice, and another 27% selecting the 4th alternative.

Turning to differences by religion, Lenski reports that the following percentages of the three religious groups rank first the "classic Protestant Ethic" alternative ("the work is important and gives a feeling of accomplishment"):<sup>10</sup> Protestants, 52%; Jews, 48%; Catholics, 44%. The difference of 8 percentage points between Protestants and Catholics is not great, but it meets the .10 level of significance (one-tailed) used in *The Religious Factor*. The small Jewish subsample does not differ significantly ( $p > .10$ , one-tailed) from either of the other categories. Lenski also notes, without presenting data, that "application of controls for class position of respondents and of their parents, and for the education of respondents . . . revealed that the differences between white Protestants on the one hand and Jews and Catholics on the other are greater among the better educated and in the middle classes than among the less well educated and the working classes." (Lenski, 1963:91).

On the other hand, Lenski finds that Jews and Catholics are high on alternatives 1 and 4 which refer to chances for advancement and high income. He reports only the percentage for the two alternatives combined and without controls:<sup>11</sup> Jews, 45%; Catholics, 40%; Protestants, 31%. This difference confirms Lenski's belief that there are two somewhat separate though related systems of work values which deserve to be distinguished. Protestants tend to be higher on the "classic" Protestant Ethic, while Jews and Catholics are higher on the more contemporary version. Finally, Lenski notes that all the reported differences are small and cannot account for the major part of the relative economic success of Protestants and

Jews as against Catholics. Perhaps for this reason, he does not include results for this question in his summary table comparing the size of differences by religion with the size of differences by social class (Lenski, 1963:326).

Our own findings on this question are reported in Table 1. The results successfully replicate Lenski's work in part. A somewhat greater proportion of Protestants than Catholics give first rank to the sense of accomplishment and worth of work. The difference of 5 percentage points between the two groups would not be significant for Lenski's sample sizes, but just makes the 10% level of significance (one-tailed) for our larger subsamples. There is also a trend for Catholics to be higher (by 4 percentage points) than Protestants on first choice given to income or chances for advancement. Lenski's finding that Jews were highest on first choice of advancement or income, and Protestants lowest, does not hold up well. The differences are slight and Jews are actually lowest on these combined responses (and highest on the "classical" Protestant Ethic response).

As noted above, Lenski reports, but does not present, data showing that the religious factor operates more strongly among the middle-class and better educated. We have created social class (occupation and income) and educational controls as close as possible

Table 1. Percent of Protestants (P), Catholics (C) and Jews (J) Giving First Rank to "Protestant Ethic Responses" (1966 Data).

Relig. Group	1st Choice to A <sup>a</sup>	1st Choice to B <sup>b</sup> or C <sup>c</sup>
P	49% (498)	40% (498)
C	44 (433)	44 (433)
J	52 (29)	38 (29)

\* Base N's in parentheses.

<sup>a</sup>"The work is important and gives a feeling of accomplishment."

<sup>b</sup>"Chance for advancement."

<sup>c</sup>"High income."

<sup>10</sup> It is not completely clear that totals for religious groups were used, but absence of qualifications makes this likely. Here as elsewhere in this paper, Lenski's fourth "socio-religious" category of Negro Protestants is omitted; all respondents included in the three categories shown are white.

<sup>11</sup> This assumes that each percentage stands for first choice of one alternative or the other—an assumption implied but not explicit (Lenski 1963:91). The percentages here, it should be noted, are largely determined by the preceding set, but the determination is not complete since there were two other possible response categories to the question.

to those generally used by Lenski.<sup>12</sup> Comparisons between Protestants and Catholics within class and educational categories for the work-values question are shown in Table 2. (There are too few Jews to allow for such controls.) Our results bear out the 1958 finding that differences between Protestants and Catholics are primarily located within the upper stratum. Among the upper-middle class and the college-educated, Protestant-Catholic differences increase slightly over that found for the total samples, while only small, though consistent, differences occur

<sup>12</sup> The social class and educational criteria used in the two surveys and the resulting distributions are shown below, with cases omitted where classificatory information is lacking:

among the other strata. In summary, then, our results in 1966 for this question are generally in accord with those obtained by Lenski in 1958, although all associations are small.

### *Attitudes Toward Work*

Central to Weber's conception of the Protestant Ethic was the high valuation of work as man's vocation in this world. This contrasts with the view that work is simply a necessary evil, or indeed a consequence of Original Sin. To explore further this contrast, both the 1958 and 1966 surveys asked the following questions: "Some people tell us they couldn't really be happy unless they were

#### *A. Percent in Each Social Class in 1958 and 1966*

Social Class	Criteria	Lenski's Dist. (Whites only)	
		1958 Dist.	1966 Dist.
Upper-Middle Class	White collar occupation: Family head income \$8,000 or more in 1958; \$10,000 or more in 1966.	17%	29%
Lower-Middle Class	White collar occupation: Family head income less than \$8,000 in 1958; less than \$10,000 in 1966.	26	18
Upper-Working Class	Manual or service occupation; Family head income \$5,000 or more in 1958; \$7,000 or more in 1966.	32	39
Lower-Working Class	Manual or service occupation; Family head income less than \$5,000 in 1958; less than \$7,000 in 1966.	25	14
Total N		100 (502)	100 (1004)

#### *B. Percent in Each Category of School Years Completed in 1958 and 1966.*

	Lenski's Sample	1966 Sample
13 years or more	21%	35%
12 years (high school grad.)	—	32
9 to 11 years	—	21
0 to 8 years	20	12
	100%	100%
N=	(524)	(1008)

The 1966 study used the same occupational groupings as Lenski, but raised the cutting point for income by \$2,000 to take account of changes in income level between 1958 and 1964. Partly as a result of this change, the 1966 "social class" distribution is somewhat different from Lenski's. There is also a slight difference in occupational distributions as such, since Lenski's combined middle-class categories account for 43%, as against 47% for the 1966 sample. This may represent a change in the Detroit occupational structure, but may simply be a result of sampling error plus the 1966 exclu-

sion of older men from the sample. The greater difference between the two distributions involves income within the two occupational categories, and since this is a relative matter it should not result in marked changes in other relationships. It is also clear that the present sample is distributed somewhat differently in education than Lenski's, no doubt in part because of the 1966 age restriction. We should also note that class differences between Protestants and Catholics in the present sample are not great and educational differences are virtually nonexistent.

Table 2. Percent Protestants and Catholics Giving First Rank to "Work is important" by Social Class and Education (1966 Data).

Social Class	Protes- tants		Catholics		Education	Protes- tants		Catholics	
Upper Middle	68½	(148)	58½	(94)	Some College and Above	62½	(165)	50½	(145)
Lower Middle	47	(86)	44	(91)	9-12 Years of School	44	(271)	42	(240)
Upper Working	40	(181)	38	(175)	0-8 Years of School	37	(62)	35	(46)
Lower Working	37	(78)	37	(68)					
							(498)		(431)

working at some job. But others say that they would be a lot happier if they didn't have to work and could take life easy. How do you feel about this? Why is that?" Respondents were then coded into one of three general categories: 1. a completely positive attitude toward work, which emphasizes its intrinsic rewards or its moral character; 2. a neutral attitude, which involves a preference for working, but one based on extrinsic factors, such as boredom with too much leisure; 3. a negative attitude, which involves a frank assertion by the respondent that he would be happier if he did not have to work.

The 1958 survey found the following percentages of males in each religious group expressing a *positive* attitude toward work: Jews, 42%; Protestants, 30%; Catholics, 23%. The difference between Protestants and Catholics here is significant at the .10 level, according to the table Lenski provides (1963:372). He also reports that controls for generation, region of birth, and class show Protestant-Catholic differences to be on the increase rather than on the decrease. "Catholics who were northern born, third-generation Americans were much more likely to have a *negative* attitude toward work than were first- and second-generation immigrants (36% vs. 14%)" (p. 90). The *opposite* trend occurred among Protestants for *positive* attitudes toward work (30% vs. 24%). Moreover, positive attitudes toward work were directly correlated with social class for Protestants, but the correlation was inverse for Catholics (see Lenski, 1963:98, Table 10).

Lenski interprets this to mean that Protestants take a more positive attitude toward more demanding and rewarding positions, while Catholics show a more positive attitude toward positions that are less demanding and hence less rewarding. None of the above controls are applied to Jews because of small sample sizes, and there is no comment on the largest percentage differences in 1958 for the question, those between Jews and *both* Protestants and Catholics.

Our own findings on this question are shown in Table 3, alongside Lenski's results where these were reported in detail. The value of placing results from both years together is to allow direct comparisons of trends in subgroup differences. Absolute percents for any given subgroup should not be compared, however, since as explained earlier the 1958 survey included parts of the population excluded in 1966. For the present question, the populations are somewhat closer than usual, since Lenski presents these data also only for males, but there are still differences by age and birth place.

Our results do not replicate those of Lenski's with regard to Protestant-Catholic differences. We find only a trivial and unreliable difference on this question for the two religious groups as a whole. When generation is introduced as a third variable, we find slight and probably unreliable effects *opposite* to those reported by Lenski. When social class is introduced as a third variable, there is little variation of any kind in *negative* attitudes toward work. There is a tendency for positive attitudes to increase with

Table 3. Percent of White Males Expressing "Positive," "Neutral," and "Negative" Attitudes Toward Work.

Religious Group	Lenski's Findings				1966 Findings			
	Pos.	Neut.	Neg.	N	Pos.	Neut.	Neg.	N
Protestants	30%	(not separated)	(-)		32%	58%	11%	(481)
Catholics	23	(not separated)	(-)		33	55	12	(421)
Jews	42	(not separated)	(-)		34	55	10	(29)
By Generation:								
Protestant, 3rd Gen. or More	30%	(not separated)		(40)	33%	54%	14%	(259)
Protestant, 2nd Gen. or Less	24	(not separated)		(37)	36	56	8	(125)
Catholic, 3rd Gen. or More	(not separated)		36	(22)	32	57	11	(190)
Catholic, 2nd Gen. or Less	(not separated)		14	(69)	35	52	14	(219)
By Class:								
Protestant:								
Upper Middle	36%	52%	12%	(25)	46%	46%	9%	(145)
Lower Middle	32	57	11	(19)	26	64	10	(84)
Upper Working	23	56	21	(39)	28	59	12	(172)
Lower Working	30	33	37	(27)	22	68	10	(78)
Catholic:								
Upper Middle	19	56	25	(16)	39	53	8	(95)
Lower Middle	18	57	25	(28)	34	57	9	(88)
Upper Working	18	64	18	(38)	30	56	15	(171)
Lower Working	33	48	19	(21)	30	55	15	(66)

\* Where N's are not given for Lenski's groups, they were not presented in the book. The total N's in 1958 were no doubt the same or close to those reported in our ft. 9.

Class criteria here are the same as those presented in ft. 12. With regard to generational criteria, in the 1966 study 1st-generation persons were, as mentioned earlier, excluded from the target population altogether, and only Northern-born respondents are included in the generation comparison. Lenski's survey includes 1st-generation immigrants, and his tables apparently exclude Southern-born respondents from third-generation or more categories, but not from second-generation categories.

class position for *both* religious groups, rather than a reversal in direction as Lenski found. The only line of support for Lenski's original results is that there is a greater spread between the top and bottom classes for Protestants than for Catholics, but the figures are of uncertain reliability and meaning, and they constitute only a very partial replication of the 1958 results.

### *Spending and Saving*

Shifting from attitudes toward work to values about consumption, Lenski "inquired into the manner in which families use their income." One question concerned installment buying: Changing the subject a bit, do you think it's a good idea or a bad idea to buy things on the installment plan? Lenski reports 46% of his total sample in favor of installment buying, 38% against, with the balance not decided. In 1966 we find 47% for, 50% against, and 3% other. The differences here are not great and could easily result from differences in sample design (e.g., Negroes included in 1958 but not in 1966), changes over time, or a combination of sampling error and format variation.<sup>13</sup>

Lenski reports the following percents by religious groups in terms of *disapproval* of installment buying: Jews, 56%; Protestants, 44%; Catholics, 40%. The Protestant-Catholic difference is not reliable even at the .10 level (one-tailed), but Lenski does find the same interaction with generation reported earlier: Catholics closer to immigrant status (1st and 2nd generation) are more disapproving of installment buying than are Catholics in or beyond the third generation; the opposite is true among Protestants. Table 4 presents the 1958 findings, along with comparable percentages from the present study.

<sup>13</sup> This question was included at the end of a five-minute attitude form which the respondent filled out himself mid-way in the hour interview. The form was completed entirely within the larger interview and does not result in loss of respondents; such a procedure was used primarily to allow observations of the house to be made by the interviewer for purposes unconnected with the present replication. The 1958 and 1966 questions were worded identically, but the 1958 version included an interview box for "unsure" if volunteered; the 1966 item did not offer such a choice explicitly. This latter minor difference in format occurs also for the "attitude toward work" question discussed earlier.

Table 4. Percent Disapproving of Installment Buying.\*

Religious Group	Lenski's Findings	1966 Findings
Protestants:	44% (-)	52% (481)
Middle Class, 3rd Gen.	38 (60)	50 (159)
Middle Class, 1st & 2nd Gen.	34 (32)	49 (67)
Working Class, 3rd Gen.	51 (41)	54 (190)
Working Class, 1st & 2nd Gen.	42 (53)	52 (61)
Catholics:	40% (-)	50% (422)
Middle Class, 3rd Gen.	35 (31)	44 (94)
Middle Class, 1st & 2nd Gen.	41 (51)	49 (86)
Working Class, 3rd Gen.	35 (34)	51 (107)
Working Class, 1st & 2nd Gen.	39 (84)	53 (126)
Jews	56% (-)	39% (28)

\* For the analysis by class, both studies exclude Southern-born respondents. Only Lenski's study includes 1st generation immigrants.

Our overall findings by religion show even less of a Protestant-Catholic difference than the small one reported by Lenski. We also found a reversal for Jews, with the latter less rather than more disapproving of installment buying than the other two groups. This was such a puzzling finding that it led us to check our entire coding procedure for the question, but a count based on the original 29 raw questionnaires for the subsample of Jews confirmed the finding already reported.<sup>14</sup>

<sup>14</sup> A control for occupation shows that this favorability toward installment buying is much stronger among Jewish businessmen (7 of 9 favoring) than



Simultaneous controls for generation and class produce slight trends in the direction Lenski noted: third-generation Protestants are more disapproving of installment buying than second-generation Protestants, while the reverse effect is found among Catholics. However, the percentage differences are very tiny and could easily result from chance or from other differences correlated with religion, e.g., socioeconomic differences not controlled by the crude categorization into "middle-class" and "working-class." Certainly the net "interaction effect" found in the present study is much less than that reported in *The Religious Factor*.

#### *Other Attitudes and Values Regarding Work and Consumption*

*The Religious Factor* presents the results for several relevant attitude questions which we did not attempt to replicate exactly in the 1966 study. Lenski's findings on these questions will be briefly reviewed, with our reason for omitting the question or the results of modifying it also noted.

(a) *Belief in the Possibility of Success:* Lenski asked two questions designed to determine whether a respondent believed it is possible to rise from the working-class to the middle-class and whether he believed that ability rather than family connections makes for success. He reports that with class held constant (dichotomized into Middle-class vs. Working-class), over 10% more Protestants than Catholics hold such beliefs. We did not attempt to replicate these questions because we could not see a clear connection between them and Weber's central focus on the Protestant Ethic. As Lenski himself had earlier stressed, the concern with work that supposedly developed out of early Protestant views had to do not with the striving after extrinsic success, but with positive satisfaction in pursuing one's vocation in this world.

(b) *Divine Concern with Economic Striving:* A somewhat more relevant question seemed to be one that asked whether "God

is more pleased when people try to get ahead, or when people are satisfied with what they have and don't try to push ahead." But Lenski found only a five percentage points difference between Protestants and Catholics on this question, though in the "right direction." He also reports that the question was difficult for respondents to answer and that many tried to avoid the forced choice. For these reasons, the question was not included in the 1966 study.

(c) *Use of Leisure Time:* In another chapter of *The Religious Factor*, Lenski reports data from an earlier (1953) Detroit Area Study, which shows a trend for Protestants to say they would use extra leisure time productively (e.g., social service work, reading, gardening) rather than indulgently (e.g., loafing, going to movies, shopping). This seems to be directly relevant to Weber's conception of a puritan work ethic, and we included the following similar (although not identical) question in our study immediately after the question on whether the respondent would be happier working or not working: "Suppose you *did* have a lot of free time. What would you most like to do with it?" Responses were coded into two basic categories of "Productive" (mainly self-improvement in content) and "Indulgent" (mainly casual social contacts, sports, and recreation). Differences by religion in 1966 are shown below:

	Productive	Indulgent	Total	N
Protestant	22	78	100%	(476)
Catholic	26	74	100%	(424)
Jewish	21	79	100%	( 28)

Differences are slight, and if anything the trend is for Catholics to be less "indulgent" than Protestants.

(d) *Concern with Thrift:* Respondents in 1958 were asked how important it is to save regularly, and why? This "ant-and-grasshopper" inquiry was used to determine whether respondents emphasized ascetic self-discipline in the early Calvinist tradition or indulged themselves in more consummatory directions. Results are not reported in detail, but apparently Protestants and Catholics did *not* differ in belief in the desirability of saving. The only difference found between the two groups was that a greater proportion of

among Jewish professionals (4 of 11). We suspect that this question may be ambiguous to some businessmen, and that they may interpret it as indicating favorability toward allowing installment buying by customers, rather than in terms of personal preference.

Protestants (28%) than Catholics (19%) gave more than one reason for saving. We did not find this numerical result as pertinent to the thrift argument as other questions, and therefore did not attempt to repeat the long series of inquiries used to obtain it.

(e) *Follow-up to Installment Buying:* After the question on installment buying reported earlier, the 1958 study asked: "Why do you feel this way?" Lenski reports that in explaining their disapproval, Protestants more often cited moral reasons than Catholics: 30% of the working-class Protestants, as against 18% of the working-class Catholics. Figures for middle-class respondents are not given, but by implication are less clear. Of the additional attitude questions treated in this section, this is the only one which seems both directly relevant to the main argument and somewhat promising in terms of reported results. It is unfortunate that we were not able to include it in the 1966 study.

#### *Positional and Performance Measures*

Although Lenski does not explicitly distinguish between attitudes and values on the one hand and performance indicators on the other hand, such a distinction appears essential. Weber's emphasis, although historical in its source of data, was basically social-psychological in nature. Individual Protestants were seen as developing beliefs and values which in turn led to distinctive performance in the world. Such performance finally led to changes in social position, for example, to a rise in one's occupation, income, and general status. If only one of these links can be focused upon, the psychological dimension seems most proximate for investigating the construct "Protestant Ethic." Differences in social position or even in performance cannot by themselves tell us very much about their origins. With this qualification in mind, we will examine the remaining evidence that Lenski offers for differences in Work Ethic between the two religious groups.

(a) *Labor Union Activity:* As suggested above, Lenski believed that "in many respects the values for which the unions stand are in opposition to the values embodied in the 'Protestant Ethic' and the 'spirit of capitalism.'" This is because unions view

work as a necessary evil and emphasize security rather than achievement. The Benjamin Franklins of today should presumably be men who wish to escape from the union rank and file, not contribute to its solidarity.

Lenski focuses on the lower middle class (clerks and salesmen) as the place in the social structure where union membership is possible but not taken for granted. He finds that 38% of the Catholics in the middle class ( $N=27$ ) belong to unions, while only 15% of the middle class Protestants ( $N=33$ ) have such membership. (No difference appears among working class men.) Our results do not replicate the white collar difference: among clerical and sales workers we find that 23% of the Protestants ( $N=66$ ) and 23% of the Catholics ( $N=49$ ) belong to unions.

The 1958 study did not include other questions on union involvement. But Lenski refers to other surveys done through the Detroit Area Study and elsewhere which show Protestant-Catholic differences. He cites the 1952 Detroit Area Study data showing that Protestant union members attend union meetings less often than Catholics and also say they are less "interested" in their union than Catholics. We did not repeat either of these questions exactly, but we did ask union members whether they felt "very involved" or "not very involved" in their union. Exactly 28% of the union members (177 Protestant union members, 166 Catholics) of each religious group claimed to be "very involved." A control for occupational level did not change this indication of no difference.

(b) *Self-Employment:* Just as unions are seen by Lenski as the locus of collectivistic and security-minded sentiments, so the self-employed are viewed as representatives of individualism. He finds little difference between Protestant and Catholic males in overall rates of self-employment (8% and 10%, respectively). Controls for immigrant generation and region of birth alter the picture, with Protestants showing a greater representation among the self-employed. (see Table 5). Lenski concludes from these trends (and similar trends for past self-employment) that "even in the bureaucratized modern metropolis there are real and significant differences among the major socio-religious groups in the degree to which they value occupational

Table 5. Percent of Each Religious Group Self-Employed.\*

Religious Group	Lenski's Findings		1966 Findings	
1st & 2nd Gen.				
Protestants	15%	(39)	11%	(266)
Catholics	8	(71)	11	(193)
3rd Gen. Northern Born				
Protestants	12	(42)	14	(118)
Catholics	4	(24)	13	(211)
Total				
Protestants	10	(-)	12	(384)
Catholics	8	(-)	12	(404)
Jews	">50%" (27)		36	(25)

\* 1st generation men were not included in the 1966 sample. Both samples used only males. Lenski does not report the N's for the two missing entries.

independence and autonomy, with Jews ranking first, white Protestants second, and Catholics third" (1963:104). The findings of the present study, however, confirm these

earlier results only with regard to Jews. As Table 5 shows, we find no difference at all between Protestants and Catholics on this measure.

(c) *Budgeting*: Lenski was interested in discovering whether Protestants were more likely than Catholics to budget family expenditures. He found, however, no difference between the two religious groups on this question, and we therefore did not attempt to replicate the question.

(d) *Vertical Mobility*: We have saved until last the question of "vertical mobility," which Lenski treats first. Although it is obviously important to discover such differences if they exist, their origin is bound to be ambiguous. Even given the same attitudes and values, groups starting from different points will end at different points.

Lenski reports on the conflicting evidence in this area, and then presents data (p. 85) from the 1958 study showing greater Protestant than Catholic intergenerational mobility among white males. His data are not unambiguous, and this is an area where there is a fair amount of replication, often with larger national samples and using more recent analytic techniques.<sup>15</sup> Nevertheless, we

<sup>15</sup> See Glockel, *op. cit.* and Featherman, *op. cit.*

Table 6. Occupational Level of White Male Respondents by Father's Occupation and Religion (1966 Data).\*

Respondent's Occupational Level	Father's Occupation							
	Middle		Upper Working		Lower Working		Farmer	
	Prot.	Cath.	Prot.	Cath.	Prot.	Cath.	Prot.	Cath.
Upper Middle	54%	47%	32%	28%	22%	27%	18%	21%
Lower Middle	18	17	12	10	12	10	10	0
Upper Working	10	17	23	34	27	35	26	53
Lower Working	17	19	33	28	39	28	46	26
Total	100	100	100	100	100	100	100	100
N	153	95	112	144	147	162	77	20

\* Occupational levels here and in Lenski (1963, Table 8, p. 85) are not the class levels described in ft. 12 above, but instead involve distinctions between: (1) professional and managerial, (2) clerical and sales, (3) craftsmen and foremen, and (4) operatives, service workers, and laborers. (Based on communication from Gerhard Lenski.) Note also that males over 64 and foreign-born are excluded from the 1966 sample and the above table, but not from Table 8 in *The Religious Factor*.

have closely comparable data on this subject for the Detroit metropolitan area for 1966, presented in Table 6. Following the same mode of analysis that Lenski uses, we do not find much consistent support for his conclusion concerning greater Protestant mobility. Evidence in one part of the table is contradicted by evidence in another part, and the N's are too small to allow diverse conclusions based on specific pairs of cells.

#### RELIGIOUS INVOLVEMENT AND THE PROTESTANT ETHIC

A problem with comparisons of "Protestants" and "Catholics" is the vague and omnibus nature of such labels. For one thing, "Protestant" covers many different denominations and sects in America, and Weber was at pains to detail differences as well as similarities among these. In addition, within religious groups, individuals differ widely in the depth of their involvement, and if indeed it is religion that is a source of attitudes and values, we might expect these variations in involvement to lead to variations in adherence to the "Protestant Ethic."

Lenski handles the first problem by arguing that "denominational groupings within Detroit Protestantism no longer constitute self-contained socio-religious groups" (1963:21). He notes that his 1958 data show large "amounts" of marriage and friendship across denomination lines, and that half the Protestant respondents were in favor of denominational mergers. Most significantly, he reports that dependent variables in his data show few differences by denomination that are not simply accounted for by class position or regional background (1963:396-398).

Our own larger sample allows a somewhat more adequate analysis of denominational effects, and shows them to be about as impressive as simple Protestant-Catholic differences. Using only the four denominations on which we have at least 75 respondents, and employing the Work Values question discussed earlier, we obtain the results shown in Table 7. Even with controls for three indicators of socioeconomic status, five of the six between-denomination comparisons are as large as the original Protestant-Catholic difference we reported in Table 1, and four of

Table 7. Percent Choosing "The work is important and gives a feeling of achievement"<sup>a</sup>, by Denomination (1966 Data).

Religious Group	Without Controls	Contr. for Socioecon. Status <sup>b</sup>
Baptists	35% (103)	39% (103)
(Catholics) <sup>c</sup>	44 (433)	- (433)
(All Prot.) <sup>c</sup>	49 (498)	- (498)
Presbyterians	52 (75)	47 (75)
Lutherans	53 (110)	53 (110)
Methodists	59 (87)	57 (87)

<sup>a</sup>The "work values" question was dichotomized into the fifth alternative quoted here vs. the other four combined.

<sup>b</sup>The four denominations shown here were included as one set of predictors in a multiple classification analysis. The other predictors used as controls were education, occupation, and income. Results are not changed appreciably if only education is included as a control.

<sup>c</sup>Protestant and Catholic figures from Table 1 are also shown here for comparison purposes. Note that "all Protestants" include respondents from other denominations in addition to the four separated out here.

the six are as large as the Protestant-Catholic difference reported for this question in *The Religious Factor*. It is not clear that the ordering of denominations is what a careful reader of Weber would anticipate, since the more "Calvinistic" Presbyterians score closest to the Catholics, while the Lutherans are unexpectedly high in giving the classic Protestant Ethic response. However, it does appear that "Protestants" constitute a rather heterogeneous category, and that at the very least we must expect responses for that category to vary depending on the varying proportions of these denominations in different parts of the United States.<sup>16</sup>

<sup>16</sup>We also attempted to learn to what extent a control for ethnicity affected the Protestant-Catholic difference reported in Table 1. We included our three religious groups (Protestants, Catholics, Jews) and also eight nationality groupings (based on a question about "What nationality background do you think of yourself as having—that is, besides being an American?") as predictors in a single

Let us turn now to individual differences in personal involvement in religion. Lenski deals with the important issue of depth of commitment by constructing one index of "associational involvement" in the church as a formal organization, and another index of "communal involvement" in the informal but encompassing subcommunity of co-religionists. The former is operationalized by frequency of attendance at church services, the latter by the degree to which primary relationships are limited to members of the same religion. (See Lenski, 1963:23, for details of index construction.) He shows that the two indices are only slightly related to each other, hence can usefully be treated as separate measures.

Although Lenski makes a convincing case for the general importance of "communal involvement" as a vehicle for religious influence, it turns out that the index does *not* distinguish within religious groups in terms of economic attitudes and behaviors. He concludes that "the churches, rather than the sub-communities, are the primary source of the differences in economic behavior" between Protestants and Catholics (pp. 124-125).

As the above quotations suggest, associational involvement does appear to be an important specifying variable for the 1958 data. In most of the questions where comparisons were possible, frequency of church attendance among Protestants was positively associated with the Weberian "spirit of capitalism." There was no, or even a slight, negative relationship for the same variables among Catholics. The quantitative results for each question are not presented by Lenski in tabular form, but most of the questions which are reported to show the relationship involve social positions (e.g., self-employment) rather than values or attitudes. In particular, neither the question on what is valued about

work, nor positive attitudes toward work as against leisure, showed any relation in 1958 to degree of associational involvement. This lack of a relation is not treated by Lenski as a negative finding concerning the influence of the religious factor on economic behavior, but rather as a sign that "the Protestant churches have allowed the doctrine of the calling to be neglected."

Our own findings, reported in Table 8, differ considerably from Lenski's, insofar as comparisons can be made.

1. Lenski reports that the "work values" question showed little relation to associational involvement; we find a consistent trend for the more actively involved among both Protestants and Catholics to say that the significance of the work is their main concern in choosing a job. This may simply reflect uncontrolled variations within crude class categories that are related to associational involvement.

2. For attitudes toward work, we find a relation that Lenski apparently expected: positive attitudes going with frequent church attendance among Protestants but not among Catholics. However, Lenski's own *findings* appear to have been in the opposite direction.

3. Lenski finds both occupational self-employment and disapproval of installment buying greater among more actively involved Protestants. We find weak trends in the opposite direction for both religious groups.

4. Lenski reports that both membership and interest in unions are least among active Protestant church-goers. We find opposite trends.

More generally we find little systematic or meaningful difference in the effect of associational involvement on Protestants and Catholics. We have not been able to attempt replication of certain questions used by Lenski in this analysis because they were not included in the 1966 survey, but the results we can check are not encouraging.

Lenski reports several other ways in which some of the dependent variables under discussion seem to be influenced by type of religious commitment. For example, he finds that a "devotional" orientation, as indicated by personal prayer and concern to determine God's will when making decisions, is positively associated with a commitment to work. But in this instance, as elsewhere in these

multiple classification analysis. With nationality controlled, the original five point percentage difference between Protestants and Catholics on the Work Values question is halved. Thus ethnicity does seem to be a relevant variable, as Rosen (1962) and others have argued. Both ethnic and denominational effects are much slighter when the dependent variable shifts to the "attitudes toward work" question. Apparently the latter variable is simply not a useful one in this study, for as we saw earlier it fails to show any significant relation to religion.

Table 8. Percent of "Protestant Ethic" Responses for High and Low Associational Categories within Religious and Social Class Categories (1966 Data).\*

"Protestant Ethic" Response	Protestants				Catholics			
	Middle Class		Working Class		Middle Class		Working Class	
	Active	Marg.	Active	Marg.	Active	Marg.	Active	Marg.
	(79)	(154)	(58)	(201)	(144)	(37)	(155)	(85)
Intrinsic work valuation in job choice	67%	56%	43%	38%	54%	43%	42%	29%
Positive attitude toward work	43	36	32	25	37	38	31	29
Disapproves of installment buying	43	53	47	55	46	46	48	60
Presently self-employed	16	18	2	4	22	27	4	6
Member of union**	31	21	57	64	19	36	61	67
Very involved in union**	23	4	19	16	5	9	17	20

\* The measure of Associational Involvement here is slightly different from Lenski's. Lenski classifies as Active "those who attend worship services every week, plus those who attend services two or three times a month and also some church-related group at least once a month," with all others classified as Marginal. The 1966 questionnaire included only a question on frequency of attendance at religious services, and we have distinguished only between those who attend at least once a week (Active) and those who attend less often (Marginal).

\*\* For these two questions, middle class refers only to clerical and sales workers, and the relevant N's are reduced to 13 (Protestant-Active), 53 (Protestant-Marginal), 37 (Catholic-Active), and 11 (Catholic-Marginal).

supplementary findings, the relation of religious variables to other variables is not associated with Protestant-Catholic differences, but holds equally for both religious groups. Therefore, we do not attempt here to replicate such results.

#### REANALYSIS OF LENSKI'S 1958 DATA

We have reviewed Lenski's 1958 findings bearing directly on the application of the Protestant Ethic hypothesis to Protestant-Catholic differences in the United States, and have presented evidence from a 1966 replication of most of the more promising 1958 results. The 1966 survey provides a single important confirming replication regarding work values, and we will explore it further below. Against this lone successful replication, a variety of failures to replicate have been presented.

As indicated earlier, our 1966 sample

omitted women, the foreign born, and men over 65, and it also had a somewhat lower response rate than did the 1958 study. It is conceivable that these differences in target populations account for some of the differences in results. Although we have not attempted a complete reanalysis of Lenski's 1958 data, we have done so using age, sex, and nativity controls on the initial Protestant-Catholic differences for three main items: "work values," "attitudes toward work," and "installment buying." It is worth noting that the reanalysis proved a great deal more difficult than anticipated, because of the problems encountered first of all merely in reproducing the original results reported in *The Religious Factor*, before proceeding to introduce sample controls. In only one of the attitudes reviewed here were we able to obtain Lenski's published results perfectly, although in the other two cases the

discrepancies were minor and very likely due to differences in placement of one or two cases. The possible explanations for such minor discrepancies are manifold: errors in our, or Lenski's, use of computing equipment; errors by Lenski in preparation of tables or at later stages in writing; errors by us in interpretation of Lenski's codes and recodes; actual damage to the data over the intervening decade. The whole effort left us with a healthy respect for the variability of percentages for reasons other than either sampling or measurement error, and with a sense of how easily borderline significance levels can be altered in this size sample by minor changes in coding conventions (for example, inclusion or not of missing data when calculating percents). We suspect that these problems in reanalysis are not unique to Lenski's data, and that only with the greatest care is it possible to transmit data and codes in reliable form from one investigator to another.

When controls for age, sex, and nativity were applied to bring Lenski's sample to essentially the same composition as the 1966 replication, the results did in fact tend to converge. For the "work values" item this means that in the 1958 tables rerun using only 21- to 64-year-old, native-born males, Protestant-Catholic differences were reduced to almost the exact sizes of the 1966 results given in Table 1. For example, "the work is important . . ." shows a 6% Protestant-Catholic difference in Table 9, as compared with our 5% difference in 1966. Thus our earlier report of a successful replication for this item holds and is even improved when the 1958 and 1966 samples are brought more closely into line.

The two other items in Table 9 show a similar movement toward the 1966 results when similar age, sex, and nativity controls are applied. This means, however, that Protestant-Catholic differences for these items tend to disappear among 1958 native-born men under 65. For the "attitude toward work" item, the original difference reported by Lenski appears to come entirely from men over 64 and men born outside the United States—relegating the result either to the record of a disappearing generation or an indicator of the religious correlates of old age. Because of the difficulties noted above in

Table 9. Percent of 21-64 Year Old Native-Born Men in Each Religious Group Giving "Protestant Ethic" Responses (Reanalysis of 1958 Data).

Religious Group	% Saying "The work is important..."			N
Protestants	50%			(93)
Catholics	44			(88)
	Attitude toward Work			
	Posi- tive	Neu- tral	Nega- tive	
Protestants	26%	52%	22%	(89)
Catholics	26	57	17	(86)
	Disapprove Install- ment Buying			
Protestants	29%			(91)
Catholics	27			(88)

this reanalysis, along with the small n's within subsample categories and the generally small percentage differences by religion found for all these items, we are reluctant to place great emphasis on such specific findings.

#### FURTHER PURSUIT OF OUR ONE POSITIVE FINDING

Most of our conclusions about an intrinsically religious factor must, therefore, be negative. But not quite all, for a single question on work values repeated in 1966 did, as already reported, replicate Lenski's results; moreover, stronger findings were produced in favor of his hypothesis than were apparent in his own 1958 data. Also, the question seems theoretically closer to the conceptual meaning of the "Protestant Ethic" than most of the other variables introduced by Lenski. It is possible, of course, that the finding is due to chance, given the large number of results that have been reviewed here. But it is important to avoid Type II as well as Type I errors, and for that reason the "work values" result bears closer examination.

The 1966 difference between Protestants and Catholics in their basis for evaluating jobs has proven surprisingly tenacious. In particular, under a variety of controls Protestants, significantly more often than Catholics, rank as most important their attitude that "the work is important and gives a feeling of accomplishment." Examination of this response using combinations of socioeconomic background factors has specified the religious difference, but in the process generally sharpens rather than eliminates it. Several such controls are shown in Table 10.

The difference on this question is rather clearly located in upper middle-class and, to

a lesser extent, upper blue-collar occupations. These occupational differences in turn are not much changed when either respondent's education or father's education is held constant. An interesting interpretation of the results might note that the three occupational areas in question—professional, managerial, and craftsmen—are probably the most non-routinized parts of the occupational structure. They are types of occupations that can be entered *both* for intrinsic reasons relating to the content of the work and for extrinsic reasons having to do with advancement in income and status. Religious sources of values could thus operate to fill these posi-

Table 10. Percent Emphasizing Importance of Work (5th Alternative) in Choosing a Job, by Religion, Occupation, and Education (1966 Data) \*

Respondent's Occupation

Rel.	Profes- sional	Managerial	Clerical	Sales	Craftsman	Operative & Laborer
Prot.	69 (99)	59 (68)	53 (36)	40 (30)	48 (103)	33 (157)
Cath.	<u>58 (67)</u>	<u>49 (71)</u>	<u>52 (25)</u>	<u>38 (24)</u>	<u>41 (134)</u>	<u>34 (109)</u>
Dif.	+11	+10	+ 1	+ 2	+ 7	- 1

Respondent's Occupation (Selected) and Education

Rel.	Professional			Managerial			Craftsman		
	0-8	9-12	13+	0-8	9-12	13+	0-8	9-12	13+
Prot.	(0)	54(22)	72(77)	(4)	58(31)	64(33)	60(20)	46(76)	(7)
Cath.	<u>(2)</u>	<u>54(11)</u>	<u>57(54)</u>	<u>(1)</u>	<u>51(35)</u>	<u>46(35)</u>	<u>29(24)</u>	<u>37(84)</u>	<u>42(26)</u>
Dif.		0	+15		+ 7	+18	+31	+ 9	

Respondent's Occupation (Selected) and Father's Education

Rel.	Professional			Managerial			Craftsman		
	0-8	9-12	13+	0-8	9-12	13+	0-8	9-12	13+
Prot.	68(41)	66(32)	80(20)	57(30)	79(19)	44(16)	44(70)	59(17)	(9)
Cath.	<u>53(17)</u>	<u>55(33)</u>	<u>69(16)</u>	<u>49(47)</u>	<u>42(19)</u>	<u>(5)</u>	<u>40(77)</u>	<u>42(36)</u>	<u>47(15)</u>
Dif.	+15	+11	+11	+ 8	+37		+ 4	+17	

\* Each percent is shown with its base N in parentheses. Percents are not calculated for N<15.



tions not with differential frequency, but rather for different reasons and perhaps with different consequences in terms of actual role performance.

In the light of previous sections of this report, we will wish to regard this line of thought as merely an intriguing hypothesis for further investigation, certainly not as a conclusion based on broad empirical evidence. The findings first of all require urgent replication, since they have been selected as the only "reliable differences" from among a large number of comparisons.<sup>17</sup> If replicable, they then suggest the value of concentrating the search for a current equivalent of the Protestant Ethic in particular directions. This means not only locating it more specifically in the social structure, but also both clarifying and, if possible, validating the secular value dimension involved. Rather than expanding the Protestant Ethic thesis in many and diverse ways, construct specification seems very much in order.

#### DISCUSSION

The 1966 Detroit Area Study replication of parts of the 1958 Detroit Area Study has called into question conclusions derived from the earlier survey that bear on "religion and economics." Four explanations are available.

(1) There may have been a genuine change in Protestant or Catholic attitudes and values during the intervening eight years. This explanation seems the least plausible one to me, because the variables involved do not appear subject to rapid change on the basis of transient events. Lenski's own conclusions point toward gradual widening of Protestant-Catholic differences.

(2) Variations between the two surveys may result from the more limited population defined by the 1966 study. Women, men over 64, and the foreign-born were deliberately excluded, and the response rate was also somewhat lower in 1966. There is little in

Lenski's analysis to suggest that these factors *should* affect major conclusions, but our re-analysis of Lenski's 1958 data does seem to point to such special but not especially meaningful subpopulations as the source of some of his findings.

(3) Lenski's results may point toward religious differences of a much more specific and limited character than he expected and concluded. This is the import of the one successful replication in 1966, which concerns a single but strategic question on work values and locates the religious difference mainly among men in professional and managerial occupations. Those interested in the Protestant Ethic hypothesis would seem well advised to follow up this result, staying close to whatever the item represents. It must also be kept in mind that the difference discovered here is by no means a large one.

(4) There is the real possibility that all the differences reported by Lenski, as well as the one most successfully pursued in the 1966 data, are due to "chance." The reader who has followed the detailed comparison of the 1958 and 1966 results may have noted by now that the original findings presented by Lenski were by no means uniform and large in their implications for a "religious factor" in the economic realm. For a number of variables introduced by Lenski, there were no reliable differences *even* in 1958, and in other cases the differences barely met the rather lenient demand of a one-tailed significance level of .10. In still other cases, there were no 1958 differences between Protestants and Catholics as a whole, but differences appeared in certain subgroups when controls were applied. Such subgroups were often quite small, and selective attention to them capitalized on sampling error. A more general problem that runs through *The Religious Factor* is a tendency to assume a basic Protestant-Catholic difference as fact, and therefore to interpret negative findings as adding specific nuances in meaning to the nature of this fact, rather than as calling it into general question. For example, when "communal involvement" turns out to be unrelated to economic values, Lenski's conclusion is that "the churches, rather than the subcommunities, are the primary source of the [religious] differences in economic behavior" (1963:124-125). But another, and

<sup>17</sup> The application of significance tests to complex survey analysis has been attacked and defended regularly at least since Hanon Selvin's (1957) "A Critique of Tests of Significance in Survey Research." However, none of the more controversial issues are involved in the present point, which concerns only the loss of clear meaning to conventional significance levels as an analyst scans many tables and selects for interpretation the few that are "significant."

at least equally reasonable conclusion, would have been that an important linking hypothesis had been tested and disconfirmed, raising the problem of whether the earlier findings meant what they seemed to mean about religious differences.

While the specific implications of this paper have to do with the Protestant Ethic thesis as developed by Lenski, there is surely a larger implication concerning the place of replication in empirical sociology. *The Religious Factor* was an original and stimulating work, and while the analysis carried out within it seems somewhat problematic in 1969, the primary issue lies not with the book but with the need to strengthen traditions that make replication an assumed part of sociological research. This is particularly true when the original finding is a nonobvious but intuitively interesting one. As with all science, the development of sociology can be greatly stimulated by unexpected results, but the more remarkable an empirical finding, the more it requires careful scrutiny and systematic replication.

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## THE RELIGIOUS FACTOR IN DETROIT: REVISITED

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*American Sociological Review* 1971, Vol. 36 (February):48-50

SINCE Editor Schuessler has been kind enough to invite a brief reaction (but "not . . . a rebuttal") to Howard Schuman's excellent article (*ASR* 36:30), I find it hard to resist the temptation. Though I

have not written much on the interrelations between religion and other major institutional systems since the publication of *The Religious Factor*, I still find the subject immensely interesting.

In the closing paragraph of Schuman's article, he argues persuasively for the need for more frequent replication, especially "when the original finding is a nonobvious but intuitively interesting one." I think this is a fair characterization of much of *The Religious Factor*. In my research, I have never been very interested in exploring the obvious. I have always had too great a respect for the basic sociological insights of educated laymen, especially journalists, politicians, lawyers, and others whose work forces them to be observers of society. Therefore, it has always seemed to me that the chief task of the empirical researcher is to search out the unobvious—with all the risks that this entails.

Applied to the subject of religion at the time I undertook my study, the influence of religion on secular social life seemed anything but obvious. Most social scientists seemed to assume it did not exist, if one could judge from the absence of reference to it in their writings and research. Yet there were hints in both theory and research that this might not be the whole story. These hints were especially numerous in the area of politics (even before the 1960 presidential election), but they were not wholly lacking in the field of economics. Still, in the middle 1950s, when I first began to think about this study, systematic data were few and far between. This led me to write in the Preface to my book that I hoped that my study would "stimulate further research."

I mention this background because it obviously influenced my stance on the crucial "fully-only" issue. Faced with modest, but potentially important, differences on many questions, especially in the economic area (to which Schuman's replication is limited), I concluded that a "fully" strategy made more sense than an "only." To have adopted the latter might well have buried the issue for another twenty years.

In one respect, at least, this strategy worked. The number of empirical studies relating religious attitudes and behavior to other areas of life has increased substantially since 1961, and there are suggestions that at least some of these studies were in response to *The Religious Factor*.

Clearly, some of the "findings" of *The Religious Factor* have not found support in

subsequent studies. This is more often the case in the area of economics than in the area of politics. But even in the economic area there seem to be some tantalizing, and potentially important, differences that hold up. The chief of these in Schuman's study is the one concerning differences in work values which, as Schuman notes, are concentrated in the professional, managerial, and skilled worker groups, "the most nonroutinized [and I would add, most critical] parts of the occupational structure."

This finding is particularly interesting because it links up with the findings of several other studies which report that Protestants are somewhat more likely than Catholics to be recruited to *these same occupational strata*, at least when controls are applied for differences in origins and opportunities (Jackson *et al.*, 1970; Glenn and Hyland, 1967; Organic, 1963; Mayer and Sharp, 1962; and Weller, 1963). Though this pattern has not been found in certain other studies, including Schuman's (see also Lipset and Bendix, 1959), those which report positive findings generally employ more rigorous techniques of analysis. The Jackson *et al.* and Organic studies are especially impressive, since by using the standardization technique they bring the scattered evidence of large and complex mobility matrix tables together into a single crucial statistical comparison.

I would not want to claim that differences in rates of occupational mobility of the type reported in *The Religious Factor* and these other studies have yet been confirmed, but neither would I concede that they have been refuted. If anything, the net balance of subsequent research on this intriguing subject seems to suggest that as recently as the 1950s, differences between the two major religious groups still survived in American society, and Schuman's findings suggest a reason for this.

But even if this debatable thesis is accepted, it can still be argued that the differences involved were not large. However, this should not surprise anyone who works with a complex model of human behavior. If survey research of thirty years tells us anything at all, it is that no single variable explains much of the variance in individual behavior. Men are every bit as complex as the better novelists have been arguing for years. All

this is why I believe that Table 50 may yet prove to be the most important table in *The Religious Factor*. That is the table in which I sought to show that modest as most of the differences were between white Protestants and Catholics, with class controlled, they were roughly comparable on the average, *over a wide variety of variables*, to the differences between the classes with religion controlled.

This brings me to my final point: times are changing, and some of the most important changes in the religious realm have been of a kind that few would have predicted as recently as the early 1960s. Even now, Vatican Council II seems to have been more a sport than a natural outgrowth of a dominant earlier trend.<sup>1</sup> Vatican Council II was a major religious revolution: it legitimized dissent within the Church, thereby reordering the whole structure of authority both organizationally and intellectually and thus sweeping the Catholic Church swiftly into the mainstream of contemporary life. In effect, it was a kind of belated consummation of the Protestant Reformation. Because of this, I am inclined to think that such differences as existed between Catholics and Protestants a decade ago have been seriously eroded and are likely to diminish even more in the decade ahead. Thus, *The Religious Factor* is, at best, a picture of an era that has ended.

If I have any real criticism of Howard Schuman's otherwise excellent piece of research and analysis, it is that he seems to lack an interest in change. Nowhere in his

<sup>1</sup>Almost the only sociologist I know who came close to anticipating the revolution which Vatican Council II precipitated was Joseph Fichter, who, in an exchange of correspondence with me in the middle 1950s, predicted a fairly early victory for the liberally oriented theologians over the conservatively minded bishops (though I doubt that even he anticipated anything quite so swift or sweeping as the Council and its consequences).

article did I find a reference to Vatican Council II, yet that historic series of meetings took place between the time of my study and the time of his replication. Like many of the rest of us, he has probably been overly influenced on the theoretical level by structural-functionalism with its curiously synchronic, ahistorical orientation, and on the methodological level by the technique of the sample survey with its built-in timeless bias. Personally, I believe that these limitations can be overcome and must be overcome. Structural-functional theory should be replaced by theories which take the time-dimension explicitly into account and which make social change a prime concern, and sample surveys should be designed, not merely for replication (important as that is), but even more for time-series analyses. But I say this less as a criticism of Schuman than as a suggestion for all of us.

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#### ITEMS—Continued

and it is anticipated that they will discuss it at their next meeting in February. Perhaps some useful guidelines will emerge from these and related discussions. ASA members are urged to communicate their views to Professor Ely Chinoy, Department of Sociology, Smith College, Northampton, Massachusetts 01060.

■ By now ASR readers have grown accustomed to the "new look" of our tables, which are typed in this office. These "home-made" tables have placed

an added burden on our small but stalwart staff. To ease this burden, we will shortly make available a set of directions for typing of tables by author. These directions require that horizontal tables cover either 38 pica spaces or 80 pica spaces, exactly. When a vertical table is called for, it must be exactly 125 pica spaces in width, and no more than 49 lines in length. We intend that this statement will be available shortly after the first of the year.

K. S.

# FREEDOM AND SOCIAL ORGANIZATION: A COMPARATIVE ANALYSIS \*

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American Sociological Review 1971, Vol. 36 (February):51-65

*The problem consists of an apparent, discrete difference between communal and formal organizations. Research was designed to test the first hypothesis: If a group is primarily oriented to the attainment of a specific goal, then it excludes familial behavior. Four types of groups are selected: (1) Groups such as Relocation Centers for the Japanese in the U.S. in World War II, "captive communities," challenge the hypothesis in seeming to contain the family in a formal organization. (2) Groups such as Shakers and Trappist monks, "limited communities," seem to exclude the family from communal organizations. The other groups are logical counterparts of the first two: (3) "Intentional communities" (Hutterites and Kibbutzim) and (4) "total institutions" (English Public Schools). Neither should require that the first hypothesis be modified if it is valid.*

*Study of the Relocation Centers generated the second hypothesis: The greater the deprivation of freedom, the greater the antagonism of those deprived. Freedom is thought to increase as alternatives increase, to require discipline, to be relative to the definition of the situation and thus to be both psychological and social. Study of the limited communities generated the third hypothesis: Only if a group is not primarily oriented to the attainment of a specific goal will it maximize the freedom of its members. Intentional communities and limited communities support all hypotheses. The organization of the group appears largely responsible for the definition of the situation relative to freedom. The family, although not necessary to communal organizations, is nevertheless quite basic to it and is antithetical to formal organizations.*

A COMPARATIVE analysis can be pursued in many ways: it can be cross-cultural, or it can focus on different groups within the same society. It can compare groups between broad categories as well as within them. This paper reports on research that at one time or another has involved all of these approaches, although only one type (between-category comparisons) is emphasized in the present discussion. The central concern in the study is to understand differences between various kinds of highly institutionalized human groups. The thesis is that they fall roughly into two discrete types, called here "formal" and "communal" organizations. The initial hypothesis to this distinction (outlined below) has already received some verification; more is added here. Two additional hypotheses

were developed and are presented in this paper.

*The Initial Hypothesis.* The analysis begins with a research finding from a recently completed study (Hillery, 1968). There appeared in the data a sharp, qualitative distinction separating formal organizations (such as businesses, hospitals, and prisons) from communal organizations (such as cities, villages, and neighborhoods). This difference apparently depends on whether the group is organized primarily to obtain a specific goal. If the group is so organized, then it is a formal organization, if not, then it is a communal organization.<sup>1</sup>

Such a qualitative difference is unusual, not only in the study of community but in sociology as well. We more often discuss variations in quantity; we stress the continuum instead of the either-or difference. The basic research strategy chosen to examine this qualitative difference consists in a study of that area where the distinction *seems* to be weakest, i.e., to study those

\* Revised version of a paper read before the Midwestern Sociological Society, St. Louis, Missouri, April 16, 1970. I wish to express my thanks to all those who have generously contributed advice and suggestions, and in particular to James L. Price, Julia Brown, Gene Summers, Charles Perrow, Alvin H. Scaff, and Robert Nisbet. The study resulting in this publication was made possible by a Ford Foundation Faculty Research Fellowship. The contents of this discussion, however, are the sole responsibility of the author.

<sup>1</sup> The relation between specific goals and formal organization is of course part of the literature. The converse is almost never stated, that communal organizations are *not* primarily oriented to the attainment of a specific goal.

groups that could possibly contain the characteristics of both formal and communal organizations (at least on an *a priori* basis). One of the central characteristics of formal organizations has already been mentioned: specific goals. Research to date (cf. Hillery, 1968) indicates that one of the central characteristics of communal organizations is the family (i.e., a group having at least as its nucleus a sanctioned sexual union of a man and a woman who are also engaged in child nurture). The basic question may thus be phrased in terms of Hypothesis 1: *If a human group is primarily oriented to the attainment of a specific goal, then it excludes familial behavior.*

This hypothesis grows out of a taxonomy of human groups, the essentials of which are presented in Chart 1. This is not the place to give the rationale for developing the taxonomy (see Hillery, 1968). It should be noted that there are innumerable ways of differentiating human groups. The criteria chosen here are admittedly not sufficient for a complete understanding of human groups, no more than the criterion of motility is sufficient for understanding the difference between animal and plant life. But like motility, the criteria are believed to be useful—certainly for present purposes.

One problem deserves special treatment, since it figures so largely in community literature, namely, the problem of spacial use or territory. It is interesting that for conceptual purposes the problem of territory appears to become of less theoretical importance as the concepts become more general. Thus, while territory is quite important to the understanding of a particular city or a particular prison, generally it does not seem to be as important to understanding the difference between communal and formal organizations. In fact, Nisbet (1970) has introduced a distinction between personal and territorial groupings that cuts across the taxonomy introduced here. His distinction is a useful one and has an extensive development (e.g., Maine, 1861; Jenks, 1898). It raises the distinct possibility that the emphasis of community theorists on territory may be misplaced. In the struggle of the state (as a formal organization) against the family and related communal organizations, the state has sought increasingly to destroy

the power of its older rival groups, and one way in which this was done, as Jenks (1898) has shown, was by giving more consideration to an individual's territorial location than to his group (i.e., communal) membership. In fact, it could well be argued that formal organization, per se, increasingly emphasizes a sharper definition of boundaries, growing out of a greater interest in controlling individual behavior, which stems in turn from the concern of the organization with task orientation. From this point of view, it would be argued that the important point is not territory itself but rather the relationship between territory and an emphasis on specific goals.

There are numerous other such problems that could be mentioned. The discussion here is limited to only one set of criteria which can be shown to be fruitful for raising important points about the nature of human groups. The groups to be studied are arranged within the taxonomy as they would appear if the taxonomy were valid. But certain groups are so chosen as to challenge the taxonomy. Thus, two taxonomies are presented in Chart 1: A general theoretical taxonomy, represented by the broad categories of Formal and Communal Organizations, Expressive and Informal Groups. (For the development of this taxonomy, see Hillery, 1968). Within this chart, a smaller taxonomy is presented (elaborated in Chart 2) depicting Captive, Intentional, and Limited communities and Total institutions. Captive communities constitute a challenge to the hypothesis in that they appear to contain the family in a formal organization; limited communities appear to challenge the hypothesis in that they seem to exclude the family from communal organization. The particular intentional communities are selected such that they modify the family in certain ways; the particular total institution is chosen because it departs from practices common to total institutions generally. In spite of these variations, if the initial hypothesis is valid, neither the intentional communities nor the total institutions should require a modification of the general taxonomy.<sup>2</sup>

<sup>2</sup> A point to note of some methodological importance is that the groups listed in Chart 2 are ap-

Chart 1. A General Taxonomy of Human Groups.

The system has primacy of orientation to specific goals

Yes

No

Degree of Institutionalization

High

FORMAL ORGANIZATIONS	COMMUNAL ORGANIZATIONS
State	Nation
Firm	Vill (village or city)
Local Business	Family
"Captive Communities"	Intentional Communities
Total Institutions	Limited Communities
.....	.....
EXPRESSIVE GROUPS	INFORMAL GROUPS
Social Movements	Ethnic Groups
Crowds	Cliques

Low

— qualitative difference  
.... quantitative difference

Source: Adapted from Hillery, 1968.

*Note on Terms.* Formal and communal organizations are in essence defined in the chart. They are both highly institutionalized, differing in that formal organizations have primacy of orientation to the attainment of specific goals, whereas communal organizations do not (see below for a more complete explanation). Expressive and informal groups are not of concern to this study, although it may be noted that the dotted line in Chart 1 is intended to express the existence of a continuum as opposed to a discrete difference

proximately comparable as concerns the degree of institutionalization and the level of commitment or involvement of their members.

Chart 2. A Research Taxonomy Emphasizing Borderline Cases of Communal and Formal Organizations.

The system has primacy of orientation to specific goals.

Yes

No

Contains the Family

Yes

No

CAPTIVE COMMUNITIES	INTENTIONAL COMMUNITIES
Relocation Center	Hutterite Kibbutz
Slave Plantation	
Concentration Camp	
TOTAL INSTITUTIONS	LIMITED COMMUNITIES
English Public School	Shaker Trappist

(represented by the solid line). Groups of low institutionalization are depicted as not being sharply distinguished in respect to goals: this is because their goals are less often defined with precision.

In view of the difficulty of defining "community" (cf. Hillery, 1951), the term is used very loosely; in this research, "community" refers simply to the three kinds of groups listed in Chart 2. All communities, then, are not necessarily communal organizations, according to the general taxonomy (i.e., captive communities). Other communal organizations are shown in Chart 1.

Since the concept "goal"<sup>3</sup> is used quite broadly in the literature, it is important that "specific goal" be defined clearly. The definition follows Parsons (1960:17-18). A specific goal has at least three characteristics: (1) the product of the goal is identifiable, such as automobiles, M.D. degrees, etc.; (2) the product can be used by another system; i.e., the output of one system is the input for

<sup>3</sup> This term has been developed to the greatest extent by Perrow, 1968. Cf. also Etzioni, 1964; and Gross, 1968, 1969.

another system, and (3) the output is amenable to a contract; e.g., it can be bought and sold. Again because of the many connotations of "goals," it may be helpful to use the term "output goal" as synonymous with "specific goal" (See Gross, 1968, 1969).

The hypothesis may be more clearly (if less briefly) stated by expressing "output goals" in terms of their consequences. In order to produce an output goal, the members of a group must behave such that a measurable output results, and this output in turn must be able to be contracted for by another system. The hypothesis maintains that the wholehearted commitment of a group to such a goal (or even several such goals) is antithetical to the family; e.g., such a group would exclude persons from acting out familial roles within the group.

The group-types that are used to investigate this hypothesis are discussed in the following order: (1) captive communities, (2) limited communities, (3) intentional communities, and (4) total institutions.

*Captive communities.* A study of the Japanese Relocation Centers in the U.S. in World War II (hereafter called "the Centers") brings forth the following two points: (1) the Centers were in part communal organizations, but (2) they cannot be understood without introducing the concept of freedom. In other words, these were *captive* communities, and to ignore the fact of their captivity, of the loss of their freedom, is to ignore a key point. The Centers in general represent one of the best-documented group types in the sample, much of the important literature still being accessible. (See especially Spicer *et al.*, 1969, Thomas and Nishimoto, 1946; Leighton, 1945). The story is too well known to require more than a brief description. In response to mounting war hysteria in World War II, the Japanese in the West Coast, both citizen and alien, were evacuated inland. Most of them (110,000) were sent to ten "Relocation Centers," located primarily in the western states.

There were important differences in the Centers, as will be seen, but they shared in common the fact that the people were forced to set up living patterns against their will, in places not of their own choosing. Further, the places to which they were sent

were "administered"<sup>4</sup> by a staff they did not select. Most of the evacuees stayed in the Centers for two and a half years.

The Centers strongly confirm the hypothesis that was advanced at the beginning of this paper. Perhaps most important is that the staff of the Centers manipulated the families contained within the Centers, in one way or another. Sometimes this was done directly, by internment of a male member. More often, the manipulation was unintentional, as when family members felt compelled to follow other members who were reassigned to certain camps because they classified themselves as disloyal. At other times, some members were encouraged to leave the Centers, whether or not they went with their families. The significant point is that family life was secondary to the task of custody, and the family was made to fit custodial rules more than vice versa. In other words, the decision to place the Japanese in custodial centers was made and maintained apart from any consideration as to whether they were family members. To be sure, provisions were made for families, if the Japanese happened to have families. But familial considerations had no effect on the specific goal of custody. Therefore, the first hypothesis is maintained: To the extent that a group (here, the staff) is primarily oriented to the attainment of a specific goal (in this case, custody), it excludes the family (in that families, as families, were not at the same time permitted to be both staff members and evacuees).

The question arises: what is the difference between captive and other communal organizations? The inescapable conclusion is implied in the label that has been given them: *captive* communities have been denied a significant increment of their freedom. The problems raised when the concept of freedom is introduced are admittedly formidable, but face them we must if we are to understand what has happened.<sup>5</sup>

<sup>4</sup>This concept was introduced by Weingrod (1966) and later elaborated by Kushner (1968). The present writer contends, however, that the Centers are significantly closer to total institutions than they are to administered communities.

<sup>5</sup>The most penetrating discussion of freedom that has come to my attention is that of Kolb (1953). He elaborates many issues that are only mentioned tangentially in this paper and others that are not



Freedom is not often discussed in the sociological literature,<sup>6</sup> and accordingly it is helpful to consider some related concepts. In part, freedom is reciprocally related to power—the more power one has over me, the less freedom I have.<sup>7</sup> Following Bierstedt's analysis (1950), we may think of power as latent force, and force as the reduction of alternatives. It follows that in one sense freedom increases as one's alternatives increase (Hillery, 1968:75–76.)

But even from what little has been presented here about the Centers, it is apparent that freedom depends on the definition of the situation. One defines the things that constitute his freedom, the things that are his alternatives. (I do not define my skin or my rib cage as a prison, and so I do not feel constrained.) This leads to a second aspect of freedom, the act of making a choice (developed in Roth, 1969). If one alternative is chosen, another is thereby ruled out, and thus the choice necessitates a discipline. From this point of view, the act of disciplining one's self is as much a part of freedom as is an increase of alternatives. For example, referring again to the ghetto, if a group of Jews build a wall around their quarter to keep the gentiles out, we may class this as an act of freedom. If on the other hand these same gentiles were to build a wall against the wishes of the Jews, this would be an act of deprivation, of loss of freedom.

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discussed at all. The major difference from this paper is that Kolb's emphasis is more psychological; mine is more sociological; and Kolb is more concerned with a discussion of freedom per se.

<sup>6</sup> In addition to Kolb's (1953) work, other pertinent discussions are those of Nisbet, 1962; DeGré, 1946; and Furfey, 1953:117–118. A few words are needed concerning related concepts, which are nevertheless not the same as freedom in its more general sense: *Alienation* involves not as much the product of the interaction between groups (which is the emphasis in this paper) but the loss of interaction (as is shown in the use of the concepts "estrangement," "separation," "malaise," etc.). *Autonomy* refers to the purely psychological aspect of freedom. *Centralisation* is one aspect of disciplined freedom, though it can also be regarded as a deprivation of freedom, depending on the definition of the situation of the actors involved. The concept of *voluntarism* may be treated as roughly synonymous with freedom (see Joseph Fichter's discussion of this concept in Gould and Kolb, 1964).

<sup>7</sup> Nisbet (1953, 1962) has argued that community cannot be understood except in the light of power.

These observations can be ordered under four headings. Recognizing that we only begin to describe freedom here, we may say that (1) freedom increases as alternatives increase; (2) any selection from among alternatives requires discipline; (3) freedom is relative to the definition of the situation and thus is both psychological and (4) social.<sup>8</sup> In regard especially to the first two points, they appear to be two dimensions, although interrelated. On the one hand, alternatives cannot increase forever. On the other hand, man is social and his freedom is thus disciplined by the necessity of association with people (cf. Nisbet, 1953, 1962: 237). In fact, people themselves may and do become part of one's alternatives, as in mate selection.

The alternatives, it should be noted, need not attain any special quantity (beyond some plural) in order to be significant; the value of the selection depends on the system. Choice of a job within a prison, for example, is not equal to choice of whether to go to prison or not. It follows that in some cases a single choice may determine all subsequent alternatives (cf. Parsons, 1966:9 on cybernetic aspects of control).

These concepts can be readily applied to the Centers. (1) As mentioned, a group of people were forced or compelled (i.e., made to act against their will) to settle in the various Centers and to live there, most of them for several years. (2) The people responded to this act with an attitude that could best be called antagonistic (Spicer, 1969). This occurred in all of the camps, though as we shall see, in varying degrees. In fact, from the evidence a second hypothesis may be advanced: All other things equal, *the greater the deprivation of freedom, the*

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<sup>8</sup> An important point is made by Berger (1963: 141), and one which is assumed in this paper: freedom is more than a scientific concept, and only some of its manifestations can be observed under controlled conditions. It is the scientific manifestations in which we are interested.

Part of the problem for future research is disentangling the relation between the more objective aspects of freedom (such as providing additional alternatives) and the more subjective (psychological) aspects. The fact that some prisoners may love their chains ("institutional dependents") argues for this separation in fact. At the present stage of research, the distinction is more conceptual than heuristic.

*greater the antagonism<sup>9</sup> of those deprived.* The point is not whether such a hypothesis is obvious or not. The point is that it is a matter of record (Spicer, 1969:83), and if it can be extended to other groups, it will have far-reaching consequences for conclusions we may wish to draw concerning social organization.

Evidence on the extent and degree of antagonism can be measured in part by replies to a question asked of all persons in the Centers who were 17 years of age and over: "Will you swear to abide by the laws of the United States and to take no action which would in any way interfere with the war effort of the United States?" (Thomas and Nishimoto, 1946:61.) Response varied from Tule Lake, in which 42% of all eligible persons either refused to answer or answered negatively, to Granada and Poston, in which only 2% and 6% (respectively) fell in this category. Poston is particularly interesting, because though it contained the next to lowest percentage of persons who refused to cooperate, it was also one of the Centers with a severe strike in which military action of some sort was involved (Leighton, 1945). A comparison between the two Centers—one at Tule Lake and the other at Poston—is instructive.

In Poston, the disturbance was solved fairly quickly, and the Project Director took a course of action which eventually led to the Japanese themselves assuming significant control of the Center (Leighton, 1945). Tule Lake, however, was designated as the camp to which all those who openly professed disloyalty to the United States were sent with their families. Disturbances at Tule Lake eventually led to the imposition of martial law and application for denationalization by seven out of every ten of the Japanese from this Center who were American citizens (Thomas and Nishimoto, 1946:156-157).

The importance of a comparison between Tule Lake and Poston is that the people at Tule Lake were put there because many of them were defined as disloyal, by themselves and by the U.S. Thus, (1) there was a definition of the evacuees at Tule Lake as persons

who were opposed to the U.S. administration, (2) there occurred a more forcible deprivation of freedom than at Poston, and (3) this was accompanied by a greater antagonism.<sup>10</sup>

Similar observations can be had from similar conditions (although the data are not nearly so complete), particularly the slave planation (Bryce-Laporte, 1968), the more extreme types of concentration camps (such as some maintained by the Japanese in World War II—Vaughan, 1949), and various types of ghettos (Wirth, 1928; Poll, 1969; Liebow, 1967). In each case, if the members of a community believe that their freedom has been deprived, they will respond with behaviors that can be called acts of antagonism and conflict. Where open acts of conflict are prohibited (as in the Japanese Internment Center at Bacoled in the Philippines), hostility may be displayed in increased morbidity and mortality rates, and increased weight loss (Vaughan, 1949:112-113; Bryce-Laporte, 1968:153). But the crucial element is the definition of the situation. Such antagonism is not as apparent if the boundaries around the communal organization are drawn by the people themselves; this is the lesson to be gained from Wirth's (1928, especially pp. 50-51, 290) classic study of the ghetto and Poll's (1969) study of the Hasidim.

Are captive communities then "really" communities? It is impossible to explore this point here; however, we can say that captive communities, such as the Centers, contain all those components found in other communities, i.e., as listed in the model common to cities and villages, called "vills" elsewhere (see Table 1; Hillery 1963, 1968). But the Centers include a component not found in the vill—a staff to which they are antagonistic, from which they are split.<sup>11</sup> The important thing about this staff is that it causes a behavioral reaction among the inmates that can be identified with this antagonism.

Captive communities, therefore, must be conceptualized as composed of two discrete parts: (1) a formal organization or "staff"

<sup>9</sup> "Antagonism" is partly synonymous with Bales' (1950) "negative reactions" in his categories of interaction, though it also includes violent forms. It is used here in the same sense as "conflict."

<sup>10</sup> The hypothesis receives extensive support in Leighton's principles (1945:232, 235, 239-240, 252, 329), though I claim that staff and inmates are not both communal organizations (see Leighton, 1945:343).

<sup>11</sup> Such a split *may* occur in vills, but it is not necessarily (or even essentially) found. The split occurred in all captive communities.

attempting to attain (or maintain) a specific goal (custody), and (2) a group of "captives" that comprise a communal organization, based on the family, highly institutionalized, but not oriented to the attainment of a specific goal.

This raises the point of whether the captive community is a total institution in Goffman's (1957, 1961) terms. It deviates significantly from total institutions in that the very presence of the family introduces a range of freedom that prohibits the control of the staff from even approaching "total." Because of this, captive communities cannot become "forcing houses for changing persons" (Goffman, 1957:48). In this sense, the Japanese internment center described by Vaughan (1949) is closer to a total institution, for there sexual contact was prohibited. The Japanese, however, were not especially concerned about "forcing" change, outside of maintaining custody and deferential behavior.

From the evidence submitted in this discussion, therefore, captive communities do not properly belong in the cell to which they are assigned in Chart 2. The formal organization does not "contain" the "captured" families; these families, if they are even permitted to operate as families, are carefully isolated from the decision-making bodies.

*Limited Communities.* If the initial hypothesis is correct, then the family should occupy an important role in communal organizations. The question here is, can groups be found which are communal organizations (according to the criteria in Chart 1) but which lack the family? If so, then the question follows, what are the consequences for social behavior and social structure?

There are at least two kinds of groups which fit the criterion of "limited communities"—monasteries (Hillery, 1969) and Shakers (Andrews, 1963).<sup>12</sup> The monasteries

<sup>12</sup> It is extremely difficult to find viable groups that fall into this classification. Monasteries are clearly pertinent, but one may argue that they are the only "successful" groups of their kind, having existed for many hundreds of years. The Shakers rank next in longevity. However, their long life span is quite misleading; they stopped generating new communities quite soon and have been declining for most of their history (see the discussion in the text). And, according to Kanter (1968), the Shakers are the longest lived "utopian" communities (she does not consider monasteries).

to be discussed in this paper are "Trappist" (Cistercians of the Strict Observance). Data have been collected on two.

For all practical purposes, the Shakers can also be called monastic. They differ from the Trappists, among other things, in that they are wholly Protestant in genesis and in that individual houses contain both sexes (although celibacy is maintained).

Shakers have existed for almost 200 years (since 1776). But no lasting community has been established since 1829, and thus Shaker communities stopped regenerating new colonies after 53 years of operation.<sup>13</sup> Many reasons could be given for their decline. Clearly important was the lack of a recruitment policy.<sup>14</sup> In addition, Shakers were not affiliated with any established church from which they could draw members.<sup>15</sup> Finally, although the point must remain hypothetical, they had an inadequate leadership structure. Each of these points is discussed.

The lack of a recruitment policy and of an affiliation with a larger body becomes a weakness, obviously, when a system abandons the family. The interesting point is that for some types of communities families can be omitted, for even if one argues that the Shakers were really not successful (and the point is admittedly moot), Trappist monasteries have existed for hundreds of years.<sup>16</sup> The Trappists, of course, have had the Roman Catholic Church as a system from which to draw recruits.

<sup>13</sup> There were only 18 Shakers left in 1967. The maximum population was one of approximately 6,000 persons in the 1850's (Andrews, 1963:224).

<sup>14</sup> They relied mainly on revivals held by other denominations and, later, they obtained recruits from orphans.

<sup>15</sup> Although originated by Quakers, the Shakers rapidly developed their own theology, based on the belief that their founder, Mother Ann Lee, was the second incarnation of Christ.

<sup>16</sup> The Cistercians originated in 1098 A.D. Both of the monasteries investigated by the author are more than 100 years old. Trappist monasteries may be regarded as communities insofar as they are not essentially different from villages and cities, other than adjustments arising from the omission of the family (Hillery, 1969). It is important to note here the qualification that is being introduced into the role of the family in the community. As the argument will develop in later paragraphs, the family has a high degree of probability of occurring in communal organizations, but in certain extreme cases, it is not necessary. For the older point of view, see Parsons, 1966:17.

There is another difference between Shakers and Trappists. With few exceptions, the communal organizations under study choose their own ruler. The exception is the Shakers, and they support the hypothesis introduced in the previous section, since their exception to the rule may actually explain their decay. One of the problems constantly with the Shakers was that of leadership. The practice was to assign leaders to the various communities, and the leaders were at times resented (Andrews, 1963:235). According to Andrews (1963:50), the last significant leader died in 1787. After 1792, the ministry withdrew from access to the people, living apart and working through other elders and deacons (Andrews, 1963:106, 149). Then the leadership began to falter in their efforts to socialize the orphans and the young whom the Shakers brought into their communities.<sup>17</sup>

The argument is not that a multitude of rules deprived the Shakers of freedom, for they too were free to come and go. Indeed, the Trappists have regulations as restrictive as the Shakers', especially in reference to diet.<sup>18</sup> The significant difference is that each local Trappist monastery chooses its own leader, the Abbot. The Shakers did not.

It is not certain that an imposed leadership structure was even in part responsible for the decline of the Shakers.<sup>19</sup> But when limited communities are compared with captive communities, and both are compared with villas, the element of freedom looms large. A monk, Shaker or Catholic, chooses a local house as well as a rule. Apparently, if an outside source imposes leadership on a local house, it introduces a significant deprivation of foreign origin, i.e., one not of the monk's own choosing.<sup>20</sup>

<sup>17</sup> "Adolescence was a period when Shaker youth often grew resentful of authority and impatient under the many orders restricting freedom" (Andrews, 1963:235.)

<sup>18</sup> Although Shakers also became vegetarians, they did not practice the strenuous fasts characteristic of the Trappists.

<sup>19</sup> One should also mention that the Shaker communities were designed as bisexual systems, but that women soon came to outnumber the men. This imbalance too may have been in part responsible for the decline.

<sup>20</sup> For an account of the emotional importance of the selection of a ruler, see the description of the selection of a Benedictine Abbess in the novel by Godden, 1969. This novel was recommended to the

In spite of the relative deficiencies of the Shakers, both they and the Trappists raise a third hypothesis: *Only if a group is not primarily oriented to the attainment of a specific goal will it maximize the freedom of its members.* Put in another way, relative to other groups (see Chart 1), freedom tends to be maximized in communal organizations.<sup>21</sup> This hypothesis is even accentuated by the comparisons in the research design (Chart 2). If one feature had to be used to separate a monk from a prisoner (to make the most extreme comparison), it would probably be that the monk entered his group because he chose to and is similarly free to leave.

Can it be said that the monasteries maximize the freedom of their members? Certainly, alternatives are curtailed: for example, the monks are celibate; they fast and maintain extensive periods of silence. But to talk with these men (and the Shakers—see especially Neal, 1963) is to be impressed with the lack of manifest antagonism. The point is that the monk is where he is because he has made what for him is the most significant choice of his life. Deprivations are an important part of this call, but they are deprivations chosen by him, and they can be renounced whenever he so wishes.<sup>22</sup> The

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writer by one of the Trappist monks as being an accurate and particularly sensitive account of monastic life, although, of course, it concerns a monastery of nuns. See also the position of Weingrod, 1966, on "administered" communities.

Further evidence for the importance of self-rule appears in the practice of commendam, in which revenues of a monastery were given to a secular official (such as a prince) who was Abbot in name only. The practice lasted mainly from the 14th to the 18th century, and where found monasteries declined not only in material goods but in number and population (Lekai, 1953).

<sup>21</sup> See especially Warren's (1970) concept of a non-Utopian normative model of community, which implicitly utilizes this concept. The argument in the text should also make clear that freedom also depends on some degree of institutionalization.

<sup>22</sup> For impressive personal accounts of the reaction of intensely "committed" persons to the bonds of imprisonment, see Bonhoeffer, 1967, and Delp, 1963, noting especially their acceptance of what they felt was the will of their God and their antagonism to the situation of confinement. Apparently, physical confinement—if it is not of the person's own choosing—is a basic deprivation of freedom. One might also contrast these with the autobiographical journal of the Trappist monk, Thomas Merton, 1953.

monastery is the place where he is free to follow his call to the maximum of his ability.

A more complete explanation of any lack of hostility should also include mechanisms for conflict resolution. One such mechanism is that of requiring intense dedication, and this is accomplished partly by the highly selective nature of the recruitment. Over 90% of all applicants to Trappist monasteries eventually leave before full profession. (This estimate was supplied by the monks.) They are in a sense rejects, whether they are rejecting the monastery or the monastery is rejecting them. This high degree of rejection may contain a form of conflict on the part of those who cannot make the sacrifice.

Another point follows from a study of limited communities. It is possible (although rare) to have systems that are communal organizations that do not have the family, but in such cases, a great deal of energy must be expended by someone to maintain the system, energy specifically in the form of recruitment. In other words, it is vastly easier to have communal organizations with the family than without it. Thus, communal organizations have a higher probability of containing families.

There may be another point: George Bernard Shaw has said that marriage provides the maximum of temptation with the maximum of opportunity. The point is particularly suggestive in the present context. In spite of the problems inherent in any family, a freedom of access of its members to each other is apparently so permitted that people will seek out such a form of behavior. To deprive a system of the family is thus to deprive it of a significant area of freedom. The difference in a monastery, of course, is that the monks are not deprived. They renounce.

*Intentional Communities.* These are communal organizations that have been intentionally founded for some purpose other than a specific goal. Limited communities are also intentional communities, but they have been separated (because of the absence of the family) for present research needs. The purpose or intention of the community is usually ideological, whether religious, economic, political or some combination. In the systems discussed, the Hutterite colonies

illustrate religious commitment; the Israeli kibbutzim have developed politicoeconomic justification. Emphasis is on the Hutterites.

Both the Hutterites and the Israelis modify the family in that they have shorn it of virtually all functions except two: (1) sanctioning the sexual union of the spouses and (2) providing some nurturances to the members (the children receive some initial socialization, but the community also provides a substantial—probably even a major—share of emotional nurturance to all members). Communal kitchens, nurseries, and economic systems operate separately from the family.

Conditions of freedom in intentional communities are comparable to limited communities. Although monasteries may have more emphasis on asceticism (or discipline), intentional communities—at least those considered here—apply many constraints to the individual in that life is closely planned. All of them, however, require that the individual make his own decision, as a young adult, whether he will enter the community. In the kibbutzim, the new member undergoes a year's probation, regardless of whether he has been reared in the kibbutz. For the Hutterites, a new member must ask for baptism. And though personal pressures may be great (which is a significant point in considering freedom), the choice is his. It is significant that most young kibbutz members and Hutterites choose to stay.

The freedom then that is offered to the members of these two intentional communities may be called a freedom of discipline. However, another aspect must be considered: most of the new members have been very effectively socialized—effective, that is, from the point of view of the system—since they stay. Thus, the available alternatives are those that have been shown to them by the system, those they have been socialized to accept. From this point of view there is less discipline involved in the choice. (See especially Bettelheim, 1969:216.)

The hypothesis concerning families and specific goals is actually not relevant to intentional communities, since only communal organizations are being considered (and thus, the variation *between* communal and formal organizations cannot be discussed in this context). Concerning the hypothesis re-

lating freedom and antagonism, however, there is some evidence. According to Hostetler and Huntington (1967), Hutterite women are much more antagonistic than men—they complain more and are more often at odds with the system. But women are openly considered inferior by the Hutterites and are treated as such, specifically by not giving them positions of responsibility or authority. We meet conditions reminiscent of the Shakers and their “administered” communities, and of captive communities. However, in view of the marital and familial ties uniting husband and wife, one cannot speak of a staff-inmate split. On the contrary, the high degree of cohesion among the Hutterites through the centuries has made them of special interest to students of social change (cf. Eaton, 1952).

Most of what has been said in these paragraphs is also pertinent to the third hypothesis; both intentional communities and limited communities maintain and protect the freedom of their members. That the freedoms seem more tightly structured than those to which we are accustomed is another matter. The members are free to choose.

*Total Institutions.* Attention is directed to those total institutions that seem to offer the greatest challenge to the hypotheses. The relation of prisons and mental institutions to communal organizations has been considered elsewhere (Hillery, 1963, 1968), and it was from a consideration of these that the first hypothesis was developed and that much of the background for the other two hypotheses was established. Major consideration in the present discussion is given to the English public schools, as treated by Weinberg (1967). These systems are total institutions in that (1) they exclude the schoolboy's family from the operation of their system; (2) they have the institutions of maintenance (recruitment, custody, treatment, and discharge—see Hillery, 1963, 1968); (3) they are “forcing houses” for changing persons; and (4) they sharply curtail the freedom of their clients (the schoolboys). They are also primarily oriented to the attainment of a specific goal, i.e., the production of members of the British elite. Given this last point and the fact that the schoolboy's family is excluded from the staff, the first hypothesis is upheld. The other two hypotheses are also

still tenable: freedom is less than in communal organizations, and the schoolboys show hostility and antagonism which can be related to their deprivation of freedom.

English public schools differ from classic total institutions (prisons and mental institutions) in several ways. (1) On an apparently minor level, religion and recreation are utilized in the development of ethnocentrism among the clients. A form of extreme loyalty is produced, carrying over into alumni associations, the “Old Boy clubs.” More important, (2) the members are socially and biologically immature humans, and thus normal sociological and psychological generalizations do not always apply. (3) The members are highly acceptable socially. They are neither threats to the society nor are they inherently incapable; in fact, they are children of the elite, to be trained as elite. (4) The clients are made a part of the staff through the prefect system, wherein senior boys are given substantial responsibility over their juniors. In brief, the English public schools are peculiar especially in that the clients are extremely ethnocentric, immature, nondeviant, and some are part of the staff. But they exhibit the classic aspects of total institutions in deprivation of freedom and family.

The most important of these differences seems to be in the absence of a staff-inmate split. The English public schools are truly borderline because they blur the split, giving relative freedom to some. Therefore, if the English public schools are total institutions as Weinberg maintains (and I believe correctly so), it follows that the staff-inmate split need not occur in total institutions.<sup>23</sup>

<sup>23</sup> This raises a rather chilling point (again, one that is probably fairly well recognized, but it needs emphasis): it is possible to socialize people to want to be captors of others. They will accept such a role as their right, as their choice, as their exercise of freedom.

Goffman (1961:116–119) does recognize that some total institutions may lack the staff-inmate split, a point on which I was not clear earlier (Hillery, 1969). However, Goffman, does not recognize that the absence of such a split may be a critical index that a system is not a total institution. That the English public school remains a total institution is deduced from the fact that there are still two populations, the controlled and the controlling. The staff-inmate split has become blurred but (unlike the situation in monasteries) the distinction persists.

Are the English public schools then communities? One need not pursue this question as a matter of definition. If intentional and limited communities are somehow designed to maintain and protect the freedom of their members, then this is not true of English public schools. These schools are designed to force new members to become part of a system. This is qualitatively different from the rearing of children in intentional communities—where the members are given a choice to stay within the system; and it is a real choice in that each is allowed to “fail,” according to his choice. The schoolboy, on the other hand, is in the system because his parents put him there, and there he stays until he graduates. Then he is out of the system.

The key again therefore hinges on choice and alternatives. There is also evidence to support the hypothesis that deprivation of freedom is conducive to antagonism. Boys do try to run away, they often become “restive” after returning from vacations, and although conservatism is the value to which they are socialized, there is a sizeable minority who express liberal or labor views in debates (often more than 40%). Nevertheless, although hostility is there, much of it appears to be successfully diverted in at least three ways: through recreation (and perhaps religion), through extending the promise that some of them will get a chance to control, and by the fact that the schoolboy knows that his “term” had a definite ending point.

*Conclusions.* The three hypotheses are summarized in Chart 3. Note that only the first hypothesis is clearly irreversible (Zetterberg, 1966): familial behavior is not permitted where specific goals are primary, but familial behavior may also *not* appear (as in monasteries) where specific goals are *not* primary. Whether the remaining hypotheses are irreversible cannot be determined from the data at hand. Not enough is known about organizational conflict to say that it is necessarily the reciprocal of freedom, although the data in this discussion distributes themselves as if that were so. Hypothesis 3, however, does seem irreversible; it is difficult to see how communities that have slave systems could maximize freedom. This apparently could only happen if the slave communities had more freedom relative to the culture in

which such communities were found. Until data are forthcoming, therefore, it is best to suspend judgment. Accordingly, Hypothesis 3 cannot be said to be substitutable for either Hypothesis 1 or 2.

It is important to recall that Hypothesis 1, as illustrated in Chart 3, refutes the illustration given in Chart 2: Captive communities do not “contain” the family, in the sense of permitting familial behavior to determine the existence of the group. The presence of the family in captive communities—whether these be relocation centers, slave plantations, or concentration camps—is something which exists in spite of the system, not because of it. The family is tolerated, if at all; the latter possibility shown progressively by the treatment the family receives in slave plantations (where it is systematically disrupted) and in concentration camps (where the members are permitted to exist within the camp, but where certain crucial functions of the family are prohibited, such as sexual intercourse).

The question remains, to how many other groups can the hypotheses be applied? If we consider only the third hypothesis, application would seem on the surface to be quite extensive. Formal organizations appear to be especially limited in developing freedom, in that formal organizations are concerned only with part of the human being. They are primarily oriented to the attainment of a specific goal, and as such cannot be concerned with *maximizing* freedom. The very emphasis on rationality and specialization in formal organizations appears to preclude this.<sup>24</sup>

Even more relevant, the third hypothesis has been developed from total institutions and related systems (captive communities) which care for a larger share of life than other formal organizations. In the same vein, expressive and informal groups (see Chart 1) seem to sustain the hypothesis in that they cannot maximize freedom since they are less institutionalized and hence are more ephemeral. Only communal organizations are suf-

<sup>24</sup> If only in the sense that freedom is more than a rational and scientific concept, as noted earlier (see footnote 8).

Note further that the hypothesis does not say that members cannot be free in formal organizations—only that formal organizations do not *maximize* freedom relative to communal organizations.

## Chart 3. Summary and Illustration of the Hypotheses.

Hypothesis 1: If a group is primarily oriented to the attainment of a specific goal, then it excludes familial behavior.

## Familial Behavior Permitted

Yes

No

Specific Goal:

Primary

Not Primary

*	Captive Communities Total Institutions
Intentional Communities Limited Communities	Limited Communities

Hypothesis 2: The greater the deprivation of freedom, the greater the antagonism of those deprived.

## Freedom

Granted

Deprived

Antagonism:

High

Low

?	Captive Communities Total Institutions
Intentional Communities Limited Communities	*

Hypothesis 3: Only if a group is not primarily oriented to the attainment of a specific goal will it maximize the freedom of its members.

## Freedom

Maximum

Minimum

Specific Goal:

Primary

Not Primary

*	Captive Communities Total Institutions
Intentional Communities Limited Communities	?

\* = No groups appear or expected in this cell.

? = No groups appear but theoretical status uncertain.

ficiently institutionalized to provide the necessary stability on the one hand, and, on the other hand, are not limited by commitments to output goals that preclude the freedom of the individual to choose. This is not to say that all communal organizations provide the maximum amount of freedom—only that communal organizations provide maximum freedom when compared to other kinds of human groups.

Some final remarks are warranted concerning the *sociology* of freedom. One of the most intriguing aspects of this investigation is that although freedom is undeniably dependent on the definition of the situation, it appears to occur more in some social structures than in others. It is probably true that any type of stress perpetrated on the human organism can be defined as a type of freedom by that organism. The martyr is



seemingly the confirming case: persons who willingly submit to torture and death because they choose such an alternative over others. Thus we may say that freedom depends first on the definition of the situation.<sup>25</sup>

Nevertheless, although the definition of the situation is necessary, it is not sufficient. It appears to be the *organization* of the group that prompts the definition of the situation as much if not more than vice versa. The histories of slave plantations, relocation centers, and concentration camps, in particular, indicate that men will attempt to change the structure of these systems in such a manner that freedom as they define it can be more fully exercised.

Similarly with total institutions—few persons want to stay within them. That such a system may be accepted as a temporary situation is shown by the English public schools. However, the schoolboy does not remain a schoolboy forever, and he displays significant antagonism while he is within the school.

What then is the difference between structures that make for freedom and those that do not? From this preliminary perspective, commitment to a specific task, above and beyond the needs of the individuals within the system, is the pivotal negating factor. Anything which places people first seems to increase freedom, as the people will define it. Stated in so simple a manner, the statement appears almost obvious, and this principle echoes throughout the Judaeo-Christian heritage. The discovery here, if it may be called that, is one of indicating what to look for in the social structure.

There is, however, another structural feature that is important: freedom seems somehow associated with the family. Systems may be free and dispense with the family (as in monasteries), and they may "capture" the family and curtail freedom (as in some concentration camps), but in both cases, such modifications are attained only with considerable difficulty. Why this is so is not clear, except that the family generally places people first.

The family accordingly is a very basic group in communal organizations and is anti-

thetical to formal organizations.<sup>26</sup> On the other hand, the act of committing the system totally to specific goals is equally basic to formal organizations and is seldom found as the primary purpose of communal organizations. Apparently, therefore, the difference between formal and communal organizations is one of the few sociological distinctions that is not on a continuum, that is of the variety of either-or, a set of discrete attributes. When the sharpness of this dichotomy is linked with the relation to freedom that has been outlined, a new perspective is given to the dedication of so many to such concepts as democracy, liberty, independence, and emancipation. Freedom has meaningful roots at many points in human behavior.

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<sup>26</sup> Two factors are involved: (1) the family includes children and a significant number of children cannot make meaningful individual decisions—even more cannot make meaningful group decisions. (2) The family is based on emotional decisions which are antithetical to the rationality that is the manifest *modus operandi* of formal organizations, especially bureaucracies.

Extensive supportive evidence of the antithetical relation between communal (especially familial) and formal organizations is found in Barton (1969). Barton shows that wherever formal and familial roles come into conflict, as in disaster situations, the community members tend to resolve their behavior in favor of one or the other—not both (see especially pp. 154 ff). Often there is a symbiotic relationship; and often, where the formal organization cannot compete with the family but must still survive, it will allow or tolerate the family (p. 155, paragraph d).

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## DEFINITION AND MEASUREMENT OF CONTINUOUS VARIATION IN ECOLOGICAL ANALYSIS \*

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*American Sociological Review* 1971, Vol. 36 (February):65-74

*Ecological studies have typically employed a frame of reference in which observed phenomena are categorized in terms of bounded territorial units. This has resulted in a number of problems, among them the placement of boundaries, homogeneity within units, comparability between units and the restriction of statistical techniques to those appropriate for sets of nominal categories. It is suggested here that because many social characteristics may vary continuously across the community, the isopleth map may provide a method of relating these to the territory which is more congruent with their actual rates of variation and which retains a higher level of measurement for the territorial factor. This study examines the extent to which such a map represents a valid image of residential status of areas other than those specifically employed in its construction. The extent to which the isopleth map is congruent with the image of the community held by its residents, and its usefulness in testing research hypothesis are also examined.*

THE problem of relating attributes or dimensions of a social system to the territory which that social system occupies is a significant one whether human ecology is defined broadly as embracing the total exchange relationship between man and his environment, or in its narrowest sense as the study of the areal distribution of social characteristics in human communities. One dimension that can be treated profitably in the latter sense concerns the ecological distribution of social status within a given community setting.

Persons of similar status, by implication, engage in roughly similar behavior patterns which in turn are consistent with and to a large degree dependent upon their parallel life styles (and/or chances). This pattern manifests itself in many diverse settings, only one of which is residential behavior.

Persons of similar status, according to this argument, tend to select residences near one another in physical space which results in a spatial clustering of places of residence by those of similar social rank (Hawley and Duncan, 1957).

Further, these similarities in residential selection patterns augmented by an attendant gross similarity in possessions will tend to become symbolically characteristic of the status levels in which they occur. Therefore, residential areas will tend to have symbolic meanings which are associated with the other status characteristics of their inhabitants. In this way, status characteristics can be associated with residential areas even though particular residents may deviate from what is characteristic of any given area.

With the exception of the unlikely case where a given area is completely homogeneous, there will tend to be some local variation in the status levels of a dwelling area.

\* The data presented here are also utilized in Hoiberg's Masters Thesis (Hoiberg, 1969).

This raises the possibility that there will exist a relatively continuous variation in both status composition and symbolic reputation as one moves across a residential area. In this way, continuous variation in the status of dwelling areas can occur without violating the principle of residential clustering by status as outlined above.

One striking characteristic of both theoretical and empirical approaches to the relationship has been the persistent tendency to deal with space or territory in terms of bounded units. This tendency has produced a number of problems—problems of unit size, boundary location, questions of internal homogeneity (Myers, 1953) and comparability between units, and questions of the appropriateness of a given boundary system for the study of different sets of variables. Cartwright (1969), in a recent review of the current state of ecology, further points out that bounded units lack flexibility; if a particular boundary system is held constant, it may become obsolete and the units lose their original homogeneity, while if the boundaries are adjusted to community changes, the comparability of units over time is destroyed (Kitagawa and Taeuber, 1963). Finally, the construction of a system of bounded units almost always involves some kind of trade-off if more than a single criterion is employed. Units cannot generally be uniform in both area and population. Internal homogeneity with respect to one variable must often be sacrificed to homogeneity in another. These difficulties seem to arise from two major sources: the attempt to comprehend a multiplicity of variables by means of a single boundary system, and the tendency for bounded units to represent changes as occurring at arbitrary boundary lines when in fact they may have taken place elsewhere or be continuous across the entire territory. It follows that these difficulties should be avoidable by methods which relate one social dimension to the territory at a time, and which relate this directly, without the intervention of bounded territorial units.

The isoline map (for example, see Figure 1), in which lines are drawn connecting locations at which equal values occur within the range of a variable being related to the territory, suggests itself as a technique which meets the foregoing requirements. Such a

map not only provides a visual image of the way in which a variable is distributed across the territory, but is also a mathematical construction from which it should be possible to derive measures of those variables which have been systematically entered into it.

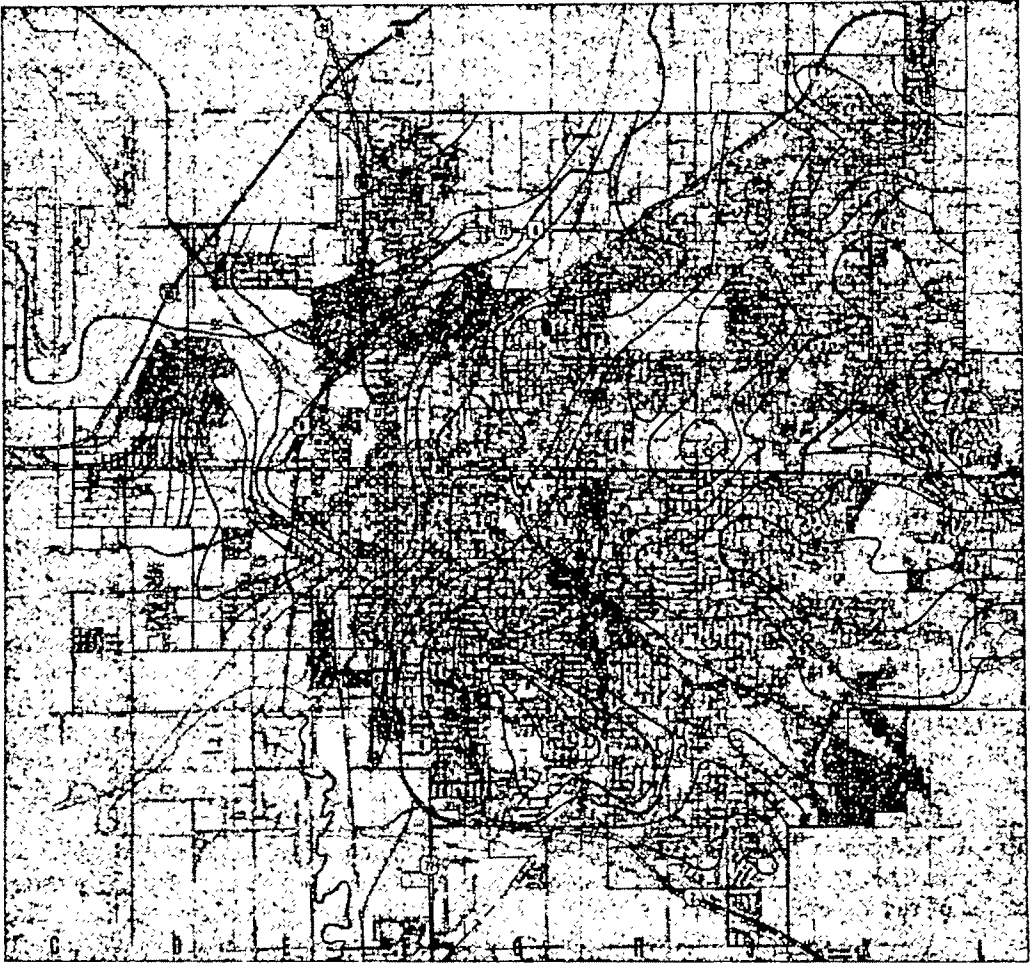
The essential formal difference between those approaches which relate other variables to territory by means of bounded units and those which make use of isolines lies in the level of measurement employed on the territorial side of the relationship. When the territorial factor is treated as a set of bounded units, it becomes, in effect, a set of nominal categories, which makes it necessary to aggregate measures of whatever variable is being related to the territory within these units. In the isoline map, on the other hand, the territorial factor is expressed in terms of a ratio scale, which does not aggregate locations but retains their specific spatial identities and interrelationships. This difference has a number of implications.

First, if observations are to be aggregated within bounded units, their specific spatial relationships are lost, and only complete enumerations or random samples within such units can be adequately representative of them. The observations on which an isoline map is based are necessarily representative of the points at which they are taken, and since their interrelationships are not lost, the principle that each element in the universe must have a known probability of inclusion in the sample is not violated by departures from randomness in their selection.

Second, the level of measurement adopted for the territorial factor influences the selection of appropriate inductive statistics in investigating relationships between this and other variables. If the territory is represented by a set of nominal units, the choice of techniques will be limited by this fact, while if ratio-scale measurement of the territory is retained, limitations on the strategies available for investigating relationships between the territory and a social variable will be due to the level of measurement obtained for the social variable.

Third, the bounded unit approach focuses upon representing the state of a variable within a given area as represented by a summarizing measure, while the isopleth approach assumes that the differences between

FIGURE 1. ISOLINE MAP



points in a series of measured locations represent the rates of change occurring across the territory. The value for a given location is to be estimated from its exact spatial relationship to measured locations rather than from its inclusion within a bounded unit to which a single value has been applied. Bounded units represent the distribution of states of a variable, while the isoline map represents the distribution of variation, though particular states of a variable in particular locations can be inferred from it, in at least some types of isoline maps.

The lines which represent a variable that has been measured directly and from which specific values for particular locations can be directly inferred are sometimes distinguished by the use of such terms as isometers or isarithms (Duncan, *et al.*, 1961; Raisz, 1938), while lines which refer to ratios or

indices are generally called isopleths, although some authors use the latter term for both types of lines. The distinction is based upon the principle that a person standing on a point corresponding to the 50-foot interval on a land contour map can assume that the ground under his feet has an elevation of 50 feet, while the person standing on a point corresponding, for example, to the 50% isopleth on a map of percent of land under cultivation cannot assume that 50% of the ground under his feet will be under cultivation; he can, however, expect to be located in an area in which it is usual for half the land to be cultivated. Similarly, of course, the sharp transition shown on a map of percent of land under cultivation where an irrigated area meets a nonirrigated area in a desert will be only analogous to the cliff which would be represented by the same pat-

tern of lines on a land contour map. Both maps, however, will make operationally derived statements about rates of change over the territory, and both will be subject to operational verification.

The social variables to which isoline mapping can be applied are those which can be conceptualized as meaningfully related to territory and measured on at least an ordinal level. Socioeconomic status, for example, can be objectified in one sense only by individuals or groups. On the other hand, neighborhood or residential area status is an environmental attribute. It is a feature of the surroundings within which, not only inhabitants, but parks, schools, churches, businesses and streets may be found. The present study concerns itself with the use of the isoline map as a method of measuring and representing variation in residential area status across a community, and therefore reports tests of the hypotheses that: (1) Continuous variation in the status characteristics of neighborhoods can be represented by means of an objective status contour map. Specifically, once the map is constructed, those subjects who deviate from it will deviate in a comparable manner from their immediate neighbors. (2) Respondents will be able to assign symbolic status meanings to the community's subareas, which will be comparable to the objective occupational status of the subareas' inhabitants.

#### CONSTRUCTION OF THE MAPS

The social character of a given location should be measurable in terms of an open cluster of societal units surrounding that point. Thus, if we obtain a measure from a number of persons, households, dwelling units, etc., as appropriate, surrounding a designated point, and assign the central tendency of this cluster to the designated point, what we have is a measure of the social environment of that location. If a sufficient number of points are measured in this manner and a system of contour lines is plotted to connect points of equal value interpolated from the measured locations, the product will be an image of continuous variation of social status over the area.

Therefore, in the construction of the objective status contour map, the first step was to designate an array of observation points at which measurements were to be taken.

The process used in this case consisted of constructing a gridwork over a map of a Midwestern city with a population of about 150,000, which divided it into 64 equal rectangles, each about a 16-block square. These areas were further subdivided into fourths, producing 256 equal rectangular areas, which covered the entire city map. When those areas which fell outside the city limits and those areas where no residential activity was evident had been deleted, 154 of these areas remained in the analysis. The geometric centers of these areas were designated as points of observation or measurement.

Clusters were designated which consisted of the three residences located closest to the established reference point. This is a very small cluster, but we wanted to expose the method to some risks in this respect. Some of the designated areas were only partially occupied, and in these cases the number of blocks within the area were enumerated, and the block, as well as the three residences within the block, were selected by a table of random numbers. Measurements in these cases were, of course, entered on the map at the point where they occurred, rather than at the center point of the area in which they fell.

Once points of reference were established, socioeconomic status scores for the clusters surrounding these points were obtained. This was done by simply consulting the city directory for the occupation of the head of the household for each of the selected residences. Each occupation was given a score through the use of the NORC transform of the Duncan SES scale (Duncan, 1961), which is based upon the income, educational level, and relative prestige ratings of occupational titles. For each point of reference then, socioeconomic status was expressed as the average of the occupational scores of a cluster of three residences.

The final task was to construct a series of isopleths or, more specifically, status contour lines connecting points of equal value as interpolated from those measured and entered on the map. Scores obtained for the various points ranged from a low of 44 to a high of 88. Status contour lines were plotted at five-point intervals interpolated on the basis of the locations of the scores for each of the previously established reference points. For

example, if one reference point had a score of 46 and a contiguous point had a score of 54, the location of the 50-point contour interval was determined by plotting the scores of 46 and 54 on a sheet of graph paper and drawing a straight line between the two points. This simple graphing procedure determines the point at which the line so drawn intersects the 50-point line on the graph paper and in turn its location relative to the other two scores. This information is then transferred to the map, and the operation is repeated for each set of contiguous scores which have an intervening contour interval. These interpolated points are the ones through which status contour lines can be expected to pass, and their locations are in each case affected by the scores occurring on all sides of them. Isopleths or status contour lines are then drawn in. This operation requires a small amount of skill and experience. Its basic principles are: (1) no two lines should meet or cross, but each contour line should either be a closed polygon on the map or run off the map at both ends, and (2) the series of contours lying between any two points on the map should be continuous (Raisz, 1938).

The construction of another map was undertaken to determine the relative prestige value of the ecological areas of the community or, in other words, the symbolic conception of the status structure of the community by a sample of its residents.

The sample of residents chosen to be respondents for an interview were selected on the basis of a simple random sampling procedure. The population from which the sample was drawn was defined as all persons listed in the city directory, a relatively complete listing of residences in the community. Given the purposes of the study, we were only interested in fairly strong relationships between variables. Thus, a random sample of 50 persons, which would yield satisfactory significance levels for such relationships, was drawn from the population. If the primary respondent was not at home or refused to be interviewed, the residence immediately next to it was selected.

Lazerwitz (1964) used a somewhat similar method when he clustered addresses from the city directory, from which he averaged at least one interview per cluster after the

various "non-response" factors were encountered. In determining the degree of departure of the "clustering" method from simple random sampling, he found that the variance of the "clustered" sample was on an average only about 1.20 times larger than its simple random sampling variance. Further, the task at hand was the determination of the conception of the status structure as it was conceived by a heterogeneously located group of residents within the city, a task which was not interfered with by the use of this procedure.

Following the selection of the sample, an interview requiring from 15-30 minutes to complete was administered to each of the respondents. The way in which the respondents' symbolic conception of the spatial distribution of status was determined consisted of first, providing him with a map of the city with the previously established gridwork of 64 equal areas superimposed on it. He was then asked, for each of twenty occupations listed, to give the number of the area on the map where he thought that a person with that occupation might reside. The occupations were selected on the basis of their heterogeneity as a group, both in the general type of work that they do and in their relative prestige value.

For each of the 64 areas a tally was made on the various occupational allocations received. From this a mean NORC transform SES score was determined for each area and assigned to the geometric center of the area as a basis for interpolating contour lines or isopleths in the manner described above. The result was a map portraying the subjective conception of the spatial distribution of socioeconomic status in the community held by a sample of fifty of its citizens. This subjective map is a more generalized and less detailed portrayal of the status structure of the community, which is at least partly a smoothing effect due to the use of a larger gridwork in this part of the analysis.

#### TESTS OF THE HYPOTHESES

Our first hypothesis holds essentially that the map can provide a valid representation of the status characteristics of locations in the community other than those locations from which data for the construction of the

map were derived. The map does not pretend to represent the status characteristics of particular subjects, but only of their immediate residential surroundings. These two variables have ordinarily not been found related to an extent producing correlations greater than about .5 (Miller, 1964:101). However, the SES scores of fifty respondents interviewed in connection with another aspect of the study were correlated with the score of the status contour within which the respondent lived. A Pearson  $r$  of .35 ( $P < .01$ ) was found. The same procedure was utilized for an alternate random sample of residents of the community and a Pearson  $r$  of .46 ( $P < .01$ ) was obtained. These findings are comparable to Kahl and Davis' (1955) report of a tetrachoric  $r$  of .46 between two indexes which are roughly comparable to ours, the North-Hatt occupational category and the census tract mean monthly rent. This comparability is brought out when our two sets of data, dichotomized at the same point as Kahl and Davis', are transformed into tetrachoric  $r$ 's, the resultant correlations of .42 ( $P < .01$ ) and .61 ( $P < .01$ ), respectively, being favorably comparable to their reported correlation of .46.

Thus while some relationship exists between the occupational statuses of these subjects and the status characteristics of their locations as represented by the map, and although the magnitude of the relationship is not out of context with what is usually found to obtain between the status characteristics of individuals and their residential areas, the correlation found is certainly not high. The crucial question for the validity of the map is whether these subjects' individual deviations from the scores found for their residential locations on the map represent deviations from the general status characteristics of their surroundings or simply inaccuracy in the map. This question can be answered by comparing the subjects' deviations from the map with their deviations from the status characteristics of persons living nearby. To test the hypothesis that those subjects who deviated from the map would also deviate in a comparable manner from their neighbors required us to ascertain the occupational scores of the three neighbors on either side of the subject, and from this cluster of six to obtain an average SES score for the neigh-

bors at that point. When this was done it was found that 83% of the sample either fit in terms of both their neighbors and the map or differed from their neighbors in the same direction that they differed from the map. Further, for the latter group, there was no significant difference ( $t = 1.124$ ,  $P < .30$ ) between the amount of deviation from their neighbors and the amount of deviation from the map. The 50 subjects' deviations from the map were also correlated with their deviations from the average status scores of their six neighbors, yielding an  $r$  of .73 ( $P < .01$ ). In sum, the hypothesis that the status contour method will yield accurate results in the prediction of the status characteristics of various locations in the community seems to be borne out by the data.

To test the hypothesis that individuals will be able to symbolically perceive the status structure of the community, and that this symbolic perception will be comparable to the objective status structure of the community, required us to measure the degree of correspondence between the symbolic status contour map and the objective status contour map. In order to do this, we designated 50 random points, and for each of these points SES scores were obtained from the objective and symbolic maps. The two sets of scores were then correlated and a Pearson  $r$  of .50 ( $P < .05$ ) was obtained. This permits us to accept our hypothesis and indicates a fair degree of relationship between the subjective image of the status reputations of various areas of the city held by its citizens and the objective variation of SES as indicated by the persons actually residing in those areas. Again, comparing our results with those reported by Kahl and Davis (1955), the tetrachoric  $r$  between the subjective map and the objective map was .88, which compares very favorably with their reported correlation of .76 between interviewers' rating of residential area (subjective index) and census tract mean monthly rent (objective index). This, in turn, lends support to the idea that status contour analysis does lead to a portrayal of the relationship between territory and socio-economic status which is congruent with the manner in which this relationship is perceived by residents of the community. Subjects were selected randomly and not for their knowledge of the community. They



were asked, not to rate the status characteristics of particular locations but merely to suggest where persons of a variety of occupations might reside. It is interesting to note that a subjective map based upon the aggregate of these responses corresponds as closely as it does with one based upon objective data. The correlation is as high as can usually be expected between objective and subjective measures of the same variable (Stinchcombe and Ofshe, 1969:116; Caplow, 1954:216-217).

Another method, and perhaps a better one in evaluating the symbolic map's general validity, is to note its performance as compared to the objective map in its relation to some third variable. Lazarsfeld (1959:65) states: "It is not the relation of the indices to one another which is the crucial problem, but their relation to outside variables." Accordingly, two "outside" variables were chosen, not so much for the substantive findings involved as for the sake of testing the comparability of the two maps in treating outside variables. The first treats residential status as a dependent variable in relation to land elevation, while the second treats residential status as an independent variable in relation to friendship choice.

An interesting question which emanates from the utilization of contours in depicting social status is the relationship between the status contours so derived and the actual geographical contours depicting the topography of the land. Hoyt (1939) contended that city areas at higher altitudes frequently were characterized by high rents because they were free from slides and floods, and thus became fashionable and desirable residential districts. Harris and Ullman (1945) also point out this relationship between high land and high social status when they postulated that, in general, high class residential zones would be on well-drained high land with a view and away from the noise, smoke, and odor of industry. Therefore, high physical altitude seems to be associated with high status in many instances. However, in those cases where the pragmatic effects of floods and slides are not relevant, height still seems to retain its relation to status through the symbolic cultural definition given to the entire concept of height. Such a symbolic meaning can be crudely displayed through the

popularity and meaning of such sayings as "top drawer," "high man on the totem pole," "top dog," etc. An interesting empirical question concerns the existence of a relationship between status and physical elevation.

To test this relationship, we utilized again the alternate gridwork that had been used in testing a previous hypothesis to establish 50 points at which both topographical measures of elevation and status measures could be obtained. The status measures were taken from both the objective and subjective status contour maps, while the geographical elevation measures were secured from the United States Geologic Survey's contour map of the City's area. Although there is relatively little elevation differential in this community, there does exist a range of some 210 feet, which, when broken down into 20-foot intervals, does offer a meaningful gradient of elevation with which the status scores can be correlated.

The resultant correlation between status and elevation when using the objective map was .73 ( $P < .005$ ). When the subjective map was used, the coefficient of correlation was raised to .78 ( $P < .005$ ). Therefore, not only does there seem to be a significant positive correlation between the status score of an area and the physical elevation of that area, but the objective and subjective maps demonstrate essentially parallel performance in demonstrating the strength of the relationship.

The performance of the objective and subjective methods of portraying residential status as an independent variable was examined in relation to friendship choice. That friendship runs essentially along social class lines is a well demonstrated empirical generalization in sociology today. Hollingshead, (1949), for example, demonstrated that clique formation, dating, and marriage all essentially followed social class lines. More recently Laumann (1966) found that persons in all occupational classes disproportionately found their friends in the same occupational class as themselves. Finally, Warner's (1949) studies tend to use associational data as the basis for determining the class position of an individual. The Warner Index for Evaluated Participation (EP) identifies the social class group within which a person participates. This index of participation was found to be

predictable through the use of the Index of Status Characteristics (ISC), which was based upon such objective class determinants as occupation, source of income, and dwelling area. Thus, a high correlation between (1) objective social class criteria, as measured by the ISC, and (2) the associations that a person had, as measured by EP, was demonstrated.

On the basis of such findings as these, we can hypothesize that the SES levels of respondents' locations in the community and those of friends' designated by these respondents will correlate positively. To obtain this correlation we asked the fifty residents who comprised the sample to determine their three closest friends and to note on the map the area within which each of them resided. To seek a more specific location than this was thought to be unwise as it might appear as an encroachment upon the respondents' privacy.

With this data in hand, a sociogram was constructed on both the subjective and the objective maps which connected the respondent, whose exact location was known, with each of his three friends, whose general location was arbitrarily plotted as the center point of the area where they were reported as living. On the basis of this sociometric overlay on the status contour maps, status contour intervals of respondents' locations and the locations of their chosen friends were determined. Obviously, the method used to identify friends' locations sacrifices some information.

The procedure just discussed was carried out for both the objective and subjective maps. The coefficient of correlation which was obtained in determining the association between the objective status contour interval of the respondent and the objective status contour intervals of his friends was .59 ( $P < .005$ ). An examination of the correlation for the subjective status scores reveals an even higher coefficient of .69 ( $P < .005$ ). It should be noted at this point that, although each subject made more than one choice of a friend, the levels of significance were calculated on the basis of the number of different subjects making the choices.

Therefore, whatever the actual relationship between socioeconomic status and friendship choice, it has been demonstrated

that the status of a respondent's dwelling area, as measured by "status contour analysis," is highly related to the status of his friends' dwelling area, as measured by the same technique, in either its subjective or objective form.

A question which naturally comes to mind at this point is whether, or to what extent, the findings just described are an artifact produced by a number of respondents simply selecting their next-door neighbors, who by virtual definition of the method would share the same environmental SES level. One of the difficulties dealing with this question lies in the fact that such an occurrence would be completely in keeping with the theoretical basis of the hypothesis we have been testing. Persons of similar SES are expected both to seek similar locations and to choose each other as friends. Moreover, if we attempt to eliminate propinquity as a factor in friendship choice by excluding nearby choices, we will artificially retain all cases contrary to the hypothesis, since the friend must live at some distance from the respondent in order to occupy a different socioeconomic neighborhood. Despite these biases, the attempt must be made, because the crucial test is whether those friends who do not live nearby are to be found at similar socioeconomic environmental locations.

Using the grid of mile-square areas on which friendship choices were originally entered, those choices which fell within the same square as the respondents' residence were eliminated. This resulted in the deletion of about one-third of the total friendship choices, eliminating all of those choices which fell within one-half mile of the respondents' location and nearly all of those which fell within three-quarters of a mile. In that part of the data which remained, the correlation between respondents' status locations and those of their chosen friends was reduced to .36 ( $P < .01$ ) on the objective status contour map and .53 ( $P < .005$ ) on the subjective status contour map. Thus, while the hypothetical relationship between locations of respondents and chosen friends received less support when only physically distant friends are studied, it remains impressive when the biasing factors mentioned above are taken into account.

It may be useful to compare the perfor-

mances of continuous and bounded unit analysis in handling the same basic data. This can be done by recalculating some of the correlations reported above using the network of 154 rectangular areas from which our original observation points were taken and comparing them with those obtained through the status contour method. This should provide adequate risk for the contour method, because the bounded units are small, about one-fourth square mile each, and the data used to assign status scores to the units are identical with those from which the isoline map was derived.

When the relationship between the status location of the respondent and the status locations of his three friends was recomputed, using the score assigned their respective bounded units, a Pearson  $r$  of .41 was obtained, compared to the previously reported correlation of .59, using the contour method. This raises the question of whether the higher correlation represents better performance in revealing an existing relationship or an exaggeration of that relationship. The latter possibility could occur if the contour method tends to provide a less exact but potentially smoother estimate of status variation than small bounded units are likely to do. Since the referent in either approach is the status characteristic of the surroundings at any residential location, we combined each respondent's occupational score with those of his six nearest neighbors to give an average for those immediate surroundings in attempting to answer this question. We found that this score correlated .25 with scores derived from the rectangular units and .50 with those derived from the isoline map. On this basis, it would seem that at least in this community, the idea of continuous variation of socioeconomic status over the territory gives closer estimates—both of the status characteristics associated with particular locations and of the relationship between residential status and friendship formation—than does the alternate conception of status occurring within homogeneous units and being separated by sharp breaks and jumps.

None of our findings preclude the existence of "natural areas" or homogeneous areas enclosed by natural, man made, or symbolic boundaries. Most communities, indeed, have instances of both types of variation. It seems

difficult even to state, in answerable form, the question of which tends to predominate. The point is simply that residential status may be continuous, which means that the status gradient can vary from low to high.

While bounded units emphasize the general status characteristics of specified areas within the community, isolines are capable, further, of presenting an image of status change as one moves across the community, as well as of variation in rates of change from one part of the community to another. This would seem to suggest that the latter method has advantages over the former, provided first, that variation of neighborhood status is at least potentially continuous and second, that an isoline map can represent the status characteristics of residential areas in a valid way. Status contour analysis does appear to succeed in representing the relationship between the territory of the community and one social system variable, socioeconomic status, in a manner compatible with the way in which the community is perceived by its inhabitants. This, in turn, would seem to warrant further use of methods in which variation of social characteristics over territory is represented by contour lines or isopleths, indicating the locations at which stated intervals in their range of variation tend to be found.

The approach we have suggested here tends to be in opposition to the use of artificial boundary systems, but not to the concept of the natural area, since it is equipped to show both gradual changes and the rapid changes which may be associated with the occurrence of "natural" boundaries. As Hatt (1946:427) pointed out long ago, "no obeisance need be made to the natural areas of the city, but only those natural areas logically determined by the data, and the problem need be constructed, used and defended."

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## SIGNIFICANT OTHERS, THE SELF-REFLEXIVE ACT AND THE ATTITUDE FORMATION PROCESS\*

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*American Sociological Review* 1971, Vol. 36 (February):74-87

*Based on a new formulation of attitude formation theory, new instruments (The Wisconsin Significant Other Battery) are utilized to measure the influence of significant others over the educational and occupational aspirations of 100 high school seniors. These new variables are combined with other variables of known effect into a hypothetical model of the process whereby educational and occupational aspirations are set. Because of the partially nonrecursive nature of the proposed model, statistical difficulties involved in its solution are discussed. In spite of these difficulties, the new variables introduced result in more satisfactory explanations of aspiration attitudes than those reported previously.*

THE importance of "others" as mediators of culture has long been acknowledged in sociology, and the influence of other persons and groups in the formation of atti-

tudes, values, self-conceptions and other psychological structures is central to much of

\*The research reported here was supported by the U.S. Office of Education, by the Cooperative State Research Service and the University's College of Agriculture for North Central Regional Research Committee NC-86, by funds to the Institute for Research on Poverty at the University of Wisconsin, provided by the Office of Economic Opportunity

pursuant to the provisions of the Economic Opportunity Act of 1964, by the Graduate School of the University of Wisconsin, the Graduate Research Committee of the University of Illinois, and the Foundation for Social Research, Inc. The writers are grateful to Edward L. Fink for assistance in all phases of the research, particularly the statistical analysis, and to Helcio A. Saraiva for computer programming assistance.

sociological theory (Katz and Lazarsfeld, 1955). Recent empirical studies, particularly in the area of educational and occupational attainment, have lent fresh support to this notion (Haller and Sewell, 1967; Alexander and Campbell, 1964; Haller and Butterworth, 1960; Sewell and Shah, 1968) and, more importantly, have made some progress toward an assessment of the relative *quantity* of influence attributable to "significant others" as opposed to other, noninterpersonal, sources (Sewell *et al.*, 1969; Duncan *et al.*, 1968). For example, Sewell, Haller, and Portes report a zero-order correlation of .59 between the expectation of a pre-selected panel of significant others (parents, teachers, and peers) and the educational aspirations of Wisconsin farm boys in their senior year of high school, and present evidence which suggests that the influence of SES on educational aspirations (and later attainments) is almost wholly mediated by the expectations of significant others (Sewell *et al.*, 1969). This finding has been confirmed for several other types of samples (Sewell *et al.*, 1970).

#### THE PROBLEM

In spite of the great progress that has been made, these recent studies have all used measures of significant other influence which are in one or more ways unsatisfactory (Haller and Woelfel, 1969, Chapter 2). Generally, these studies have either measured the individual's statement of his perceptions of the influence others have had on him (Haller and Sewell, 1967; Sewell, *et al.*, 1969; Kemper, 1963) or have dealt with a limited panel of pre-selected significant others (Haller and Butterworth, 1960; Duncan *et al.*, 1968).

One of the main reasons for the inadequacy of previous research instrumentation has been the lack of a close connection between the measures themselves and attitude formation theory. Thus, for example, Sewell, Haller, and Portes reason that parents, friends, and teachers should be influential on intuitive as well as theoretical grounds (Sewell *et al.*, 1969), and for the same kinds of reasons, Kemper selects wife, boss, colleague, and father for his panel of significant others (Kemper, 1963). In no instance has

a study (1) detected the exact significant others of a sample of individuals with an instrument of known validity and reliability, (2) measured the expectations of those others for the individuals in question, and (3) compared the effect of the expectation of others with other variables of known effect on the attitudes of individuals. It is to those ends that the present study is directed.

#### THEORY

We start with the working assumption that significant others are attitude specific. If it should turn out that some others transmit influence over many attitudes, such persons will be discoverable by accumulation of spheres of influence. If we assume, however, that others are general and pervasive in the sense of Kuhn's "Orientational Other" (Kuhn, 1964), we will miss those others whose influence, although great, may be only segmental. As a working definition, then, we assume that *significant others are those persons who exercise major influence over the attitudes of individuals.*

The theory basic to this research assumes that attitudes are relationships between a person and an object or set of objects (Green, 1954; DeFluer and Westie, 1963). But following from the interactionist postulate that man's perception of objects is always mediated by some symbolic structure (Kuhn, 1964), that relationship is assumed to be a conceptual one, that is, it is the relationship a person sees between his conception of himself and his conception of the objects in question. The process of forming a conception, on a most general level, can be seen as a process of categorization (Bruner, 1958). Thus, one may define an occupation (like doctor, lawyer, professor, etc.) by placing it into a series of categories such as "good paying job," "high status," "humanitarian," etc. Similarly, one defines himself by a process of categorization; he places himself into categories like "intelligent person," "student," "wife," etc. These categories—insofar as they exert a "filtering" effect on one's perception of the objects classed within them—once formed, we term "filter categories."

Following from these premises, then, attitude may be defined as a person's conception of the relationship between the filter cate-

gories of which he sees himself to be a member and the filter categories of which he sees the object to be a member.

It follows, then, that the process whereby attitudes are formed and changed is the same as the process by which filter categories are formed and changed. The inclusion of a set of distinct objects into a category is basically a classification based on perceived similarity, and conversely, exclusion of an object from a category is a classification based on a perceived difference. Classification is thus a cognitive act based on the *information* one has about objects and self. Information, therefore, is the basis of filter categories and, hence, attitudes as we define them here. Three sources of such information are assumed to be central to filter category formation:

*Interpersonal Influences.* Perhaps the broadest distinction between types of interpersonal influence noted in the field is that between others who hold expectations for ego and those who serve as models for ego's behavior (Kelly, 1952). According to the theory outlined above, those who hold expectations for ego may do so by (1) communicating definitions of ego's self-filter categories (and thus one's self), (2) communicating definitions of the object filter categories, and thus the object of the attitude, or (3) both. Probably both are involved in most expectations one person holds for another's behavior. For want of a better term, these others are here called *definers*. By the same reasoning, models may exert influence by serving as (1) examples for ego (insofar as ego considers the others to be a member of the same category as himself, the other's actions help define that category and consequently his conception of himself), (2) examples of the object or the object filter categories (as a doctor *defines* medicine for ego simply by practicing medicine where ego can see him), or (3) both. We shall call these two basic types "models for self" and "models for objects."

This is an unorthodox formulation. It makes no assumptions about affect, about any emotional ties that may (or may not) exist between ego and other. *It assumes that others are significant in direct proportion to the amount of information they convey to an ego about the categories he uses to define ob-*

*jects and self, either by word (definers) or examples (models), affective factors notwithstanding.*

*Self-Reflexive Activity.* Self-reflexive activity, as Mead (1934) defined it, refers to behavior in which an individual confronts himself in responding to some object and makes an inference about himself as an active self on the basis of that confrontation. We here take the term in the broadest sense to refer to any definition a person makes about his relationship to an object on the basis of his own observations. We might hypothesize that self-reflexive activity is more influential (compared to interpersonal influences and the effect of previous related attitudes) in the formation of attitudes when the object of the attitude is unambiguous and observable; in the event of ambiguous or nonobservable objects, reliance on interpersonal influence and other related attitudes should increase.

*The Effect of Other Attitudes.* In the abstract, the two sources mentioned above are probably exhaustive. In any ongoing personality, however, new information which a person receives from whatever source is at least partly evaluated in terms of its agreement with what ego already believes (Festinger, 1957). Without making any specific hypotheses about modes of resolution of conflicts or other specific results, we here refer to the more general hypothesis that other relevant attitudes which ego already holds exert some influence on the formation or change of an attitude. Thus, in setting his occupational aspirations, ego is very likely influenced by his educational aspirations—he would be unlikely to aspire to be a doctor without aspiring to be a college graduate as well.

Essentially, the theory presented here is an information theory, with attitudes defined as an individual's conception of relations to objects. Structural factors influence the kinds of significant others to which ego is exposed and the kinds of information that those significant others communicate to ego, and that information, along with what ego can observe from his own activities, provides the basic corpus out of which he sets his attitudes. That information is evaluated in terms of its consistency with previously accumulated information (i.e., other related attitudes)

and results in the new attitude. Thus, the theory delimits five critical variables: (1) the dependent attitude; (2) the information provided by significant others; (3) those elements of phenomenal reality relevant to the dependent attitude which ego directly observes as self-reflexive activity; (4) the prior attitudes of the individual; (5) the individuals' position in the social structure. *Although a hypothetical model of the causal ordering of these variables will be presented later, the main emphasis of this paper is on the definition and measurement of these variables, particularly the second.*

#### THE VARIABLES

##### 1. *The dependent variables*—educational and occupational aspirations.

Since the theory assumes significant others to be influential over attitudes, attitudes are the true dependent variables in this study. The attitudes chosen for study here are levels of educational and occupational aspiration. The concept of level of aspiration is derived from the work of Lewin (1944), and refers to behaviors which may be graded into levels of difficulty. The level of achievement to which an individual aspires within this continuum of difficulty is referred to as his level of aspiration. In this research, the number of years of education to which an individual aspires is considered his level of educational aspiration (LEA); the level of occupational prestige to which an individual aspires is considered his level of occupational aspiration (LOA). Levels of educational and occupational aspirations are virtually ideal attitudes for this research, since they are relatively stable, long range, important to the individual who holds them, virtually universal in the subject population (high school students), noncontroversial, and both are easily measurable with instruments of known validity and reliability.

Level of occupational aspiration (LOA) was measured in this research with the Occupational Aspiration Scale (Haller and Miller, 1963). Briefly, the Occupational Aspiration Scale assumes that LOA is multidimensional, including both realistic (best job you are sure you can get) and idealistic (job you would most like to have), short range (after your schooling is over) and

long range (when you are thirty years old) dimensions. The eight-item test measures each dimension twice, and the score (ranging from 0 to 80) represents a composite of all the dimensions. A fairly elaborate discussion of validity and reliability of this scale is presented in Haller and Miller (1963).

Level of educational aspiration (LEA) is more simply measured with a two-item scale measuring idealistic (supposing you had the necessary abilities, grades, money, etc., how far would you really like to go in school?) and realistic (considering your abilities, grades, financial resources, etc., how far do you actually expect to go in school?) dimensions of the aspiration. Both items were followed by the response alternatives—quit school; finish high school; go to a trade, business, secretarial or nursing school; go to a college or university (one that gives credit toward a bachelor's degree), get an advanced degree (Masters, Ph.D. or professional such as law or medicine). The two items were summed to yield a 10-point scale (Haller and Woelfel, 1969).

##### 2. *Significant Others' Influence.*

The most tedious variable to be measured in this study is the influence of significant others. A two-stage procedure is clearly implied: (1) those persons who are influential for each individual in the sample must be identified, and (2) whatever it is that they do or are that renders them influential must be measured. The process of measurement used follows directly out of the theory presented earlier, and the reader is referred to the theory for a fuller understanding of the process involved.

A. Identifying significant others. Significant others have been defined as those persons who, by word or example, convey substantial information to an individual about the filter categories that individual uses to define himself and/or the objects of his experience. Since the objects of the attitudes in question here are education and occupation, the following procedure was adopted.

Intensive interviews were conducted with 30 high school students in various high schools in Wisconsin. Although the sampling procedure is described in detail elsewhere (Haller and Woelfel, 1968), generally it tried to locate at least one individual with each

possible combination of race (black and white); SES (farm, blue-collar, white-collar, professional, executive); rural-urban; proper age in grade versus over-age in grade; and male-female. Some of the combinations make no sense (e.g., urban-farm), and some are not easily found in Wisconsin (e.g., rural negro professional), and so the total does not equal the 64 possible combinations.

These youth were asked to define education and occupation, and their orientation toward each. The resulting list of definitions were then classified into four generic "filter categories."

The original purpose of eliciting filter categories at all was to use them as cues to remind the subject to think of people who have indirectly influenced his thinking about occupation. If an individual did not influence the subject's definition of *working*, or of *being a doctor*, perhaps he *did* influence his thinking about *money* or how much money a person should earn. This would influence the individual's occupational choice; income would be a filter category for occupation. But after all the filters were coded from the occupational section of the protocol, there were far too many to include on a reasonable questionnaire. Typical responses were "working with people," "good pay," "service to humanity," "high status," "work around animals," "a way to make a living," etc. Although there were many individual responses, a striking characteristic of the list was the great similarity of most of the items to each other. The following actual filters—livelihood, means to support, to buy necessities, \$1.00–\$1.70 per hour (or other actual salary figures) means to support family, make money, compensation, survive—all involve earning money, for example. Because the number of interviews was too small for any meaningful statistical analysis, all occupational filters were intuitively classified on the basis of similarities like those listed above. Four categories emerged into which almost all the filter categories seemed easily placeable: Intrinsic Nature, Extrinsic Nature, Intrinsic Function, and Extrinsic Function.

*Intrinsic Nature*—this category is made up of all those responses indicating activities contributing directly to the work of a particular kind of job; for example, *installing*

*pipe* is part of the work called "plumbing." Some of the more frequent items included in this class were managing people, selling, farming, designing houses, singing, writing theories, etc.

*Extrinsic Nature*—this category is made up of all those responses which describe the environments in which the direct activities occur; perhaps the best synonym is *working conditions*, such as *heavy work*, *work outdoors*, *work around animals*, *work with my hands*, *leave free time for travel*, *not too strenuous*, *fun*, etc.

*Intrinsic Function*—this category describes the purpose of a job; the actual reason for the job's existing; e.g., *healing people*, *manufacturing houses*, *bettering humanity*. It is distinguished from Intrinsic Nature in that it refers to the *reason* the job is done rather than the actual activity being done.

*Extrinsic Function*—this category refers to those functions which are not inherently part of a job, but which can be served by almost any job, e.g., *earn money*, *advancement*, *high prestige*, *buy a house*, *earn the things you need*, *support family*, etc.

This, of course, is by no means the only classification schema that could be imposed on this data. Its usefulness hinges on the assumption that the mentioning (on a questionnaire instrument) of these four categories, along with several sample items of each, may cue the individual to think of the actual filter categories he has used to define occupation and, hopefully, help him remember who he talks to or sees as examples of each of them.

Although occupational filter categories are used as an example, educational filters are exactly parallel. *The initial assumption of the theory is that persons who provide information about these filter categories are significant others for education and occupation.* A questionnaire was then constructed which (1) listed each filter category; (2) asked the individual who had talked to him about each filter category; and (3) asked the individual who he knew was an *example* of each filter category. Those whom ego named as *talking about* the filter categories are considered *definers*; those listed as *examples* of the filter categories are considered *models*. Both models and definers together provide our operational definition of "significant



other." Thus, the individual is *never* asked who influenced him or whom he likes, or to whom he refers himself for definitions, etc. The operations conform very closely to the theory. Because of the complexity of the process, the reader is referred to Haller and Woelfel (1969) for a much fuller discussion of these instruments.

B. Measuring the expectations of significant others. Once the significant others for each individual had been identified by the above procedure, they were sent by direct mail a questionnaire which measured the expectations they held for their focal individual. These instruments are exact duplicates of the instruments used to measure the aspirations of the youth themselves, with only changes in appropriate personal pronouns; e.g., "How much education are you really sure you will get?" is changed to read "How much education are you really sure he/she will get?" Educational forms, of course, were sent to educational significant others and occupational forms to occupational significant others. The reader must be cautioned here, however, that only the *expectations of definers are reported in this article*. Although plausible operationalizations for the information conveyed by models are included in the data, not all egos have models, it turns out, although all have definers. The statistical problems caused by this inherently "missing data" require more elaborate treatment than can be offered here, and must await a later article.

The fact that, on the average, each focal individual had an average of 13.5 significant others led to another difficulty.

For any individual  $i$ , there were  $N$  significant others, each holding expectations relevant to  $i$ 's behavior. We hypothesized that there should be a relationship between the *aggregate value* of the expectations of others and the aspirations of ego, although the precise nature of that aggregate was (and is) a matter for conjecture. For simplicity, the arithmetic mean of the expectations of all the  $N$  significant others for any individual  $i$  was computed (Mettlin, see References). Thus for any individual  $i$ , four scores directly bearing an interpersonal influence are available: (1) his educational aspirations ( $X_3$ ); (2) the mean educational expectations of his significant others ( $X_8$ ); (3) his own occupa-

tional aspirations ( $X_4$ ); and (4) the mean occupational expectations of his significant others ( $X_9$ ). Test-retest reliability correlations (over a three-month interval) for unfamiliar variables are as follows: mean expectations of significant others who are *definers*:  $r_{t1\ t2} = .87$ ; mean occupational expectations of significant others who are *definers*:  $r_{t1\ t2} = .91$ .

C. Self-reflexive activity. This variable refers to those elements of phenomenal reality which ego may use as a basis to judge his own relationship to the object of the attitude under study. In relation to educational and occupational aspirations, we believe this set of variables certainly includes ego's relative performance level in his academic context. To get a broad picture of ego's relative performance in his school setting, an equally weighted index of (a) ego's grade point average in high school to date, (b) the number of extracurricular activities in which ego participates, and (c) the extent to which ego considers himself a "leader" in those activities was constructed.

However, the effect of this variable is not assumed to be completely direct, since these performances may be observed not only by ego but also by his significant others. Thus, this variable is presumed to have both a direct effect on ego's attitude in a self-reflexive fashion and an indirect effect through its impact on the expectations of his significant others.

D. Other related attitudes. The theory prevailing this paper assumes that the influence of attitudes ego holds toward objects related to the attitude under study will exercise independent influence on that dependent attitude. Since there are two principal dependent attitudes measured in this research (educational and occupational aspirations) and since these two attitudes are known to be related to each other (in this research their zero-order correlation is .70), we assume that each attitude exerts reciprocal influence on the other, independently of the other main variables. Thus, the level of educational aspiration constitutes our operationalization of "other related attitudes" when dealing with the level of occupational aspiration, and *vice versa*.

E. Social structural position. We assume that different locations in the social structure

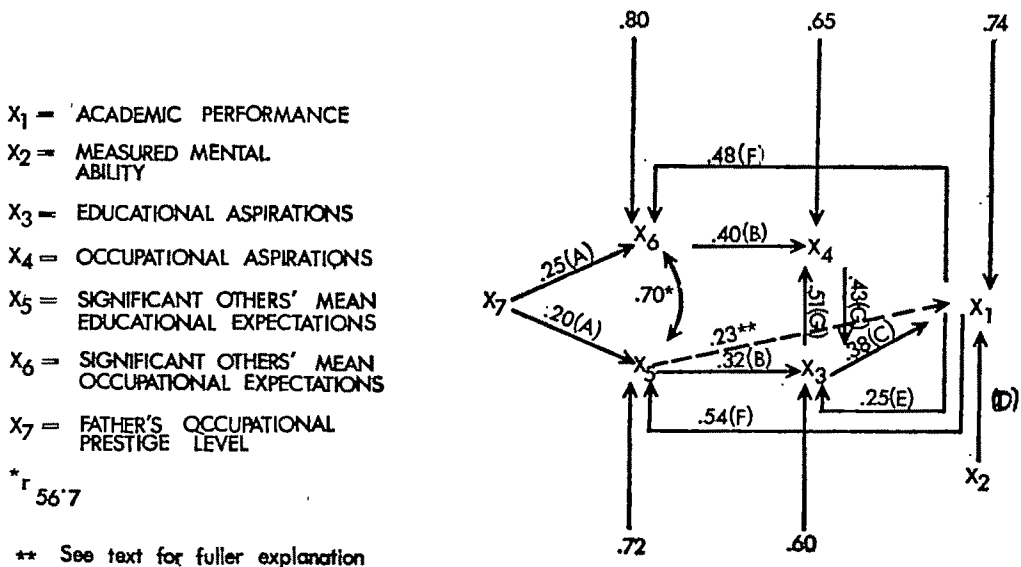
differentially expose their incumbents to various kinds of significant others who take the structural location of ego into account when setting their expectations for him. In this research, structural location is measured by the SES of ego's family. This is measured by the prestige level of ego's father's occupation, rated by the Duncan revision of the NORC scale (Duncan, 1961).

F. IQ. Although the previous variables exhaust those thought to be theoretically interesting, the genetic ability of the student may intrude on the model at the performance stage. We thus control for the IQ of the student as measured by the Otis Quick scoring test of mental ability (Otis, 1954).

Data we collected from 100 high school seniors—the entire senior class—from a small Wisconsin city high school. The Wisconsin city was selected (a) because its size (13,000) is about as large as most Wisconsin cities may be, with only one school; so all the city's students could be located in one place, and (b) because the city itself is based on a fairly mixed economy and a reasonably wide SES range might be obtained. More specific data about the sample is available in Haller and Woelfel (1969). The instruments identified 1,358 significant others for this group of students. A 68% return of questionnaires mailed to those others yielded usable data from 950 significant others. Figure 1

represents what seems a plausible ordering of these variables in this context.  $X_7$  (SES of the family) is one of the social structural factors which may exert influence over significant others and their expectations.  $X_8$  and  $X_5$  are, respectively, the mean occupational and educational expectations of the student's significant others, and represent the interpersonal influence variables of the theory.  $X_4$  and  $X_3$  are respectively the occupational and educational aspirations of the student representing the attitude variables (Haller and Woelfel, 1969: Chapt. 2).  $X_2$  is measured mental ability, here presumed to be one of the outside (nonsocial-psychological) factors which intrude on the theory.  $X_1$  is the academic performance of the student. The arrows marked (A) represent the influence of structural characteristics over the expectations others have for ego. Arrows marked (B) represent the influence the expectations of others have on the attitudes (educational and occupational aspirations) of ego. Arrow (C) represents the influence of ego's attitude on his behavior. Arrow (D) represents the influence of an outside factor (measured mental ability) on the behavior. Arrows (E) and (F) are feedback arrows. Arrow (E) represents self-reflexive activity, or the effect on ego's attitudes of his observations of his own behavior. Arrow (F) represents the effect on the expectations others hold for ego of

FIGURE 1. SCHEMATIC REPRESENTATION OF A MODEL FOR THE FORMATION OF EDUCATIONAL AND OCCUPATIONAL ASPIRATIONS. STRENGTHS OF THE VARIOUS CAUSAL PATHS ARE ESTIMATED BY BETA COEFFICIENTS



their observation of his behavior. The reciprocal arrows (G) between  $X_3$  and  $X_4$  represent the influence of related attitudes on each other. It should be emphasized that no arrow is drawn between  $X_4$  and  $X_6$  since it is assumed that the influence of occupational aspirations on academic performance is indirect. We hypothesize that the role of high occupational aspirations is to cause a person to set high educational aspirations, which result in high academic performance. Similarly, no feedback arrow is hypothesized between academic performance and occupational aspiration, since we assume that influence to be indirect; i.e., the academic performance of the student influences his judgment of his probable academic achievement and thus his *educational aspirations*, which consequently influence his *occupational aspirations*.

**Statistics.** Beta coefficients are used to provide rough indications of the degree of influence of each hypothesized causal variable, including those in the feedback loops, on each hypothesized effect variable. This technique does not provide an exactly accurate estimate of the amount of causal influence exerted by each variable. For in a model which posits "feedback loops" or simultaneous variables (two or more variables exerting reciprocal influence at any given point in time) as this model does, simply reversing the positions of the two simultaneous vari-

ables in the regression equations (that is, allowing each to act as an independent variable on the other in the same regression equation) does not take into account the reciprocal influence of the dependent on the independent variable when estimating the effect of the independent variable on the dependent (Duncan *et al.*, 1968:121-124).

No truly satisfactory method other than actual physical control of the variables has yet been devised for the solution of these nonrecursive models. Rather than halting analysis at this point, we have opted to *estimate* solutions for the equations by the simple expedient of treating the variables involved in such reciprocal loops as if they were exogenous (i.e., wholly determined outside the system) and reading their values from the data. The result of such a procedure is that the presence of substantial beta coefficients between two such reciprocal variables yielded by our procedure is clear cut evidence that a net relationship between the variables does exist, but the estimates of *how much* of that influence travels in *each direction* is inaccurate.

## RESULTS

As the zero-order correlation matrix presented in Table 1 shows, the interrelationships among the variables in the system are quite substantial. Of more interest, however, is the degree to which the hypothesized rela-

Table 1. Observed Correlations for 100 High School Seniors, Their Significant Others' Expectations and Structural and Personal Variables.

Independent Variable		$X_1$	$X_2$	$X_3$	$X_4$	$X_5$	$X_6$	$X_7$
Academic Performance (AP)	$X_1$	--	.37	.62	.49	.58	.51	.59
Mental Ability (MA)	$X_2$		--	.33	.32	.39	.42	.14**
Students' Educational Aspirations (EdAsp)	$X_3$			--	.70	.66	.59	.31
Students' Occupational Aspirations (OccAsp)	$X_4$				--	.55	.64	.33
Significant Others' Educational Expectations (EdExp)	$X_5$					--	.76	.35
Significant Others' Occupational Expectations (OccExp)	$X_6$						--	.32
Fathers' Occupational Prestige Level (SES)	$X_7$							--

\* All correlations except (\*\*) are significant at the .05 level.

tionships are borne out by the beta coefficients in Figure 1. Not all the possible arrows have been drawn, although most have been calculated. Of those calculated, none of those not presented in Figure 1 is higher than .13 (all the beta coefficients are presented in Table 2). Occupational expectation and educational expectation have not been allowed to regress on one another, for example, since (a) the educational and occupational significant others represent to some extent different persons (the conditional probability of one significant other being both educational and occupational is .70), and so the interpretation of such a relationship would be problematic; (b) doing so obscures the relationship between both variables and SES; and (c) because SES has been an important variable involved in the educational and occupational aspiration process, there is some reason to regress significant other influence on it, but our major emphasis here is not on the causal determinants of significant other expectations, even though such a study would be a valuable one. For similar theoretical reasons, neither occupational expectations nor educational expectations have been allowed to regress on educational aspirations or occupational aspirations. Mental ability and SES are treated as given and are not regressed on any of the variables. Specifically, the equations used were:

$$X_1 = \beta_1 X_2 + \beta_2 X_3 + \beta_3 X_4 + \beta_4 X_5 + \beta_5 X_6 + \beta_6 X_7$$

$$X_3 = \beta_1 X_1 + \beta_2 X_2 + \beta_3 X_4 + \beta_4 X_5 + \beta_5 X_6 + \beta_6 X_7$$

$$X_4 = \beta_1 X_1 + \beta_2 X_3 + \beta_3 X_4 + \beta_4 X_5 + \beta_5 X_6 + \beta_6 X_7$$

$$X_5 = \beta_1 X_1 + \beta_2 X_7$$

$$X_6 = \beta_1 X_1 + \beta_2 X_7$$

Where:

$X_1$  = Academic Performance

$X_2$  = Mental Ability

$X_3$  = Student's Educational Aspirations

$X_4$  = Student's Occupational Aspirations

$X_5$  = Significant Others' Educational Expectations

$X_6$  = Significant Others' Occupational Expectations

$X_7$  = Father's Occupational Prestige Level

The main finding is that where substantial relationships were predicted by the theory, they were found; and where they were not predicted, they were not found. In addition, the present operationalization of the theory explains 64% of the variance in educational aspirations and 59% of the variance in occupational aspirations, which are its true dependent variables. These explained variances are important, first because they are higher than the best previously reported (Sewell *et al.*, 1969), and secondly because they utilize the direct measure of exact significant other expectation rather than ego's perception of these expectations.

The model hypothesized that structural characteristics (in this instance represented by father's occupational level) exercised their effect on the individual through the mediation of significant others. The beta coefficients of .25 between father's occupational level and occupational expectations, and .20 between father's occupational level and educational expectations support the notion that structural characteristics influence the expectations of others; the absence of any substantial direct links between SES and *any* subsequent variable (even though there are zero-order relationships) supports the contention that significant other influence is the mechanism of mediation (this is consistent with Sewell *et al.*, 1969). The beta weights between occupational expectations and occupational aspirations (.32) are consistent with net effect of the expectations of significant others on the aspirations of youth. The strong reciprocal arrows between occupational aspirations and educational aspirations (.43 and .51) support but do not necessarily confirm the hypothesized influence of related attitudes on each other (i.e., students take into account their occupational plans when setting educational goals and *vice versa*). The arrow from educational aspirations to academic performance is consistent with the hypothesis that the attitude variable, educational aspiration, exercises substantial influence over the behavioral variable appropriate to it, academic performance. No direct link was posited between occupational aspiration and academic performance, since it was assumed that whatever effect the occupational aspirations of students may have on their academic performance would operate indi-

Table 2. Multiple Correlations and Standardized Partial Regression Coefficients for Equations Represented in Figure 1.

Independent Variables	X <sub>1</sub>		X <sub>3</sub>		X <sub>4</sub>		X <sub>5</sub>		X <sub>6</sub>	
	$\beta$	t	$\beta$	t	$\beta$	t	$\beta$	t	$\beta$	t
Academic Performance (AP)	X <sub>1</sub>	--	.25*	3.14	.02	.22	.48*	5.62	.54*	6.64
Mental Ability (MA)	X <sub>2</sub>	.13	.00	.05	.03	.34	--	----	--	----
Students' Educational Aspirations (EdAsp)	X <sub>3</sub>	.38*	--	----	.52*	5.19	--	----	--	----
Students' Occupational Aspirations (OccAsp)	X <sub>4</sub>	.02	.22	.43*	--	----	--	----	--	----
Significant Others' Educational Expectations (EdExp)	X <sub>5</sub>	.23	1.77	.32*	3.15	1.16	--	----	--	----
Significant Others' Occupational Expectations (OccExp)	X <sub>6</sub>	.03	.20	.07	.40*	3.62	--	----	--	----
Fathers' Occupational Prestige Level <sup>1</sup> (SES)	X <sub>7</sub>	.04	.45	.03	.04	.55	.25*	2.88	.20*	2.46
R	.68*		.80*		.76*		.60*		.63*	
R <sup>2</sup>	.46		.64		.57		.36		.40	
df	93		93		93		97		97	

\* Significant at .05, two-tailed test.

rectly, by raising or lowering their educational aspirations. The actual beta weight for that path is only .025, supportive of the hypothesis of no direct effect.

The arrow from academic performance to educational aspiration represents the direct feedback of academic performance on educational aspirations. The beta weight of .25 is consistent with the theoretical hypothesis that the individual's own observation of his academic performance (self-reflexive act) influences his educational aspirations. No direct link from academic performance to occupational aspirations had been hypothesized since we assumed that high or low academic performance would effect the student's occupational aspirations indirectly by raising or lowering his educational aspirations. In fact, the actual direct link has a beta coefficient of only .02, supporting the hypothesis of no direct effect.

It had further been hypothesized that significant others would observe the student's academic performance and raise or lower their expectations accordingly. These links are represented by the direct arrows from academic performance to educational expectations and occupational expectations. The hypothesis would predict high beta values here, and their respective beta values of .54 and .48 are indeed quite high. We also hypothesized that, of the three modes of influence on attitude, the individual's reliance on the self-reflexive act would increase as the object of the attitude in question became more observable, and conversely the influence of significant others and related attitudes would increase where the object of the attitude is less observable. The data bear this out. Education should be more of an observable object to a student than occupation, since he participates in education day by day, whereas he participates in the occupational structure only imaginatively if at all. Accordingly, both significant others and related attitudes exert more influence over occupational than educational aspirations (occupational expectations to occupational aspirations = .40 versus educational expectations to educational aspirations = .32; educational aspirations to occupational aspirations = .51 versus occupational aspirations to educational aspirations = .43).

Two apparently surprising findings (not

inconsistent with the theory) also emerge from the diagram. First, there is a substantial beta weight from the educational expectations of significant others to the academic performance of the student (represented by the dotted arrow between educational expectations and academic performance) which had not been anticipated in the model. A plausible explanation may be as follows: academic performance is not the behavioral variable ideally to be predicted by educational aspiration—the variable which the theory would argue directly depends on educational aspirations is *educational attainment*, or number of years of education attained. It may be, then, that in some cases students feel that their significant others expect high educational attainment from them and, to satisfy those others, perform better in school but do not raise their educational aspirations accordingly—this is potentially possible since educational aspirations are responsive to variables other than the expectations of significant others.

The second anomaly is the surprisingly low path from mental ability to academic performance. This would seem to indicate that mental ability has little to do with academic performance. This low coefficient is misleading because the academic performance variable includes not only grade point average but also extracurricular activities not so likely to be affected by mental ability. That this is the case is illustrated by the following: (1) the zero-order correlation between mental ability and academic performance is .37, while that between mental ability and grade point average is .60, and (2) when the weighting of grade point average in the measure is doubled the beta coefficient increases to .21.

#### DISCUSSION

Of first concern are the limitations imposed on inference by the present research design. Although the model bears a resemblance to path analysis, it clearly does not meet the requirements of such analyses (Blau and Duncan, 1967:165-172; Wright, 1934, 1960; Heise, 1968), and we have refrained from calling it such. It is, and should be regarded as, simply a graphic representation of a series of mathematically independent regression equations. The presence of a substantial beta

coefficient where one was hypothesized is not an unquestioned confirmation of that hypothesis. Nevertheless, the *absence* of substantial paths where they had been predicted, or the *presence* of substantial paths where they had *not* been predicted would have constituted a clear refutation of theory. In all, substantial beta coefficients were predicted in ten cases and all were found; the absence of substantial beta coefficients was predicted in five cases, and all were confirmed. Thus, 15 distinct hypotheses were supported by the data, while none were disconfirmed.

Bearing these considerations in mind, this research has nonetheless provided considerable evidence consistent with the proposition that all three of the modes of influence hypothesized by the theory—interpersonal influence, self-reflexive activity, and related attitudes—exercise strong causal influence over the formation of attitudes, and that once formed, these attitudes exert independent causal influence over behavior. Perhaps more important is the recognition that all three modes are underlain by the same basic dimension. Both interpersonal influences and self-reflexive activities, in spite of obvious differences in dynamics, are essentially processes whereby the individual receives information about himself—and more precisely about his relationship to objects. That information is filtered through past appropriate information and results in a conception of an *appropriate* relationship to the object in question. There is a strong suspicion that *this information itself is a motor toward behavior*. Although this is not the only interpretation the data will bear, it is an attractive one deserving further research. Based on such psychology, the well-known effects of social structural factors on behaviors are plausibly explained by suggesting that structural characteristics at least partially determine the persons with whom one will principally interact and what information they will principally transmit, and further that structural factors exercise control over the situations in which one will (reflexively) view himself acting. Although other interpretations are possible, the research will support this construction quite well.

With specific regard to mobility theory, several implications should be drawn. First,

the research lends credence to the multi-stage nature of the mobility process: social structural factors determine the expectations of an individual's significant others, which in turn exert causal influence over the person's attitudes. These attitudes themselves then exert directive forces over both academic performance and later educational and occupational attainments (see particularly, for the latter, Sewell *et al.*, 1969). But the process is not simply recursive; feedback from academic performance (and by implication attainments) exerts influence over both significant other expectations and individual attitudes.

The technique used in this research for detecting significant others and their expectations (the Wisconsin Significant Other Battery; Haller and Woelfel, 1969) has implications of its own. Insofar as it enables the researcher to determine the exact educational and occupational significant others for any person as well as their expectations for him, it opens the possibility of experimentally varying either the actual significant others or their expectations for any person or group of persons. Insofar as the Wisconsin Significant Other Battery is fairly easy to modify, its use is not restricted to only educational and occupational attitudes, but may be directed to other long standing, nonsituational attitudes such as prejudice, religious attitudes, consumptive and productive attitudes, etc.

Finally, the statistical difficulties encountered in this study have important methodological implications of their own. The problem of reciprocal influence among variables has been approached from several perspectives before. Duncan *et al.* (1968) have attempted to solve path models involving two reciprocal variables (educational and occupational aspirations) by an ingenious blend of multiple regression and factor analytic techniques whereby those two reciprocals were treated as separate manifestations of a latent substructure (ambition). This new variable is then treated as a single variable in a simple recursive model. While there is reason for argument about the degree to which educational and occupational aspirations are separate attitudes or merely separate manifestations of underlying ambition, such a technique would seem clearly

inappropriate in the case of obviously distinct reciprocal variables, such as the academic performance of a student and the student's educational aspirations.

Sewell *et al.* (1969) have resorted to the simple expedient of measuring reciprocal variables at different points in time. Thus, they measure academic performance at  $T_1$  and aspirations at  $T_2$ . Since it is manifestly impossible for aspirations at  $T_2$  to affect performance at  $T_1$ , Sewell *et al.* do not posit a reciprocal path. The use of time-lagged variables, however, in no way alters the theoretical presumption that *at any given point in time*, aspirations and performances are mutually interdependent. Such interactions are not taken into account in the Sewell *et al.* models, and their path coefficients are correspondingly misleading, insofar as they are the mathematically exact solutions for theoretically inexact statements. The resolution presented in this paper is technically the least sophisticated of all, but it has the advantage of preserving the theory intact rather than modifying it to meet the exigencies of method.

It has become increasingly clear that numerical manipulation of nonexperimental data is insufficient. Fortunately, the theory lends itself well to physical controls. What is clearly needed at his stage of theoretical development is an experimental design in which the variables are physically manipulated rather than statistically controlled. Such a design is not only possible but feasible since the key variables, the educational and occupational expectations of significant others, are themselves amenable to at least some physical manipulation. Although Wisconsin Significant Other Battery does not guarantee such research to be successful, yet without the capacity to detect significant others, one cannot manipulate these expectations in a direct fashion. While much research of all kinds can be performed fruitfully in this area, the understanding of the educational and occupational attainment process and of the attendant level of measurement devices has increased to the point where field experiments have become a distinct possibility.

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## DISABILITY AND DEVIANCE: NORMATIVE ADAPTATIONS OF ROLE BEHAVIOR \*

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*American Sociological Review* 1971, Vol. 36 (February):87-97

*This paper examines disability as a social process. The designation of some forms of exceptional behavior as disability provides a means for the normalization of incapacity in terms of existing role relationships. The requirements for long-term or permanent exemption from role obligations involve legitimation and adjustment to role maintenance. Behavioral rewards and punishments are not effective for regulating behavior recognized as beyond the control of the individual. Agents of social control may, however, influence or regulate behavior through their ability to provide or withhold alternative sources of gratification. Sanctions may be applied in the process of recognition of inadequate role performance, the attribution of responsibility, or the legitimation of performance failure. The accredited disabled individual is excused from role performance by legitimation and may be provided with alternative behavioral patterns for obtaining income, care, rehabilitation or other services. Legitimation may stipulate the behavioral requirements to consolidate modified expectations into a coherent pattern of adaptation—as normalization of the behavior of the incapacitated individual. This conceptualization suggests a more concentrated focus on the elaboration of behavioral alternatives within existing role relationships rather than the proliferation of specialized role repertoires.*

THIS paper is concerned with the conceptualization of disability as a social process. Although disability has received relatively little theoretical consideration, the general tendency has been to treat disability as an extension of the sick role or as a form of deviant behavior. This distorts the nature of the normative prescrip-

tions for incapacity and obscures important conceptual distinctions. We view disability as a form of adaptive behavior provided for by the norms of role relationships. As with other forms of social behavior, the adaptive enactment of disability may take deviant forms or may follow the expectations for behavioral contingencies. This perspective has ample precedent in the work of Lemert (1951, 1967); Mechanic (1966a); Davis (1961) and others (Cohen, 1959, 1966; Nagi 1969; Sykes and Matza, 1957; Levinson, 1959; Goode, 1967; DeLamater, 1968.) We believe that the conceptual distinctions involved have implications for role theory and

\* Revision of a paper presented at the 64th Annual Meeting of the American Sociological Association, San Francisco, September 1-4, 1969.

\*\* The interpretations and conclusions expressed in this paper are the author's and are not intended to represent the position or policy of the Social Security Administration.

for the analysis of deviant and conforming behavior.

#### DEFINITION OF DISABILITY

The term disability, as used here, refers to the pattern of behavior emergent from incapacity—the loss of ability to perform expected role activities because of a chronic physical or mental impairment (Daitz, 1965; Haber, 1967; Nagi, 1969:10–11). This definition focuses on the behavioral consequences of impairment. Other definitions have emphasized 'medical conditions and physical impairments (Wright, 1960:9; American Medical Association, 1958:3) or social impairments, such as age, unemployment, and cultural deprivation (Burk, 1967; Ruesch and Brodsky, 1968).

Analytically, disability is differentiated from disease, injury, impairment and acute or short-term incapacity by the nature of the consequences. Pathological conditions or functional abnormality do not necessarily represent limitations in required performance. Acute illness and short-term incapacity generally do not entail extensive and persistent behavioral changes.

Capacity limitations or handicaps resulting from mental and physical impairment differentiate disability from limitations which are incorporated into the normative social structure such as age, sex, race, education and other cultural or social attributes. Although these attributes affect individual capability, and may in some circumstances constitute social handicaps, they represent performance expectations, rather than limiting factors on the ability to meet performance expectations.

The recognition, attribution and legitimization of incapacity as disability are part of the process of normalization, by which alternative modes of behavior for the acceptance of role failure are provided. This process of normalization is the subject of this paper.

#### SICK ROLE, DEVIANCE AND DISABILITY

Disability has sometimes been conceptualized as an extension of the sick role (Gordon, 1966; Kassenbaum and Baumann, 1965). The sick role provides for contingencies in behavior through exemptions

from role obligations. Sick role expectations are derived, in part, from the recognition that an individual may be motivated to engage in illness behavior (Parsons, 1951a: 436–438). Consequently, he may be held responsible for his behavior. The acceptance and legitimation of exemption from role obligations is conditional on meeting the other sick role obligations.

Although individuals vary in their response to disease or injury, most chronic conditions and impairments are accepted as not subject to the conscious control of the individual. Freidson (1965:80–81), for example, points out that "most illness, and most impairments, are not motivated—they are contingencies of inheritance, accidents of infection, and trauma." Excusing an individual from responsibility for his condition, however, does not relieve him of accountability for his subsequent illness behavior. Role expectations and obligations may be redefined in terms appropriate to his capacities and his limitations; the requirements for the acceptance of short-term and long-term incapacity differ substantially, however.

Several studies have shown that the role expectations for acute and chronic illness are different. The sick role model does not account for variations among illness expectations (Twaddle, 1969) nor for distinctions between sickness and impairment (Gordon, 1966:99–100). Sickness expectations are related to the attributes of the illness as well as those of the individual (Kassenbaum and Baumann, 1965:3). These contingencies are not adequately explained by the sick role. There has also been little study of the process by which "medical sanctions, the approval of intimates or those having influence over them" (Mechanic and Volkart, 1961:52) confer conditional acceptance and legitimation of sick role behavior, especially in relation to chronic illness.

Significant deviations from the modal patterns of behavior may be regarded as violations of the norms unless there are institutionalized expectations which provide for variations in behavior. Illness is generally recognized as an acceptable reason for failure to meet social obligations. There are exceptions, however, when the claimed exemption is considered suspect. Motivated illness behavior, for example, is considered a

special form of deviant behavior in which the individual knowingly violates the norms of the group (Parsons, 1951b:452).

Friedson (1965:72-76), however, also classifies behavioral deviations beyond the control of the individual as deviant behavior, including disability among the undesirable differences which violate valued norms. Thomas (1966:7) expresses a similar view of disability as requiring "resocialization into a deviant social category."

There are several reasons for questioning the utility of both sick role and deviance models for the conceptualization of disability. The sick role exemption is expected to be temporary and related to the duration of illness. Exemption expectations are, however, the most variable of the sick role elements (Twaddle, 1969). Some conditions need no exemption from expected performance. Chronic or persistent incapacities, on the other hand, require changes in role relationships and activities which extend beyond the capacities immediately affected. This may include a generalized reidentification of the disabled person, in terms acceptable to other people, as well as in the disabled person's self-definition (cf. Lemert, 1967; Mechanic, 1966a).

The sick role also assumes that the patient will maintain the desire to get well; sick role exemptions should not reward illness behavior or gratify dependency needs. For the disabled person with irreversible impairment or degenerative disorders, the "desire to get well" is often an inappropriate expectation; therapeutic agents stress the acceptance of impairment and reconciliation to capacity limitations as preferable treatment alternatives (Gray *et al.*, 1969; Lemert, 1967; Fisher, 1958). Normative constraints which define the condition as undesirable may be irrelevant or dysfunctional; alternative strategies are needed to "neutralize" the impairment or to compensate for reduced capacities.

The question of responsibility for illness has a more direct bearing on disability. Treatment of acute illness must be prompt to be effective; the costs of accepting a false claim are small relative to the risks of rejecting a true claim for care (Scheff, 1964). Legitimation of a disability allegation however, may commit the provider to a long-term, high-

cost obligation. Extended incapacity permits extensive validation of the claim for exemption. The risks, therefore, of long-term exemption from performance responsibilities are greater and the process of decision making may be more deliberate.

The attribution of responsibility and the legitimation of incapacity are relevant to the examination of both disability and deviance. Treatment of disability as deviance, however, creates inconsistencies in the interpretation of adaptive responses to sickness and disability. Many role relationships specify, implicitly or explicitly, the reciprocal behavior expected in the event of incapacity.<sup>1</sup> Deviance definitions of disability imply that the behavioral adaptations of the disabled are also undesirable and raise questions about what constitutes appropriate patterns of behavior for disability.

Both Cohen (1959:481-482) and Levinson (1959) suggest that special contingency rules provide for a range of acceptable role definitions adaptive to individual differences and exceptional situations. The notion that any persistent deviation from expected behavior is deviant, without regard to the social processes for legitimation and "normalization" of behavioral deviations would seem to ignore meaningful distinctions between classes of disparate behavior (cf. Cohen, 1966:36).

As an alternative to these concepts, we are proposing a model of disability in which the normalization of exceptional behavior is provided for within the framework of reciprocal role obligations, as a means of facilitating role maintenance and social control. Responses to contingency events such as incapacity may be incorporated into the pattern of role behavior and role alternatives as institutionalized expectations of the appropriate agents of social control.

Disability, as discussed here, is a form of social adaptation to incapacity which organizes behavior in terms of a distinctive pattern of expectations, similar to the behavioral adaptations characterized by Lemert (1951:75; 1967:17, 40-41) as "secondary devia-

<sup>1</sup> Marriage vows, for example, specify responsive behavior "in sickness and in health, for better or for worse." Union-management contracts detail the arrangements for temporary and prolonged disability. The relationship of mother and child specifies care and nurture.

tion." A new social identity based on the "specialized organization of social roles and self-regarding attitudes" is acquired in the process of adjustment to disability (Lemert, 1967:41; Mechanic, 1966b:241-242). Exceptional behavior may be redefined as "normal" and acceptable in terms of new rules of interaction or constitutive norms which evolve from the course of daily events. The constitutive rules permit the normalization of behavior which, in other contexts, might be considered deviant. Deviant behavior may occur if those involved in the interaction process fail to find "alternative acceptable meanings for the departure from the rules (Lemert, 1967:19-20)."

#### NORMATIVE ADAPTATION TO INCAPACITY

Disability designations, formal and informal, provide a social mechanism for maintaining the pattern of social relationships of the impaired individual and the members of his role set; they also provide the agencies of social control with a rationale for alternative rules of behavior in obtaining or providing economic and social satisfactions.

Control agents attempt to maintain social order through the support of conforming behavior and the suppression of disruptive behavior (Cumming, 1968:1-9). Unpredictable or disruptive behavior interpreted as willful violation of the norms may be suppressed by encouraging acceptable behavior or by punishing or isolating undesirable behavior.

If the undesirable behavior of an individual is recognized as beyond his control, rewards and punishments cannot compel more adequate performance. The claimant can respond to sanctions only if his behavior is willful or governable. In the extreme case, behavioral rewards and punishments are, therefore, not effective or appropriate for regulating exempted behavior. The effectiveness of rewards and punishments are more usually a matter of degree; the demands for performance are weighted against the costs to the individual and to the social system. Performance expectations and exemptions in a state prison, for example, are not the same as the level of reasonable demands in an industrial plant; the rewards and punishments available to the control agents and the social constraints are also different. The costs of

performance may constitute a threat to the system or a danger to the physical integrity of the individual. A production line worker with failing vision may sacrifice speed for accuracy within certain limits; the reliability requirements for school bus drivers or airline pilots are such, however, as to normally exclude epileptics and the visually impaired.

At some point, either the level of performance falls below the acceptable minimum or the costs of performance exceed the available resources. If the limiting factor is beyond the control of the individual, agents of social control cannot compel or reward performance interdicted by incapacity. The nonconforming individual is, however, still dependent on society for the satisfaction of his material and social needs (Cohen, 1959:470-472). After the disability is accepted, however, behavioral expectations which take account of the impaired or residual capacities of the individual can develop, as variant but equivalent obligations. The behavioral response to these contingency norms can be sanctioned by providing or withholding gratification for conforming or deviant behavior.

Intentionally or unintentionally, these adapted expectations become means for the "normalization" of behavior, as a new set of rules by which to measure or evidence conformity. If no acceptable alternatives are provided for departures from the general rules for performance, all behavioral deviations appear anomie or deviant (Lemert, 1967:17-21). If the costs of adaptation are too high for the individual, family or community to tolerate, the incapable may be isolated or expelled. Some primitive societies practice abandonment of the aged and the helpless, as "the simplest and perhaps the most humane method of dealing with incapable necessity" (Simmons, 1945:227). The leper is, of course, the classic outcast (*Leviticus*, Chapters 13-14; Gussow and Tracy, 1968); custodial care institutions, to some extent, fill the same function (Townsend, 1962:285-328, 430-438).

The power of social control agencies to sanction behavior lies, to a large extent, in their ability to confer or deny legitimacy to behavior. Three stages may be identified in the process of normalizing incapacity:

1. Recognition of a change in behavior as a role-relevant failure of performance;

2. Attribution of responsibility for incapacity to a condition or impairment beyond the control of the individual;
3. Legitimation of the performance failure by an appropriate agent of social control.

#### RECOGNITION OF INADEQUACY

We have identified the incapacity for normal role performance as the crux of disability. The start of the process of normalizing adaptive role performance is the recognition of "role failure." The individual is perceived as no longer meeting requirements, meeting them inadequately, or making unusual demands upon the system in meeting requirements. The recognition may come from the affected person or from the reciprocal members of the interaction. It may be accepted by either or both members, or denied by one, but not both. One or the other member must claim a significant deviation from performance requirements.

Where both parties agree on the existence of a role failure, (1) the attribution of responsibility and (2) legitimation are still open to negotiation. When the parties disagree, the adequacy of performance may be negotiated, mediated by other agents of the social structure, or evaluated by technical standards.

#### ATTRIBUTION OF RESPONSIBILITY

When the underlying cause of the behavioral deviation is attributed to a condition beyond the control of the individual, the behavior is presented as incapacity. Disasters, acute infections, and accidents are often incapacitating, but usually have short-term and transitory effects. Permanent or extended impairment of function, however, often creates a need for the reorganization of behavior. Performance limitations may also be inferred from medical diagnosis or prognosis.

The role member who attributes responsibility for role failure to a condition beyond the control of the individual, by definition, accepts the existence of an incapacity. The allegedly disabled individual who denies the existence of an impairment, for example, mental illness or hemiplegia (Neff and Weiss, 1965:805-806; Fisher, 1958), must also then deny any significant behavioral failure or concede that his behavior is deviant, as a willful violation of performance expectations.

The other role members may also reject the attribution of incapacity, by defining the behavior as motivated or excessive to the impairment. The appropriateness of the behavior is not disputed if both attribute the behavior to an uncontrollable condition, although additional proofs may still be required for legitimation.

#### LEGITIMATION

Legitimation establishes the responsibility for behavior, by accepting or rejecting the claim for exemption from conventional standards. Performance deviations are interpreted as either conscious deviance from rules, or as the result of forces beyond the individual's control. The nature of the proofs may vary from situation to situation, depending on the formality of the relationship and the certifying agent.

Control agencies may respond to similar behavioral phenomena with different orientations towards treatment and control. The same person may have his behavior legitimated as incapacity by one organization and rejected as deviance by another. Stoll, (1968: 120-121), for example, postulates responses to behavioral deviations based on the agent's beliefs about the ability of individuals to control their own destiny. Other status attributes may be taken into account to legitimate one person's incapacity and to reject another with the same condition. The perception of the moral character of attributes may change with the social climate as, for example, the tendency to regard alcoholism or gambling as compulsions rather than character defects (cf. Gusfield, 1967).

The legitimating agent is the *gatekeeper* of conformity, separating the compliant disabled from the willful deviant. The deviant, having been denied the protection of disability status, may abandon his "inappropriate" behavior or may persist in his deviation from expectations; to maintain his claim for exemption, he must organize his behavior around a pattern of secondary deviance which preserves the claim to incapacity (Hirschfeld and Behan, 1963; Sokolov, 1967). In order to meet his material and social needs, he must sustain himself through performance which will not challenge his self-definition, such as begging, family support or marginal employment.

The *accredited* disabled individual is excused from his role failure by legitimation and may be provided with alternative patterns of behavior for meeting the organizational requirements for care, rehabilitation, employment, or income maintenance. His behavior will also be organized into a pattern of secondary deviations, relative to the general norms, but based on alternative patterns of expected behavior—the constitutive rules for normalization of exceptional behavior.

It is obvious that the relationship of the disabled individual with the detection, diagnostic or treatment agents is influenced by the interests each of them has in the legitimation or accreditation of disability. The claimant for workmen's compensation is in a different position from the worker denying an incapacity to the industrial plant physician. The disabled man who applies for both rehabilitation services and income maintenance benefits for disability is expected to prove both the severity of capacity limitations and the potential for successful utilization of services; the definition of the situation and the constitutive rules may exact different and possibly conflicting conforming adaptations from the applicant.

The control agent's view of the appropriateness of particular forms of adaptation is affected by his resources, his perception of his obligations to the client and to his agency, the social commitments of the agency, and the organizational needs for survival, maintenance and growth. Disability definitions, for example, may be manipulated to provide a stable flow of manpower (Mechanic, 1966b:239). Improvements in diagnostic techniques may affect the administrative evaluation of disability (Rombold, 1960). Agency allocations of resources are also regulated by the continuing needs for support from clients and sponsors. "Creaming," for example, is one way of maximizing the return on investment.

The contingency norms surrounding the availability of adaptive behavior options obviously reflect the interest orientations of the control agents, as well as of the disabled person. The orientation of employers, employees and their agents towards the definition and legitimation of disability is conditioned by the obligations incurred (or benefits conferred) in the accreditation of disability, as

well as by the claimants' other attributes. The criteria for legitimation may itself become a source of conflict (cf. Gusfield, 1967).

#### ROLE MAINTENANCE

Legitimation separates the false or deviant claims for exemption from the "true" or acceptable claims, as part of the process of developing an appropriate social response to the claimed incapacity. The social function of disability is the stabilization and maintenance of role relationships and obligations to the extent feasible for the impaired person and the reciprocal role member. The acknowledgment of incapacity as disability rather than as deviance sanctions the abridgment of the role set as a means to reduce the incompatibility of perceived demands on the role occupant (cf. Merton, 1957:379). The behavioral alternatives of the disabled individual become circumscribed in a series of conditioning acts or commitments which result from the response or expectations of others to the impairment (cf. Goffman, 1961:88).

These responses are conditioned by a contingency which nullifies the conventional standards for behavior (cf. DeLamater, 1968:450; Sykes and Matza, 1957:667). The disabled person is no longer able to conform to the usual expectations of the interaction systems upon which he is dependent for his social and material needs; the reciprocal role members or their functional alternatives are, therefore, called upon to meet their residual role commitments by providing alternatives for the losses entailed by role incapacity.

The development of this adaptive process starts with the social processes which bring the applicant to the control agent with a claim for support, rehabilitation, job placement or other services. The individual has acknowledged his incapacity and is searching for adaptive mechanisms to normalize his relationships. These may consist of efforts to manage the effects of incapacity by: (1) removing or adjusting the impairment, to permit a return to conventional expectations; (2) adjusting the man, through the addition of new or improved capacities; (3) adjusting the situation, through changes in the role requirements or the environment to fit ex-

isting capacities; or (4) providing alternative systems for obtaining need satisfactions, such as income replacement or institutional care.

As a result of these efforts at social management, the disabled individual may recover from his disability or may adapt to his capacity limitations. Recovery reverses the process of disability. When the impairment no longer limits the role-relevant activity of the individual, he is expected to resume normal role activities. Persistence in maintaining performance exemptions is regarded as deviant behavior.

Successful adaptation to disability contributes to maintenance of the network of role relationships of the disabled person. The disabled person who conforms to the expectations of the appropriate control agents receives institutional support in normalizing his role relationships. The social, moral, and economic benefits derived from the relationship with the control agents reward conformity to the constitutive norms and discourage behavior which violates these norms.

Adaptations to disability which impose limitations greater than that required by capacity impairment are maladaptive for role maintenance. Invalidism, for example, represents "one of many patterns of attempted adaptation to personal difficulties that lead to the impoverishment or distortion of social role-taking" (Cameron and Magaret, 1951: 231). The individual may react to impairment by restricting his activities and social contacts beyond the point made necessary by the capacity limitation alone.

Some people, of course, are caught between institutional alternatives, with limited opportunities for adaptation and no avenues of institutional support. The accreditation of their disability would seem to have marginal value at best. To be viable, their adaptation must be to some extent deviant, either towards greater dependency than they require or towards more independence and greater use of residual capacities than they are expected to sustain.

The loss of role capacity introduces a state of normlessness or "anomie" in which the individual's ties with normal role commitment may become attenuated by the forced consequences of disability. Continuity with past role obligations may be maintained

through the management of the effects of role limitations. After the individual is accepted as disabled, his behavioral alternatives are perceived as responsive to his capacity impairment; commitment to his usual or previous role expectations is weakened and he becomes increasingly committed to his adapted role expectations.

The cumulative effect of adaptive responses to these adjustive mechanisms is a pattern of secondary deviation which *could* be characterized as the "disabled role" or as a "disability career." The adaptive responses, however, are more meaningfully understood as contingency norms provided by the existing network of role relationships. Disability behavior and other forms of sanctioned exceptional behavior are more easily and parsimoniously accounted for by adaptations within the major role relationships than by the proliferation of a repertoire of roles for all contingencies.

#### DILEMMAS OF LEGITIMATION

A major problem in the legitimation of disability is the difficulty of determining the extent to which the impairment is causally implicated in the presented behavior. Sickness or illness determination requires only confirmation of the existence of a functional abnormality or of treatment possibilities. Disability, however, involves assessment of capacity for required role performance, not of impairment. Impairment residuals, for example, which do not affect capacities required for role performance are not disabling. Impairments, which in themselves are not sufficient to significantly affect performance, may be disabling when combined with other personal or situational attributes.

Disability typically emerges from the interaction of the impairment with individual attributes and the environment, as represented by the physical and social demands on the role occupant. The supply and demand for services also affect the extent to which an impairment of performance is regarded as an incapacity. In an economy with a scarcity of labor, for example, there is more demand for residual capacities. The society will accept greater limitations as still representing "adequate" role performance. In the "economy" of family relationships, the scarce supply of marital or parental services and the

valuation of unique services provide greater scope for adaptation of family relationships to limitations in performance.<sup>2</sup> Impairment is more easily converted into disability, as role-significant incapacity, when (1) there is a surplus of skills and (2) performance requirements are raised to reflect the higher level of skills available.

The complexity of the relationships which define disability makes it difficult to evaluate the extent to which incapacity is due to a specific impairment or functional limitation, the adaptability of the role requirements and of the individual, and the demands of the physical and social environment. Disability is essentially a judgment about the combined effect or interplay of these factors on required performance. Administrative organizations, however, usually require simplicity, standardization, and reliability for routinized decision-making. This underlies, at least in part, the tendency to define disability in terms of attributes, such as medical impairment (American Medical Association, 1958) or social impairment (Burk, 1967; Ruesch and Brodsky, 1968), rather than as interrelationships. Most disability decision-making compromises between the extremes of identifying disability as medical or social impairment.

Regardless of the normative or administrative standard used for disability, however, it is the legitimating evaluation which "makes the disability" and which differentiates disability from deviance. As Freidson (1965: 74) points out, the meaning imputed to the condition, not the physical reality, establishes the significance of the limitation. The individual who persists in a rejected or unsupported claim of incapacity is often labeled a crank or malingerer. Persistence in the denial of disability, after the incapacity has been accredited, is considered inappropriate compensatory behavior; the illusion of

capacity is felt to interfere with treatment and recovery possibilities (Gray *et al.*, 1969; Fisher, 1958:784). Although a more popular deviation than the former, denial of disability may create as much of a problem for the treatment and family allocation system as a "fictionalized handicap" (Thomas, 1966:11). The safety of the system may also be threatened by attempts to minimize or conceal capacity limitation—there is less toleration, for example, of the mental aberrations of a kindergarten teacher than of a college professor.

The legitimation process creates the exemption from responsibility and from usual role requirements by explaining behavioral variations in normatively acceptable terms. It may also stipulate further behavioral requirements to consolidate expectations into a coherent pattern of adaptation. "Normalization . . . takes place in informal interaction or through the formal agencies of social control" (Lemert, 1967:24).

The legitimation of disability is, by definition, a labeling process. It tends to reinforce the awareness of identified limitations and to stabilize behavior considered appropriate to incapacity. Labeling role failure as disability reduces critical evaluation by neutralizing conventional norms and by offering adaptive alternatives.

The labeled exemption may also discourage efforts towards recovery, through the organization of self-regarding and public attitudes which freeze the individual into the socially validated behavior. The responses to disability or impairment labels may be distorted by diffuse evaluations of capacity, limiting behavior in areas not directly affected by the disability. Identification with a condition may infer capacity limitations for the individual beyond those of the particular case (cf. Mechanic, 1966b: 242). Factors associated with a condition, but unrelated to capacity, may restrict alternatives (Olshansky, 1968). Workers with histories of cardiac conditions, for example, have difficulty in finding employment because of the potential liability under compensation or insurance agreements. These evaluations are institutionalized into hiring patterns through age and physical requirements for employment (Irvin, 1960). Families, in their desire to be supportive, may

<sup>2</sup> Role requirements and role relationships, however, may also have a cut-off point or threshold beyond which impaired capacities cannot sustain the role regardless of uniqueness. The system must either adapt to provide for reallocation of role functions as, for example, when death occurs, or take in a new occupant, with resulting displacement or "expulsion" of the disabled role occupant. The problems are similar in many ways to those of the long-term chronically unemployed worker (Stouffer and Lazarsfeld, 1937; Haber, 1964).



inhibit adaptive efforts towards independence.

Analysts of deviant behavior have pointed out that labeling may constitute an additional pressure towards deviance, through the disruption of primary relations, the closing of alternative avenues of conventional behavior and the stabilization of transitory behavior (Freidson, 1965:84-89, DeLamater, 1968; Scheff, 1966:80-84). It is apparent that labeling may have both adaptive and maladaptive consequences; however, it cannot be considered apart from the initiating behavior. Labeling increases the visibility of the disabled or the deviant and emphasizes the responses considered appropriate to the perceived behavior. It is the initiating behavior which is disvalued, however, rather than the label (Cohen, 1966:36-38; Akers, 1968). The legitimation of incapacity is intended to reduce role strain by providing adaptive opportunities to the disabled. Faulty judgments and inappropriate responses may limit the adaptations available to the disabled (cf. Davis, 1961), but this would appear to be more a problem of the appropriateness of the label than of the effects of labeling.

In much the same way, disability behavior, or impairment, or disfigurement may be accompanied by stigma, as "signs of moral inferiority" (Lemert, 1967:42). Stigmatization does not imply nor does it require deviance (Cohen, 1966:36; Goffman, 1963). Any perceived disparity of status or capacity may produce stigmatization. The acceptance of disability and the extension of role maintenance mechanisms do not restore the incapacitated individual to full use of his resources nor to full access to the social and economic advantages of capacity. The denigration of minority groups, the disabled, and other socially disadvantaged persons may be accounted for by the perceived limitations in their social capacities, rather than by violations of valued norms. It is also possible that there are no exemptions for incapacity in such areas as aesthetic norms.

Stigma has positive functions for the disabled (Goode, 1967:12-14; Lemert, 1967: 48-49), as protection against competitive demands, as well as negative aspects. Stigma is, in any case, not a necessary condition of disability, although it must obviously be

taken into account in considering the adjustments required for the stabilization and maintenance of interaction (Davis, 1961: 132).

#### CONCLUSIONS

This analysis has examined the conceptualization of disability from the viewpoint of social process and social control. We have looked at disability as sets of adaptive responses to the loss or limitation of role adequacy. The adjustive mechanisms provided by contingency norms permit exemption from role requirements and stimulate adaptations of behavior to maintain the role relationships of the disabled person. Legitimation sanctions the efforts of role partners and control agents to provide alternatives for the exempted role behavior.

We have also attempted to examine sick role and deviant behavior as alternative models or approaches to the concept of disability. The sick role directs attention away from the crucial element of legitimation of incapacity and introduces normative constraints which appear neither necessary nor appropriate to chronic disease and prolonged incapacity. By relating disability to the structure of existing normative relationships and the development of contingency norms, the adaptive response process avoids many of the conceptual problems raised by the sick role. This suggests that the tendency to manufacture roles to account for contingencies may be misdirected. Disability behavior may be more parsimoniously described by focusing on the adaptive mechanisms and responses of the major role relationships. This conceptualization also suggests that more research and theory should be directed towards the elaboration of the behavioral alternatives provided by a small set of significant role relationships rather than to the proliferation of inventories of specialized roles.

To the extent that the conception of disability discussed here differentiates disability from deviance on the basis of recognized incapacity and sanctioned deviation, it also sharpens the conceptualization of deviance as willful or "capable" behavior susceptible to control by the individual and therefore susceptible to manipulation by rewards and punishments. We have drawn this distinction

as a relatively sharp and unambiguous line between acceptance and nonacceptance of incapacity. There are, of course, many circumstances under which the distinction is ambiguous and the social responses are ambivalent; mental illness, alcoholism and obesity, for example, are particularly susceptible to variations in interpretation.

There are many areas in which society has institutionalized ways of neutralizing the commitment to responsibility and extending partial legitimation to incapacity. Alternatives such as divorce, bankruptcy, and welfare assistance are premised on the assumption that anomic or deviant responses to incapacity, regardless of responsibility, are more disruptive than the contingency alternatives. The analysis of disability behavior, as normalizing role responses, suggests an orientation towards the study of role structure and role relationships which focuses on the situational variations that differentiate deviance and conformity in the process of social regulation.

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STATEMENT OF OWNERSHIP, MANAGEMENT AND CIRCULATION.

1. Date of Filing, December 9, 1970.
2. Publication, AMERICAN SOCIOLOGICAL REVIEW.
3. Frequency of Issue, bi-monthly.
4. Office of Publication, Boyd Printing Company, Inc., 49 Sheridan Avenue, Albany, New York 12210.
5. General Business Offices of the Publishers, 1001 Connecticut Ave., N.W., Washington, D.C. 20036.
6. Publisher, American Sociological Association, 1001 Connecticut Ave., N.W., Washington, D.C. 20036; Editor, Karl F. Schuessler, Department of Sociology, Sycamore Hall 247, Indiana University, Bloomington, Indiana 47401; Managing Editor N. J. Demerath III, American Sociological Association.
7. Owner, American Sociological Association.
8. Bondholders, none.
9. The purpose, function and nonprofit status of this organization and the exempt status for Federal income tax purposes have not changed during preceding 12 months.
10. Circulation (a) average number of copies each issue during preceding 12 months, (b) actual number of copies of single issue published nearest to filing date:
 

Total no. printed	(a) 20,000	(b) 20,000
Paid circulation (mail subscriptions)	(a) 17,758	(b) 18,437
Free distribution	(a) 25	(b) 25
Total distribution	(a) 17,783	(b) 18,462
Office use, etc.	(a) 2,217	(b) 1,538

I certify that the statements made by me above are correct and complete.

N. J. Demerath III  
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# THE SOCIAL ASCRIPTION OF MOTIVES

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American Sociological Review 1971, Vol. 36 (February):98-109

*Certain conceptions of motives are examined with regard to their sociological status: (1) motives as causal antecedent variables, (2) motives as characteristically private states of persons, and (3) motives as concrete speech acts, such as the giving of reasons, justifications, intentions, or accounts. These are found wanting because they do not permit a sociological understanding of motive as a public and observable course of social action. The relevance of motive to classical sociology resides instead in its status as a grammar of everyday language and conduct. We propose that any sociological account of motives requires attention to four public social conditions for the ascription of motive: (1) motives are observer's rules, (2) motivated objects are social members, (3) motives are a grammar of application, and (4) motives formulate social types of persons. These are confirmed sociological processes because they describe how motive is socially organized.*

## I. INTRODUCTION

OUR intention is to explicate the sociological status of motives. We shall address this issue by describing how ordinary actors employ "motive" as a practical method for organizing their everyday environments. Since motive is used by ordinary societal members to manage their orderly routines, our explication will be a formulation of the ways in which members' practices are grounded in their knowledge. We shall suggest, that all sociological conceptions require some version of the common sense member (of his practical knowledge); and we shall depict motive as one common sense device for ascribing social membership, since motives are used by members to link particular concrete activities to generally available social rules. Motive, then, is one collective procedure for accomplishing social interaction, and for sorting out the various possibilities for social treatment by linking specific act and social rules in such a way as to generate the constellation of social actions that observers call "persons," "members" and "membership."<sup>1</sup>

<sup>1</sup> Readers of an earlier draft commented upon the absence of any mention of ethnomethodology as strange. Limitations of space prohibit us from the sort of detailed comparisons which such a topic requires, and there is unfortunately no adequate description of ethnomethodology in the literature; so, for example, Denzin's paper (1969) is hopeless not merely for his misunderstandings, but for his inadequate

## II. THE SOCIOLOGICAL POSSIBILITY OF MOTIVE

Social scientists tend to conceive of motives as private and internal characteristics of persons which impinge upon and coerce these persons into various behaviors. In this view, motives are seen simultaneously (1) as "causal" antecedent variables (antecedent to the event of interest), and (2) as characteristic "states" of persons engaging in the behavior. We maintain that these senses of motive are inadequate because they issue from a common misconception of motive, namely that motives are concrete, private and interior "mainsprings" that reside in

quately concrete conception of analysis; e.g., in superficially focusing upon the surface features of the approach—the concern with "meaning," with the "actor's point of view," etc.—he loses sight of the analytic character of ethnomethodology and of analysis in general. Wilson (1970), on the other hand, may understand ethnomethodology but not what to do about it. If ethnomethodology obviates literal description, then suggesting that we be "more explicit and self-conscious" (1970:706) is regressive if not vacuous — it is only to ask that we improve on the same literal procedures. As to the contrast between this paper and ethnomethodology, we must first record the great influence of the writings of Garfinkel (1957), though this influence has not worked itself out in our thinking in the ways it has in his students. Ethnomethodology, as it is practiced by these students, not only fails to supply our program with its rationale but denies this rationale at critical analytic points. Ethnomethodology seeks to "rigorously describe" ordinary usage, and despite its significant transformation of standards for conceiv-

people, rather than public and observable courses of action.<sup>2</sup>

If sociologists have sought to understand and formulate descriptions of social action, and if the analytic status of social action resides in its character as behavior which is normatively oriented to the very same environment it constitutes, then motive can function as an observer's rule for deciding the normatively ordered character of behavior. That is, motive is a public method for deciding upon the (sociological) existence of action. In this usage, motive is an observer's rule of relevance in that it represents a sociologist's decision (his election) as to how items of concrete behavior are to be reformulated as instance of social action.<sup>3</sup>

ing of and describing such usage, it still conducts its inquiries under the auspices of a concrete, positivistic conception of adequacy. Ethnomethodology conceives of such descriptions of usage as analytic "solutions" to their tasks, whereas our interest is in the production of the idea which makes any conception of relevant usage itself possible. Whereas ethnomethodology uses the ordinary world "seriously" (they hope to solve analytic problems by doing naturalistic science on this world), we treat the everyday world as a proximate occasion for initiating inquiry and not as a "fact" to be reproduced. In our respective attitudes toward ordinary language and the everyday world, we have about as much in common with ethnomethodology as Heidegger shares with Austin. Finally, ethnomethodologists would regard our task in this paper as a stipulative exercise in legislating the use of a "concept," while we would treat such an objection as a failure of analytic nerve, as the typical positivist gambit (which goes back at least as far as Protagoras) of refusing to exercise analytic authority, despite the fact that such authority grounds their entire enterprise with its intelligibility. Some of these issues are taken up in detail in work now under way.

<sup>2</sup> This misconception leads to the contention that motives are only peripheral to the classic tradition of, say, Marx or Durkheim—that human motivation plays the role of unstated premises and is properly disregarded for analytic purposes (the ecological argument), or requires explication only in order to account for more of the variance (the social psychological argument of Homans, 1964; Inkeles, 1959). We contend instead that motive plays a central role in such traditions, a role we can recognize if we grasp the analytic status of motive rather than its concrete character.

<sup>3</sup> For example, the sociological import of economic determinism in Marx is not the impersonal effects of brute facts upon an organism, but rather his formulation of a meaningful environment constructed by and seen from the perspective of a typical actor. To say "economic determinism exists" is to decide to formulate actors as oriented to selected particular

The classic roster of terms used by sociologists—social class, community, religion, suicide, bureaucracy, conflict, and the like—all require in their various ways this sort of conception. Motive then, serves as a theorist's election that some rule is relevant for explicating the character of some event as an instance of action. Motive is not in this regard a thing in the world but a way of conceiving social action.

In a more precisely sociological sense, it is observers who introduce a topic into behavior, whether motives or any other. Thus, to say that "social class exists," or that some group is high on need achievement, is for an observer to decide that some collection of persons is oriented to their status as a collective actor and is to be conceived as acting under the auspices of such an orientation. When an observer asserts that the American workers constitute a social class, he is deciding that the collection conceivable as American workers presently show in their behavior their status as a collective actor, the identity of which the observer provides on the basis of their relation to the instruments of production.<sup>4</sup>

The upshot of all this—that motives function as observers' rules of relevance—is that though motives might be described as personal properties or characteristics of persons, they acquire their analytic force as observers' rules for depicting grounds of conduct.<sup>5</sup> Motives are a way for an observer to assign

features of their socially organized environments in such a way as to enable this orientation (now called "the economy") to produce their routine actions. To describe economic determinism is then to assign a rule of relevance to actors which serves the purpose of explicating social structure by reference to their grounds of action (the economy) as a set of sociologically intelligible events of social structure (economic determinism).

<sup>4</sup> This is also the sense in which Weber's *Theory of Social and Economic Organization* (1947)—particularly the sections in which he depicts types of social relationships—can be read in a fundamental way as a set of methods for conceiving of motives. Each relationship is a different method for formulating actors as acting under the auspices of relevances to which they are methodically and selectively oriented.

<sup>5</sup> Thus, conventional notions of motives as concealed premises or as peripherals that increase variance are incorrect in the fundamental sense that it is in the *observer's* ascription of such rules, and not in the state-of-affairs the ascriptions recommend, that the analytic status of motives resides.

relevance to behavior in order that it may be recognized as another instance of normatively ordered action.

It should now be clear why motives cannot be private and internal; if motives are sociologically depicted in the ascription of rules, the ascription of rules itself requires (presupposes) the use of a language that is public and observable. Even when we speak of "hidden" motives, we are of course engaged in fully intelligible and observable courses of treatment—some public criterion enables us to grasp the topic. The so-called hidden motives, slips, and the like are observable states of affairs which can be discussed in sensible and concerted ways. To treat motives as private is to confuse the state of affairs which motives report with the analytic status of the term, which status is supplied by the public and by generally available rules that make motive reports socially possible and observable modes of social action.<sup>6</sup>

### III. GRAMMATICAL AND FACTUAL PERSPECTIVES IN THE FORMULATION OF MOTIVE

We have said that motive is a term regularly employed by actors to accomplish their routine affairs. When practical actors are "doing motives," they are engaged in formulating themselves and their environments, in constructing and treating with their common sense courses of action. To say "He had the jealous motive to murder her" is to do no more or less than lay out and characterize an environment in such a way as to report on a social state of affairs, to make some behavior possible, to limit the use of other behavioral possibilities, and so forth. The sociological status of the idea rests on whatever must be known in order to produce motive-talk as a recognizable or observable course of action. To provide a motive, then, is to formulate a situation in such a way as to ascribe a motive to an actor as part of his common sense knowledge, a motive to which he was oriented in producing the action. Thus to

give a motive is not to locate a cause of the action, but is for some observer to assert how a behavior is socially intelligible by ascribing a socially available actor's orientation.<sup>7</sup> Questions of orientation do not require factual solutions (they are not either true or false), but rather, grammatical solutions. To talk motives is to talk grammar.

At the same time, however, social psychologists have used motive as a technical term designed to unravel the causal patterning of a sequence of actions (Atkinson, 1958; Cattell, 1957; Lindzey, 1958; Smelser and Smelser, 1963; White, 1963), e.g. need achievement as an antecedent of economic development (McClelland, *et al.*, 1953). One problem with such technical usage is that it does not reformulate motive from the perspective of a practical member; i.e., it ignores motive as a social course of action, and so it fails to provide for the relevance of motive as an activity engaged in by practical members.<sup>8</sup>

To treat motive as a cause, as in "What was his motive for suicide?" is to commit one version of the fallacy which Austin (1965) and others have discussed in detail: the fallacy of presuming that suicide is an act which somehow describes some antecedent state of mind which preceded or caused it. On the contrary, to recognize the act of suicide as an intelligible event-of-conduct is to assign to that behavior its identity as social action, in this case by formulating a motive. Thus, motive is a rule which depicts the social character of the act itself. It is not that his suicide reports some antecedent depression, or that murdering his wife reports his jealousy, or that leaving the party reports upon his boredom. Rather, the character of the suicide, the murder, the departure are identified through the clarification

<sup>7</sup> This is perhaps what some sociologists intend when they invoke phrases like "actor's point of view," "what it means to the actor," and "significant symbol."

<sup>8</sup> Technical versions, which causally locate motives in neural circuits, libidinal arrests and the like, reformulate what is essentially a member's device without respect for those relevances and interests of the member which generate the behaviors of interest to the technician. Whatever the efficacy of neural paths, they have no social relevance except as they can be understood to be employed by the members.

This is not a new criticism and has been stated most articulately by the British ordinary language philosophers (Peters, 1958; Austin, 1965).

<sup>6</sup> There may be a concrete psychological state-of-affairs that corresponds to a motive report. We do not address this and so are not trying to run psychologists out of business. Our purpose, again, is to analytically describe the social organization of motive ascriptions as courses of social action.

tion of unstated circumstances which make these actions socially recognizable as suicide, murder and departure. To say "I want a motive for the murdered wife" is not to say that I want merely his antecedent state of mind which the wife's murder follows, but is instead to explicate the situation (the context, knowledge, conditions) which makes the event socially possible (a recognizable murder).

The best criticism of the causal account of motive is to be found in Melden's *Free Action* (1961) in which he demonstrates that since any conception of a cause presupposes a description of the very action for which the cause is identified, these accounts violate the necessary assumption of the analytic independence of cause and effect in the Humean model of causality. That is, some thing cannot be cited as a cause of an event if this "something" is involved (presupposed) in the very description of the event; analyses of motive accounts show that whatever is cited as a motive serves to more fully and completely characterize the event for which it is formulated, and cannot then be treated as independent of the event. Melden says: "this explanation does not refer to a present moment, sliced off from what has gone before and what will follow, but to the present action as an incident in the total proceedings"<sup>9</sup> (1961, 98-99).

This is why it is elliptical to assert that motive describes only and simply a state of mind, when instead it serves to demand an explication of the circumstances which confer upon this putative state of mind its reasonableness as an account. Because such an explication amounts to a theory or formulation, it would be more correct to say that the quest for a motive (why did he kill her?) is a request for a theory.

<sup>9</sup> We have not seen successful demonstrations of objections to this position in the philosophical literature, for those who claim that motives *can* be causes, or that motives *do* explain, usually accomplish this by changing the sense of "cause" and by treating "explanation" concretely rather than analytically. Of course, motives *can* be causes if one uses cause in a different sense; our notion is that any different sense makes "cause" a constituent feature of the action (i.e., a presupposed element of the action), and hence, not a "cause" in the ordinary sense at all. But, one is free to do this. Secondly, motives *do* explain, but by providing a way to see "cause" as an intelligible link, not by citing a cause.

#### IV. MOTIVE AS SURFACE STRUCTURE

We have sought to show that motive acquires its analytic character as a public (methodic) product rather than as a private "state," and that it is to be understood grammatically (as part of the meaning of an action) rather than as a factual report on some contingent, antecedent event. Now, however, we want to show how the analytic sense of motive is not located through a report of usage, but rather by formulating the conditions of knowledge which make such usage possible.

One option to the technical causal idea is thought to be a conception of motives as members' purposes, reasons, justifications and accounts (Gerth and Mills, 1953; Schutz, 1962; Scott and Lyman, 1968; Shwayder, 1966). In this tradition, the actor is used by the observer as a research informant, whose report acquires analytic status because the actor is thought to be a privileged and exclusive source on questions of his motive.

The paradigmatic procedure here is to ask the actor, "Why . . . ?" and expect him to cite a reason, goal, or intention, e.g., "Why did you leave the party?"; "Because I was bored"; "In order to make my appointment"; etc. In some cases we take these reasons and call them symbols and meanings, but that sort of substitution does not really tell us much about the methodical ways in which such statements are generated to begin with—how, for example, the actor is constrained to cite a reason at all; how it takes the form it does (giving a reason instead of, say, telling a joke); how it comes to be acceptable to the hearer that it is an answer. In other words, its status as a common sense practical device, as opposed to mere idiosyncratic noise or gesture or cue, remains unstated.<sup>10</sup> The methodic social and hence sociological feature of motive lies not in the concrete, substantive reason an actor would give for his behavior, but in the organized and sanctionable conditions that would regularly produce the

<sup>10</sup> We are not making a sampling argument here. Even when an unanalyzed event occurs repeatedly, it remains only a regularity. Lacking analysis, we would still be without an understanding or description of it, regardless of the times and places it had been counted.

giving of a reason by a competent member in the first place. The reason given is no more than the surface expression of some underlying rule(s) that the former requires in order to be understood (Chomsky, 1965).

The surface performance which is displayed in the use of motive might be that of offering a reason, goal, or intention, but to provide an account of motive in these terms is to ignore the deep structure which makes the surface display possible at all. As we shall investigate below, motive acquires its analytic status by virtue of the fact that it requires for its use certain deep structures for conceiving of "person," "member," "responsibility," "biography," and the like; these deep structures are absolutely necessary for an ordinary member's competent and sensible employ of motive as a device, because they generate the variety of surface reasons he may cite in any particular case. It is through these deep conditions that an analytic conception of the ordinary sociological use of motive is provided.

It should be clear then, that the similarity which typical symbolic interaction accounts appear to achieve with our present formulation—through such phrases as "vocabularies of motives" and the like—is misleading, for they treat concrete speech acts—such as giving reasons or justifications, or citing intentions—as providing an analytic explication of motive, whereas we treat such usage as surface phenomena which are made possible by deeper conditions of knowledge. The concrete character of such versions of motive is conveyed in their practice of treating motive as the practical actor's expression of his knowledge, in contrast to the present suggestion that analytic status is supplied through some conception of an observer's method of constructing a practical actor (no matter what he is concretely taken to perform or say).

For example, Gerth and Mills (1953:116) cite Weber as we do but interpret him as using motive as equivalent to "an adequate reason for conduct." On the other hand, we are saying that this usage fails to capture the analytic character of motive as an *observer's decision to treat or reformulate* some behavior as a reason, and that this is an entirely different matter. So too, they see

motive as "ascribed through talk" (1953:114) and as a "term" of the talk itself, whereas, in our view, speech is just a medium for the concrete expression of motive, but it is a set of prior and deeper conditions of knowledge which permit an observer to treat talk as intelligibly predicating motive. The symbolic interactionist finds it impossible to formulate a version of motive that is analytically distinct from conventional versions, because he still conceives of motive as a practical actor's concrete report of his state of mind: he only shifts his focus from the state-of-mind to the talk, by treating the talk as some sort of public indicator of the mind. Gerth and Mills show this quite explicitly when they eventually surrender to a concern for questions such as what the differences are between professed motives and "real motives" (1953:119), and with the actor's degree of "awareness" of his motives (1953:125). These are not the questions of those who have grasped the analytic character of motive.<sup>11</sup>

We can now locate the major difficulty which unifies the various conventional accounts of motives, i.e., the conception which appears to unify the various sorts of troubles we have been discussing; these accounts pose the problem of motives as a factual rather than a grammatical one, which leads easily to the trap of treating motives as causes, as states-of-persons, and as concrete speech-acts such as reasons, accounts, and justifications. And these accounts treat motives as raising a concrete, factual question of "why?" rather than as attempts to formulate the socially organized conditions under which such a question is sensible to those who raise them. Any strategy which equates the factual surface structure of motive talk with an analytic conception of

<sup>11</sup> Though more ingenious than most, Kenneth Burke (1945) is not exempt from these charges, for in his effort to locate the analytic parameters of motive, he confuses analytic and concrete conditions (i.e., the parameters of act, agent, agency, scene, and purpose). However, despite Burke's failure to explicate the grammar of motive, in more than a metaphoric sense, he performs a service by continually keeping before us the notion that the way in which priorities are allocated to these parameters is a function of a theoretic election (that it is observers who formulate motives), though we do not get a description of how such formulations are accomplished.



motive cannot provide any more than a concrete and irrelevant record.<sup>12</sup>

In sum, when we speak of motive, we have in mind neither the technical observer's notion of the causes of an action, nor the actor's report of why an action was done. We do not require either an explanation or reason for the action, but rather some description of the socially organized conditions which produce the practical and ordinary use of motive in the mundane affairs of societal members. To locate motive is thus not to "find" anything but to describe the necessary and analytically prior understandings and conventions which *must* be employed in order for a member even to invoke motive as a method for making a social environment orderly and sensible.

#### V. THE DEEP STRUCTURE OF MOTIVES

##### A. *Motives Are Observers' Rules*

Motives acquire their analytic status as observers' rules. They are not forces or events in the world extraneous to an observer. Motives are sociologically possible only because some practical observer has methods and procedures—i.e., rules—for locating them as events in the world, not because that is where they really are. Because events cannot "exist" sociologically except as courses of treatment, and because courses of treatment are not intelligible except through available social rule, motives cannot be located except by rule. Consequently, motives are accomplished exclusively through the use of such methods and procedures.

When a member says "He had the jealous motive to . . . . .," we expect that the statement can be understood as a description of possible behavior. Of course, there can be disagreement with the surface con-

tent of the statement—perhaps he did not have the jealous motive—but it nevertheless remains an intelligible remark because it describes an understandable or socially possible motivated action. Others do not think every such statement literal nonsense. This is again to distinguish between concrete surface phenomena or causal properties (whether the object does or does not have the jealous motive) and the analytic deep structure that makes such phenomena possible (that an observer can talk intelligibly about jealous motive, whatever the factual status of his talk).

The point here is that there must be some rule—of language, interpretation, or culture—by which motive-talk takes life as a description.

Now to say that some rule is available is to remove the sociological habitat of motive from object (the person who had the jealous motive) to discourse about the object (how it is understood that a motive ascription has been made). This is to say, again, that the sociological import of motive resides in its procedural implications for the *treatment* of objects and not in the states of the objects themselves. Motive is a procedure.

This first feature brings motive into the full corpus of sociological ideas: rules must exist for such a procedure as motive to exist. They are socially organized treatments. Motive is not something an actor has—it is not a property of an actor. It is not something the sociologist decides that some person owns, in the sense that it is the "thing" which he owns. Rather, motive is a member's method for deciding what alter owns. Thus, the sociologist does not search for motives in objects of talk and treatment, but in the talk and treatment itself.<sup>13</sup>

One socially organized condition for addressing the topic of motive, therefore, is the assumption of the availability or relevance of a motive ascription rule. The necessary availability of such a rule, or rules, can be seen by noting that motives are a

<sup>12</sup> We are not prescribing how motive-talk should be accomplished; rather, we are stating that under any and all occasions of motive-talk in our society, to which we have reference, such talk requires members to make certain assumptions about their environments. Furthermore, their talk shows that they make these assumptions. Again, whether members know these conditions or can report them to us is analytically irrelevant in the same sense that Chomsky's native speakers cannot concretely reproduce his theory even though they do show its correctness and provide for its formulability through their behavior.

<sup>13</sup> Note, though, that the concrete talk—the speech act(s), the usage—does not provide the observer with the motive (as some versions of sociology might have one believe), but such analytic status is located in terms of the observer's decision as to what must be known in order to recognize in the talk, the analytic "presence" of motive.

common sense class of events, in that to do (observe) motives is not to be exclusively doing (observing) something else (writing a play, sleeping). That motives are the topic in any case, as opposed to some other topic or no topic, requires that others understand they are the topic, which in turn requires that the socially available rule for introducing the class "doing motives" be displayed in the behavior of the introducer. As with any form of social behavior, the members themselves conceive the doing of motives to be rule-guided. Thus, one kind of common-sense-sociological rule is the motive ascription rule, and the most elemental necessary feature of this deep rule is: there are rules for the ascription of motives.

### *B. Motivated Objects Are Theorizers*

One rule for the ascription of motives therefore is that the observer-user knows there are motive-ascribing rules. A concomitant of this first rule is that the ascriber know (assume, presume) that the *object* knows there are motive-ascribing rules.

In order to be called motivated, the object of an ascription cannot be treated as if he were doing the behavior haphazardly or coincidentally. He is, in other words, treated as if he has the capacity to "know what he's doing." Any object thought to be unable to know what it is doing cannot be treated as motivated, even though that object resembles a human organism, e.g., a brute or an infant. That ascribers know there are rules requires that they impute knowledge to the object that there are rules.<sup>14</sup> Otherwise, the ascriber's knowledge could not be conceived by him to be a practical guide to the object's behavior, since the latter could not be deemed to be oriented.

Alter, as rule-guided, incorporates the

fundamental sociological principle that generally available rules are the analytic equivalent of membership (community, group, pair, etc.). Rules make actors' methodicity and concert possible by transforming what would otherwise be nonsense into intelligible social behavior. That alter can be conceived (by observing ego and sociologist) to be rule-guided encompasses the status of motives within general sociology: motive, as with any other sociological classification, refers to certain actions by rule-guided ascribers and objects. The ascriber is rule-guided in his characterization of an object, while the object is (assumed to be) rule-guided in his behavior, and thus assumed to "know what he's doing." Both ego and alter assume of one another that they are, or could be, doing motives. Each is a theorizer, in the sense that both must be looking to rules in order to carry off their activity as doing motives. For motive treatment to occur, as for any kind of membership treatment to occur, ego and alter must necessarily generate for one another their status as members oriented to rules. Here a set of concrete activities is so formulated that those activities become members through the application of a corpus of rules. This is a detailed way of saying they are members of a social relation.<sup>15</sup>

By "rule-guided" we do not mean that actors are automata governed by abstract rules, that rules are clear and unchanging and automatically applied, or that it cannot be difficult, confusing, and vague for members to act like members. We do not mean that actors are rule-governed (Bennet, 1964). On the contrary, it is those who can behave but not act whom we conceive to be rule-governed

<sup>14</sup> We should perhaps remind the reader here that the phrase "knows there are rules" does not imply "knows *the* rules." All kinds of substantive mistakes and arguments over content and application may occur in the course of motive ascription without ever denying that there are rules. What the rules are, and that rules exist, are two quite different ideas, and correspond to our previous distinction between surface and deep structure, respectively. This same distinction carries through to "knowing what he's doing." We can be quite mistaken about what it is we are doing without being treated as if we *couldn't* know, e.g., as in therapy.

<sup>15</sup> Take, for example, the brute. There are things which can be said of such an organism, e.g., it is enraged, it is contented, it is sleeping, and so forth. These are characterizations, in that they depict some state of affairs, and rule-guided ascriptions at that. But they are not, and could not be, common-sense ascriptions of motives because the brute's behaviors are not thought to be displays by a rule-guided actor—the brute is thought not to be socially "responsible." He is thought not to be responsible insofar as his activity is, in Weber's terms, behavior rather than action. Nor is the brute thought to be a bona fide member, and for the same reason: he cannot be a member because he cannot be said to know what he's doing through an orientation to member rules.

automata, for they seem only to play out as mechanisms the untransformed universal needs and drives of every man.<sup>16</sup> The distinction between rule-governed and rule-guided is comparable to the one between behavior and action, and it is surely correct that the use of rules by members is an accomplishment in the hardest sense of the term. We are only asserting that ego and alter must assume, however difficult the application of substantive ascriptions, that *some* corpus of membership rules is being used in behavioral displays before they can be characterized by substantive motive schema. It is necessary that ascriber and object are assumed to be of a certain kind, namely theorizers.

We are not suggesting either that members always neatly agree on what is happening around them, or that the substance of rules is common to all interactants. Whether behavior goes well or badly, whether it "deviates" or "conforms," is not an issue. We do not equate the deep notion of theoreticity with the substance of interaction. Theoreticity is the observed, rule-guided identification of the doing of anything at all, whether well or badly, whether deviant or conforming. (Even to be considered one or the other requires that some imputation of rule be used.)

Take the case of mental illness. If the person is conceived by members to be doing nothing in particular, to be merely a set of either random or universal behaviors, then no question of motive will ever arise (or will arise only once, to be dismissed by the finding of nonmembership). Only when being crazy can be seen as organized or rule-guided—again, as being a display of some membership—does the possibility of motivated mental patients occur.

To be theoretic is thus to be conceivable by some observer as methodically rule-guided, rather than haphazard in behavior. To ascribe a motive, among other ascriptions,

is of course to formulate the intelligible character of some behavior. The warrant for motive ascription is that the object be theoretic, that his behavior is capable of being formulated as action, just as it is the warrant for any common-sense facet of membership.

### *C. Motives Have a Grammar*

A third organized condition of motive ascription is a grammar that locates for a potential or would-be ascriber those conditions in the world which give notice that an ascription is to be done. The grammar moves a motive rule into behavior, from availability to ascription. It is a (collection of) rule(s) of use for the doing of an ascription. It depicts for the observer a procedure for actually ascribing available designations to a world populated by members. The grammar links a phenomenon in the world to the available corpus of designations.

This grammar of motives is used whenever an event is to be collected within a biography. We may think of the biography as a collection of "owned experiences," in that actors conceive themselves as having particular pasts which routinely inform an observer about the possibilities for their behavior; and we may think of events as some observer's definition of a situated environment of objects, specific to time and place. Motives are the social characterizations, generally available, the grammar which is used when biography and event are to be linked. The grammar produces for the ascriber a relation between some practical phenomenon and the common sense biography with which that event now comes to be associated through the process of ascription. A common sense biography is the observer's version of a set of owned experiences (husband), a set which comes into contact with, or is juxtaposed against, particular concrete (and hence socially problematic) phenomena in the world (dead wife). The grammar is the rule (connect the experience—husband—with the event—dead wife) that conjoins the two as an accomplishment of organized and concerted treatment (a jealous motive).

Imagine a hypothetical community where a murder is committed, and the husband of the victim is eventually identified as the killer. Initially, everyone could be suspect, and to "look for the motive" is to address

<sup>16</sup> Universals remain constants for the brute, and they govern him in the sense that they are not given the differentiated and variable expression that we think of as motivated social action. Jealousy can be a motive precisely because it is not universal in husbands. Think of thirst as a motive here—what would it be like to enact the thirst drive in such a way as to have it said that "he killed her because he was thirsty?"

the links between the murder and various collections of owned experience in the community. The ways these experiences are joined with murdered wife methodically generate the motivated (or not) character of any link.

The idea of motive thus serves to formulate for members their interactions, insofar as they conceive interaction as experiences framed in events. Actors are thought by observers to have biographies and to engage the world with them. The grammar produces the link between the two. Motives are resources for connecting an event with a biography, and they generate the event as a member of the class of experiences owned by a body (as depicted in common sense).

Similarly, motives depict for us how the event shows or displays a biography. Insofar as the biography and the event can be seen to be membership, this is done through the ascription of motive. Otherwise—in the absence of such a grammar—observers would be unable to organize the current and flow of socially intelligible events, nor could they observe the products of biography; i.e., they could not see interaction as a course of history. They would be without a temporal method. Events could only be seen to be performed and disembodied, not enacted by some theoretic-nontheoretic incumbent of a situated social world. Motives thus characterize biographies enacting events are specific to events and distinctive of biographies. They are a grammar in that they methodically collect these disparate phenomena. And they are social in that they transform what would otherwise be fragmentary series of unconnected immediate events into generally intelligible social courses of behavior. It is through motive as a culturally available designation that the observer recovers alter's membership out of observed temporal phenomena, because motives delineate the biographical auspices of acts in situations.

#### *D. Motives Formulate a Type of Person*

The grammar, then, includes some collection of owned experiences which can be allocated to the agent of the act, and some rule(s) for showing the related character of the event and the collection of experiences. Because there are alternative collections (of experience) available, the use of the gram-

mar poses a selectional problem for its user in this sense: it has to include a search procedure for deciding the relevance of one biography (one collection) as compared to other possibilities. The search procedure is essentially the rule for showing the *possible* relevance of the biography for the event. Such a rule amounts to the formulation of a type-of-person.

Thus, when users formulate the biography called "husband," the relevance of which to the event "murdered wife" is decided through a formulation of circumstances and characteristics such as jealousy, they are formulating the biography (husband) as the type of person whose jealousy could produce the event of murdered wife. In this way the grammar is a provision for explicating the link between the biography and the event and this explication is supplied by the formulation of a type of person.

The grammar of motives enables members regularly to address the fact that the relevance of biography to event is formulable through a rule which locates the owner of the biography as the type of person who would do the event.<sup>17</sup> Type of person, then, explicates the circumstances and understandings required to assert the relevance of any biography for the event. The heart of the grammar is thus its rule for formulating a type of person. It is in this sense that any observer's ascription of a motive serves to formulate for some activity, a person.

#### *E. Motives Formulate Actors' Methods*

If the application of the grammar is equivalent to the formulation of a type of person, what is required of such a formulation? Note that type of person is an identification of certain characteristics, traits, dispositions, and behaviors which make ownership of the biography relevant to the event. While the items on such a list (characteristics, traits, dispositions, behaviors) all tend to itemize concrete features of the person, they only acquire their analytic sociological interest as

<sup>17</sup> While we separate these features of the grammar for analytic purposes, they are concretely indistinct. As one formulates a biography of owned experiences presumed to be relevant to the event, this presumption can already be informed by an unstated rule which has decided that relevance as the kind of person who would do the event.

descriptions of possible ways of relating to the event, i.e., as potential courses of action. To formulate a type of person is to formulate a course of action on the grounds that no matter what one predicates substantively of persons to make their biographies relevant to the event, such relevance is only assigned on the assumption that the predicates depict a typical, possible course of action. Person, then, depicts a typical possible actor.

To say his motive in murdering his wife was his jealousy is to explicate the circumstances which make him the type of jealous person who would (could) murder his wife—that murdering his wife is one possible method available to him for doing jealousy. In this way, the event is formulated as the agent's possible method for doing whatever the formulation of the motive requires as a course of action.

He killed himself because he was depressed, or he left the party because he was bored—both are observers' ways of saying that killing oneself is a method of doing depression, or that prematurely leaving the party is a way of doing boredom. This understanding is important because members regularly raise the question of motive and address it as a sensible topic only of those classes of events which are recognizable as possible products of actors' methods. Questions such as "What was your motive in spilling the ink?" (Austin, 1966), or "what was your motive in speaking so softly?" come up in ordinary usage when something is fishy, *and* when it is sensible to ascribe to an actor some methodic display of intention or purpose. To assign a motive is then necessarily to assume that the event exhibits possible methodicity.

As we have stated, at the deepest level such methodicity is addressed in the observer's conception of the event as showing the agent's character as a type of person—by the agent's being formulable as a possible method, as it were, to "do" whatever type of person which he is formulated as.<sup>18</sup> The

observer assumes, then, that the agent shows in the event (uses the event as) one possible method of identifying himself through his action as a particular type of person: that the murder of his wife identifies him as a jealous type of person, leaving the party as a bored person, and so forth.

When an observer formulates a member in this sense, he is stipulating that whosoever he is formulating has knowledge to act under the auspices of his (the observer's) formulation of a type of person. The formulation of a type of person (of one who has a motive) thus requires the observer to assume that the one so formulated is capable of "showing" the type of person for which the observer's formulation provides. Therefore, when the observer formulates a motive, he is formulating a type of person and is required to assume that the one so formulated is a member in the deep sense of the term, i.e., as one who can generally be expected to know what he's doing in the possible circumstances where he might or could be doing it, circumstances which include those of motive ascription.

No matter what members conceive themselves to be doing when they ascribe motives, we say they are engaged in conceiving others (or self) to be doing whatever activities they do because they "own" a particular collection of experiences which can be used as a normative order for creating sensible events. Those to whom motives are ascribed are thus formulated under the auspices of the following requirements: (1) That they treat such a collection of experiences as possible grounds of action in situations, (2) that other collections of experiences are possible alternative grounds of action, (3) that the activity which is done is the actor's way of showing, to the one who ascribes, the relevance of his own collection of experiences (of showing himself as a type of person), and (4) since every kind of showing as type of person is a method of excluding other possible persons, that the actor as *this* type of person is selectively doing whatever he comes to (since according to his theoretic status in common, he could have come to something else.) It is in these ways that regular motive ascriptions are regularly introduced in regular social intercourse; it is in these ways that motives formulate actors' methods.

<sup>18</sup> Perhaps, this is what Aristotle intended in the following: "... acts are called just and self-controlled when they are the kinds of acts which a just or self-controlled man would perform, but the just and self-controlled man is not he who performs these acts, but he who also performs them in the way just and self-controlled men do" (1105b 5-9, p. 39).

## VI. SUMMARY

In summary, we have attempted to depict the socially organized conditions which members tacitly accredit in their accomplishment of motive as a sensible and observable event. We have tried to demonstrate that motive can be sociological by showing that motive shares with all matters of sociological interest (class, bureaucracy, suicide, etc.) a status as a common sense formulation. Motive depicts for any observer a course of social action.

Motive is a sociological procedure for describing how organisms show themselves as persons. To gain an identity as a person requires that disparate activities and experience can be collected by actors under some typification, a process which makes the activities graspable for them through the formulation of alter's motivated identity. The condition of membership in common makes it possible for an observer to expect that alter, because he knows what he's doing and hence is responsible, will use common rules as a standard of orientation when he displays his biography in situations, thus knowledgeably revealing his type of person.

Given (the observer's imputation of) alter's capacity for knowing what he is revealing, together with his being a differentiable type of person, the observer can also warrantably conceive alter's future, and thus organize not just the current biography and event but future links between the two as well. Motives are a procedure for organizing an historic and regular interactional future.

Most fundamentally, then, whatever a motivated actor does will show his methods for affirming himself as a person. Being responsible and capable of displaying some other collection of owned experiences, the observer is permitted to note that he nevertheless did display the collection he displayed. For any member to ascribe a motive is thus to do no less than to generate a person. It is to formulate from situated performances a responsibly displayed and differentiated collection of experience.

While each of these public and rule-guided conditions must exist for motive ascription to occur sensibly, they can of course be accomplished in a variety of substantive ways which we have not discussed here. For example, that there is a grammar under which

a motive ascription rule is invoked suggests that members regularly differentiate between activities which do and do not require the use of that grammar; i.e., there are some activities which needn't be organized according to motive criteria. In the same vein, there are undoubtedly a variety of methods in use by which members identify organisms as possible theoretic actors, e.g., the various ways it is decided that a child is enough of a member to be called a theoretical actor and so a potential object of motive ascription.

More centrally, one pervasive feature of social organization concerns members' methods for tracking and formulating biographies from a universe of possibilities, and the various rules for deciding the relevance of particular biographies as particular types of persons. The bureaucratic method, for instance, is probably very different from the familial one. All these are categorization problems which members regularly resolve, methodically producing the organization of their every day environment. To describe such particular solutions requires other papers, the possibility of which we can now begin to grasp. Being interactional and rule-guided—being a social method (just as bureaucracy, social class, or institutionalization are methods)—confirms that the status of the deep structure of motive is sociological.

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# NOTES AND COMMENTS

## COMMENT\* ON "THE ESTIMATION OF MEASUREMENT ERROR IN PANEL DATA"

Wiley and Wiley (1970) have made a contribution to the literature on dealing with errors of measurement by showing how to build a model employing the assumption of homogeneity of error variance in panel data. They argue that this assumption is more plausible than the assumption that the reliability remains constant over time (Heise, 1969). Since we have available data which allow a statistical test of which assumption is the most plausible, this note was written to give the results of this test and to demonstrate how such tests can be performed

\*The research reported herein was performed pursuant to Grant No. OEG-2-700033(509) with the United States Department of Health, Education, and Welfare and the Office of Education.

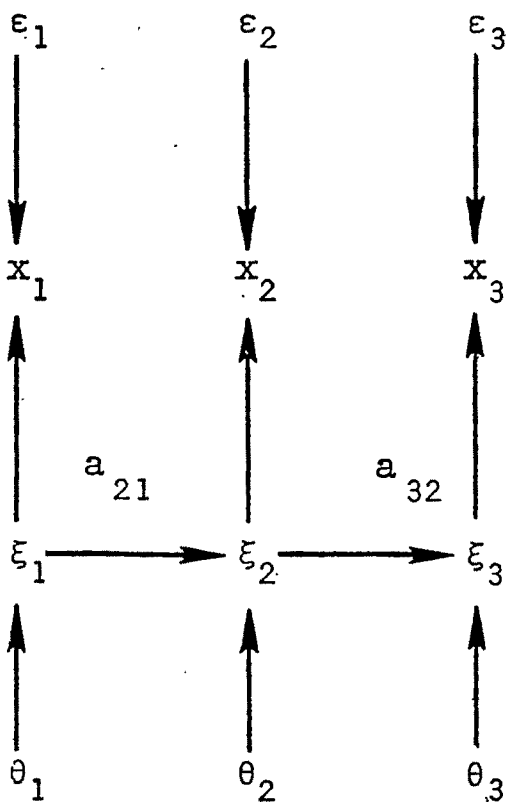


FIGURE 1. A Three Wave Model

when at least four sequential measurements are available.

The model employed by Wiley and Wiley (1970) is shown in Fig. 1. In this model the reliability of a measure ( $x_i$ ) is the square of the correlation ( $\rho_i$ ) between that measure and its underlying true score ( $\xi_i$ ). Denoting  $a^*_{xi}$  and  $a^*_{xi}$  as the standardized path coefficients corresponding to  $a_{xi}$  and  $a_{xi}$  respectively, path analysis indicates that the correlations generated by the model are:

$$\begin{aligned}\rho(x_1 x_2) &= \rho_1 a^*_{21} \rho_2 \\ \rho(x_1 x_3) &= \rho_1 a^*_{31} a^*_{32} \rho_2 \\ \rho(x_2 x_3) &= \rho_2 a^*_{32} \rho_3\end{aligned}\quad (1)$$

It follows from (1) that

$$\rho^2_2 = \frac{\rho(x_1 x_2) \rho(x_2 x_3)}{\rho(x_1 x_3)} \quad (2)$$

$$[\rho_1 a^*_{21}]^2 = \frac{\rho(x_1 x_2) \rho(x_1 x_3)}{\rho(x_2 x_3)} \quad (3)$$

$$[\rho_2 a^*_{32}]^2 = \frac{\rho(x_1 x_3) \rho(x_2 x_3)}{\rho(x_1 x_2)} \quad (4)$$

Thus, without making any assumptions about homogeneity of error variances or reliabilities, it has been demonstrated that the reliability of  $x_2$  ( $\rho^2_2$ ) is identifiable, and hence also that the corresponding error variance  $V(\epsilon_2) = V(x_2) [1 - \rho^2_2]$  and true score variance  $V(\xi_2) = V(x_2) - V(\epsilon_2)$  is identifiable. For the two outer measures  $x_1$  and  $x_3$ , only the products  $[\rho_1 a^*_{21}]$  and  $[\rho_2 a^*_{32}]$  are identifiable.

Now consider the case in which four sequential measurements are available. Making the same assumptions about the fourth measure that Wiley and Wiley (1970) made about the first three, the model in Fig. 2 is obtained. Generalizing the results of equations (2), (3), and (4), we see that in Fig. 2:

(a)  $\rho_2$ ,  $V(\epsilon_2)$ ,  $V(\xi_2)$ , and the product  $[\rho_1 a^*_{21}]$  may be identified using either  $x_1$ ,  $x_2$ , and  $x_3$  or  $x_1$ ,  $x_2$ , and  $x_4$ .

(b)  $\rho_3$ ,  $V(\epsilon_3)$ ,  $V(\xi_3)$ , and the product  $[\rho_2 a^*_{32}]$  may be identified using either  $x_1$ ,  $x_3$ , and  $x_4$  or  $x_2$ ,  $x_3$ , and  $x_4$ .

Path analysis of Fig. 2 also indicates that  $\rho(x_2 x_3) = \rho_2 a^*_{32} \rho_3$  and  $\rho(x_1 x_4) = \rho_1 a^*_{21} a^*_{32} a^*_{43} \rho_4$ , which means that  $a^*_{32}$  is overidentified. There-



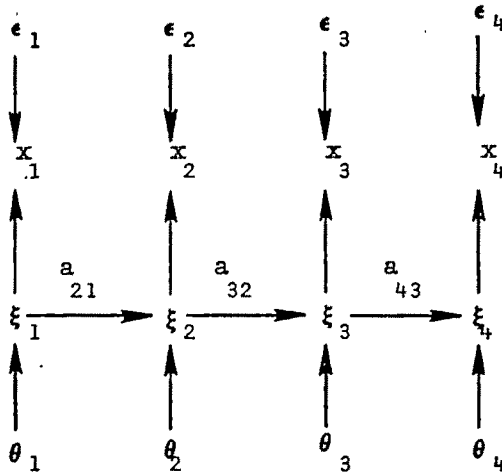


FIGURE 2. A Four-Wave Model

fore  $a_{21} = a_{21}^* \sqrt{V(\xi_1) \div V(\xi_2)}$  is identifiable. Generalizing to multiple wave panel studies, we may state that, when the assumptions of the Wiley and Wiley structural model are given, error variances, true score variances, and unstandardized regression weights between corresponding true scores are identified for all but the first and last measures. For this reason it appears unnecessary to make either the equal reliability or the equal error variance assumption for inner measures. However, one might wish to know which is the better assumption to

Table 1. Correlations for Quantitative (below Unities) and Verbal (above Unities) Test Scores.<sup>a</sup>

Grade	5	7	9	11
5	1.000	.849	.795	.779
7	.742	1.000	.868	.838
9	.718	.747	1.000	.860
11	.687	.686	.791	1.000

<sup>a</sup>Standard deviations for quantitative scores are 8.986, 13.771, 16.986, and 17.699, respectively; standard deviations for verbal scores are 11.748, 12.704, 13.756, and 14.379, respectively.

make about the first and last measures in order to identify the corresponding true and error variances and regression weights among true scores. Given at least four-wave data, suggestive but not conclusive evidence about which (if either) assumption is better may be obtained by comparing the estimated error variances and reliabilities for the inner measures.

The four-wave data to be analyzed using the model in Fig. 2 were collected in a longitudinal study (Anderson and Maier, 1963), which in-

Table 2. Model Parameter Estimates and Goodness of Fit Tests.

Model	Estimates <sup>†</sup>					Fit		
	$[\beta_1 a_{21}^*]$	$\beta_2$	$\beta_3$	$[\beta_4 a_{43}^*]$	$a_{32}^*$	$\chi^2$	d.f.	p
SCAT-V Data								
Fig. 2	.884	.960	.942	.912	.959	1.38	1	.240
$a_{32}^* = 1$	.877	.941	.927	.903	1.000	42.61	2	.000
$\rho_2 = \rho_3$	.816	.952	.952	.956	.959	2.17	2	.338
$V(\epsilon_2) = V(\epsilon_3)$	.887	.950	.952	.908	.960	12.18	2	.002
SCAT-Q Data								
Fig. 2	.851	.872	.919	.860	.925	2.78	1	.095
$a_{32}^* = 1$	.823	.840	.899	.852	1.000	42.77	2	.000
$\rho_2 = \rho_3$	.557	.899	.899	.894	.924	5.40	2	.067
$V(\epsilon_2) = V(\epsilon_3)$	.851	.873	.918	.861	.925	2.80	2	.247

<sup>†</sup>The symbol "<sup>^</sup>" denotes an estimate of a population parameter based on sample data.

cluded a group of students tested in the 5th, 7th, 9th and 11th grades with the School and College Ability Tests (SCAT), which yields a Quantitative (Q) and a Verbal (V) score. Table 1 gives (previously unreported) correlations and standard deviations on these tests for a sample of 703 males with complete data.

As Goldberger (1970) notes, the path analysis literature offers no guidance in systematic estimation of overidentified models, such as that depicted in Fig. 2. To obtain estimates, we used Jöreskog's (1970a) general method for the analysis of covariance structures with its associated computer program (Jöreskog *et al.*, 1970). The four-wave model in Fig. 2 is of the *quasi Markov simplex* type, the analysis and programming of which is discussed in detail by Jöreskog (1970b). Under the assumption that the observed distributions are normal (reasonable for these data), Jöreskog's procedure yields maximum likelihood estimates of model parameters and a large sample chi squared test is computed for testing the fit of the model to the data. Furthermore, the program allows certain model parameters to be specified as equal to other parameters or to some constant. This is useful for the present problem because the chi square fit before imposing a restriction (e.g., equal error variances) can be compared to the chi square fit for the more restricted model as a measure of the tenability of that restriction. The analysis proceeded in four steps:

1. The model in Fig. 2 was analyzed without assumptions about equal error variances or reliabilities.

2. To test whether it is reasonable to believe that  $\xi_2$  and  $\xi_3$  are perfectly correlated, the a priori restriction that  $a^*_{23} = 1.0$  was imposed. The chi square for this condition less the chi square for the first condition is the chi square with one degree of freedom for testing the restriction.

3. To test the equal reliability assumption, the a priori specification was made that  $\rho_2 = \rho_3$ . The chi square in this condition less the chi square in the first condition yields a chi square with one degree of freedom for this hypothesis. This assumption is equivalent to the assertion that the error variances are a fixed proportion of the corresponding test variances.

4. To test the equal error variance assumption, the specification was made that  $V(\epsilon_2) = V(\epsilon_3)$ . The chi square test of this hypothesis is the difference between the chi square for this condition and the one for the first condition and also has one degree of freedom.

The results of the above analysis are shown in Table 2. In step one, for both SCAT-V and

SCAT-Q, the  $\chi^2$  is small, indicating a good fit. The pattern of the estimates is reasonable in that  $\hat{\rho}_2$  and  $\hat{\rho}_3$  are approximately equal (published test reliabilities are equal and of the same order of magnitude as these estimates), whereas  $[\hat{\rho}_1\hat{a}^*_{12}]$  and  $[\hat{\rho}_1\hat{a}^*_{13}]$  are lower, as expected since they are the product of a reliability and a true factor correlation. When the assumption that  $a^*_{23} = 1$  is inserted, the  $\chi^2$  increased significantly ( $>40$ ) for both SCAT-V and SCAT-Q. The third step testing the equal reliability assumption yielded a fairly good fit, and the difference  $\chi^2$  does not suggest that this hypothesis should be rejected; however  $[\hat{\rho}_1\hat{a}^*_{12}]$  appears unreasonable since it is approximately equal to  $\hat{\rho}_2$  and  $\hat{\rho}_3$ . For SCAT-V  $[\hat{\rho}_1\hat{a}^*_{12}]$  is slightly larger than  $\hat{\rho}_2$  and  $\hat{\rho}_3$ , which would require  $\hat{a}^*_{12}$  to be greater than 1.0 for  $\hat{\rho}_1$  to equal  $\hat{\rho}_2$  and  $\hat{\rho}_3$ . In step 4 the difference  $\chi^2$  for SCAT-V is statistically significant ( $\chi^2_1 = 12.18 - 2.38 = 10.8$ ) although the absolute magnitude of the difference may not be too important. The step 4 results are more sensible than the step 3 results since  $[\hat{\rho}_1\hat{a}^*_{12}]$  and  $[\hat{\rho}_1\hat{a}^*_{13}]$  are both less than  $\hat{\rho}_2$  and  $\hat{\rho}_3$ . The step 4 difference  $\chi^2$  for SCAT-Q (like step 3) is not statistically significant. Overall, these results suggest that the equal reliability assumption gives a good statistical fit but yields theoretically unreasonable results; whereas the equal error variance assumption may yield poorer fit but estimates which are like the original model of step 1.

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### COMMENT ON "CORRELATES OF POLITICAL COMPLEXITY"

The recent article by Mark Abrahamson, "Correlates of Political Complexity" (ASR 34: 690-701), contains an interesting "Discussion" of the relationship between warfare and political complexity. My own research—some published (Otterbein and Otterbein, 1965; Otterbein, 1968a; Otterbein, 1968b), some unpublished (Otterbein, 1966; Otterbein, 1968c; Otterbein n.d.)—provides answers to some of the questions and queries raised by Abrahamson. In this comment, "answers" to three of his queries will be formulated by presenting my data on 50 preindustrial societies in 2 x 2 tables.

"Theoretically, political complexity may be related to external threat, warfare, or especially, to conquest. . . . If there is variation in common among conquest and all of these other variables [demographic concentration, socioeconomic development, etc.], it raises the possibility that the reported relationships are spurious. The key test of this potential spuriousness lies in the magnitude of the relationship between conquest and political complexity. The lower this relationship is, the less the likelihood of spuriousness" (Abrahamson, 1969:699). If conquest is operationalized as either political control (as a reason that the military organization goes to war) or military success (as measured by territorial expansion), the magnitude of the relationship between conquest and political complexity can be ascertained. Table 1 shows the

Table 1. Reasons for Going to War<sup>a</sup> by Political Complexity.

	DPP	PC	Total
Chiefdoms & States	7	9	16
Bands & Tribes	<u>30</u>	<u>0</u>	<u>30</u>
Total	37	9	46

$$\phi = .68, \chi^2 = 20.98, p < .001$$

<sup>a</sup>Defense, Plunder, or Prestige (DPP)  
Political Control (PC)

Table 2. Military Success<sup>a</sup> by Political Complexity.

	TCC	TE	Total
Chiefdoms & States	10	6	16
Bands & Tribes	<u>20</u>	<u>7</u>	<u>27</u>
Total	30	13	43

$$\phi = .12, \chi^2 = .64, n.s.$$

<sup>a</sup>Territory Constant or Contracting (TCC)  
Territory Expanding (TE)

relationship between (1) political complexity—Service's (1962) four types of social organizations provide the index—and (2) the reasons for going to war (Otterbein, 1966; Table 14 in Otterbein, n.d.). Table 2 shows the relationship between political complexity and military success (Otterbein, 1968c; Table 23 in Otterbein, n.d.).

These two tables provide only an indecisive answer; although both coefficients are positive, the correlation is significant in Table 1, but not in Table 2. For the sake of completeness, I should mention that military success (i.e. territorial expansion) is significantly correlated with a scale of military sophistication— $r_{ps} = 0.44$ ;  $p < .01$  (Table 22 in Otterbein, n.d.).

Abrahamson (1969:699) writes: "A preliminary indication of this relationship i.e. the relationship between conquest and political complexity was possible by relating the political complexity scores to measures of external warfare developed by Otterbein (1968 [a]). Unfortunately, the results of this comparison must be interpreted very tentatively. Otterbein has rated the frequency with which a sample of preindustrial societies were both the instigators and victims of external attack; however, their success, in either role, is not indicated." I am

Table 3. Military Success<sup>a</sup> by Frequency of Attacking.

	TCC	TE	Total
Continual or Freq.	10	12	22
Infreq. or Never	<u>19</u>	<u>1</u>	<u>20</u>
Total	29	13	42

$$\phi = .54, \chi^2 = 12.03, p < .001$$

<sup>a</sup>Territory Constant or Contracting (TCC)  
Territory Expanding (TE)

Table 4. Military Success<sup>a</sup> by Frequency of Being Attacked.

	TCC	TE	Total
Continual or Freq.	12	6	18
Infreq. or Never	<u>17</u>	<u>7</u>	<u>24</u>
Total	29	13	42

$$\phi = .04, \chi^2 = .08, n.s.$$

<sup>a</sup>Territory Constant or Contracting (TCC)  
Territory Expanding (TE)

indebted to Abrahamson for drawing my attention to the possible relationship between my two types of external war and military success. Table 3 shows the relationship between the frequency of attacking and military success. Table 4 shows the relationship between the frequency of being attacked and military success.

As one might have predicted, those societies on the offensive are militarily successful, while those societies on the defensive are just as likely to have constant or contracting territory as they are to have expanding territory.

Continuing, Abrahamson states (1969:699-700): "In addition, the relationship could be examined only in seven societies; none of the others were included in both samples. The results show small to moderate correlations between political complexity and both types of external war involvement. Neither relationship approaches significance ( $p > .10$ ). These results suggest, very tentatively, that the correlations between political complexity and the proposed correlates are not spurious reflections of their common variation with warfare. As indicated previously, however, the role of warfare in pre-industrial societies is a very complex issue, requiring substantially more exploration." Table

Table 5. Frequency of Attacking<sup>a</sup> by Political Complexity

	CF	IN	Total
Chiefdoms & States	8	7	15
Bands & Tribes	<u>19</u>	<u>13</u>	<u>32</u>
Total	27	20	47

$$\phi = .06, \chi^2 = .15, n.s.$$

<sup>a</sup>Continual or Frequent (CF)  
Infrequent or Never (IN)

Table 6. Frequency of Being Attacked<sup>a</sup> by Political Complexity.

	CF	IN	Total
Chiefdoms & States	5	10	15
Bands & Tribes	<u>18</u>	<u>14</u>	<u>32</u>
Total	23	24	47

$$\phi = .21, \chi^2 = 2.14, .05 < p < .10$$

<sup>a</sup>Continual or Frequent (CF)  
Infrequent or Never (IN)

5 shows the relationship between political complexity—using the index developed from Service (1962)—and the frequency of attacking. Table 6 shows the relationship between political complexity and the frequency of being attacked. These two tables essentially agree with the results which Abrahamson obtained. One difference, however, does exist. The relationship between the two variables in Table 6 approaches significance, whereas Abrahamson, using seven societies and his index of political complexity, failed to obtain a significant correlation.

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## REVIEW SYMPOSIUM

**PERSONALITY AND INTERPERSONAL BEHAVIOR**, by Robert Freed Bales. New York: Holt, Rinehart and Winston, 1970. 561 pp. \$11.95.

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Bales presents in this book what my students call his "chopper" model. It is a new conceptual scheme for interpreting interpersonal orientations in groups. Visualize yourself up in the air in a helicopter: you rise up; you drop down; you tilt and shift to the right, tilt the other way and shift to the left; you nose down and move forward or you tilt upward, pull backward. Now, if you imagine being surrounded by an interpersonal world, then your ride is like being within Bales' spatial model. Rising *up* is seeking material success or dominating others; dropping *down* is depreciating the self or being subordinate or passive; shifting *right* is positive (being affectionate, warm, agreeable), shifting *left* is negative (being resentful, cold, disagreeable), moving *forward* is commitment to group values and tasks; moving *backward* is adherence to heretical thoughts and deviant modes. To determine these various positions and directions, Bales categorizes information from these various sources: how members rate one another (Does he seem pessimistic about group ideals? Does he seem submissively good?), how members behave toward one another (asking for information, giving opinions, seeming friendly, etc.), fantasies produced in the group setting (Does the hero act in a dominant manner?) and, finally, value-statements ("It's the man who stands alone who excites our admiration"). Now, if you go up, right, and forward, you produce behavior, expressions, signals, messages, and so on that generate in those others around you certain perceptions about where you are heading (at least relative to where they are or are heading): you will seem to others to be ascendant, friendly, a group leader who wants solidarity among members and progress for the group and favors a democratic mode of decision-making. And if you were to fall off, reversing directions, you would generate perceptions that you were heading for failure and withdrawal. And so on for the twenty-four other directions. Direction is ascertained not by a fixed point, but by eliciting the subjective defi-

nitions in the minds of members. Returning to our analogy, the model is more concerned with the position of each helicopter relative to the others than in where the whole unit of helicopters is going.

The model may be used to predict coalitions and subgroup formations. The observer (participant or otherwise), (1) takes a variety of readings on the various group members, (2) locates each within the three-dimensional subjective space, (3) determines distances between positions, and (4) by applying certain principles of coalescing, suggests the more likely coalitions, subgroups and isolates. This structure can be charted and presumably tested with other independent data. Going one step further, he can speculate on those shapes (formed by positions and their interconnections) characteristic of particular kinds of groups. The reader will find one such shape for the self-analytic groups that Bales has been instructing at Harvard.

The next feature of the model is important and one to which we shall return later, for it is easy to misunderstand. When you rise in your helicopter to the northeast you are entering a region where a certain type of pilot tends to go (or more accurately, a region where it would seem appropriate for that type of pilot to go). Favored directions taken by pilots suggest personality characteristics of the pilots. For each of the twenty-six directions (or regions), Bales presents a sketch of a conceivably existent personality, or type of person, who would fit the region. Personality characteristics include: how he sees himself, what ideas and values he will express, the quality of his interaction, his conflicts and coalitions with others, personality traits such as dominance and sociability, how he sees his parents, and so on.

While it is likely that Bales considers his model relevant to the science of social psychology, he addresses his book to group participants for their practical use in understanding more fully what is going on in their groups. The style is personal and unassuming. To paraphrase, "From a long struggle in trying to understand our research findings and the dynamics of self-analytic groups, I have arrived at a schema which lends order to certain interpersonal phenomena. I have found it useful—I

present it to you hoping you will find it useful too." In this spirit, the reader is within the first few pages introduced to a series of exercises through which he begins to apply the model: think of a group and persons in it; ask this list of questions; then see how the answers relate to the dimensions in the model, up, down, right, left and so on. Almost immediately, the reader feels invited into a rather intimate working relation with the author. Just as quickly, the reviewer wonders whether he is intruding as he comments on the model before the reader has tried the exercises, and as he raises questions about it before it becomes internalized in the reader's mind. Be that as it may, it is useful to both the social scientist and the student who is applying the model to consider two major questions: First, what is Bales' model a model of? Second, where, when, and how should it be applied? Answers to both questions are helped by retracing the steps from the original sources of data to the model's final form.

One major set of original data are readings taken on twelve five-man groups meeting for ten hours each in the Harvard Laboratory. These readings include pre-session personality measures, categorizations of interaction, self- and other-ratings, guesses as to how others are rating the self, value statements, responses from in-depth interviews and so on. Another set of data consists of responses of 552 subjects to Bales' Value Profile. Arthur Couch performed a factor analysis of both the pre-session personality variables and the various domains of variables, including personality, subjective ratings, and value statements. With readings on what must originally have been around 600 variables, and with factors emerging from several factor analyses, Bales undertook the task of identifying a workable number of dimensions that took into account the greatest amount of information.

Many readers are familiar with the artistry Bales displayed in *Interaction Process Analysis* in reducing the variety of group interactions to twelve categories. He shows equal skill in constructing the present model. From an intuitive familiarity with groups he has worked with and with literally hundreds of factor loadings, he has devised a simple schema, easy to grasp and easy to remember. While based on empirical findings, the model is not constrained by them. Bales has made his choices in terms of his values of simplicity, order, and reasonableness. After arriving at his three dimensions, he relates measures on approximately 130 different variables to those dimensions. Carefully, in a 70-page appendix, he reports the evidence which justifies statements that such-and-such a reading permits labelling it either up, down,

right, left, forward, or backward, or points in between. With some evidence "clear and strong" and other "indirect and weak," he tolerates considerable slippage between reading and directional assignment. For instance, in a sample of 115 directional assignments, evidence for 71 was direct, while in 44 it was either indirect or weak.

Overall, more than 300 assignments are made. Here are some examples of how measures are related to directions: Veering to the *left* is identified by such measures as (a) high anxiety, (b) dependency, (c) disagree with others in interaction, (d) low rate in seeming friendly, (e) others rate him as resentful, (f) rates self high on understanding, (g) high on acting hostile toward others. Dropping *down* is identified by (a) making statements that devalue the self, (b) rates others high on extroversion, (c) guesses others will rate him high on individualism, (d) low on acts initiated in group interaction. Moving *backward* is identified by (a) rates others high on accepting authority, (b) scores high on wish-fulfillment, (c) rates self high on favoring expression of feelings and need for gratification, (d) receives relatively few agreements from others. And so on for all directions.

With this background, let us return to the question of what the model represents by considering what the model is *not* about.

First, the model is not of the group as a small social system. In fact, the contrast between Bales' previous models and this one is sharp and clear. In *Interaction Process Analysis*, he conceived of the group as a small social system with functional problems that need to be resolved (communication, evaluation, integration, etc.), and then classified observable behavior according to its relevance to those problems. "The group as social system" was one of the distinguishing features of Bales' work in the 1950's: system problems, oscillation between problems of adaptation and integration; the group as an equilibrium maintaining system; phase sequences as one after another functional problem is worked on, etc. The group overshadowed individuals. The personalities of members (and the error variance their differences produced) could be managed by running a large number of groups and pooling the data. From this earlier stance, the present one is an abrupt about-face, for here the themes are "what kind of person does member A seem to be to others," how are perceptions, ratings, reported fantasies, actions, and statements vis-a-vis a given actor interrelated, and what is the configuration of these interrelationships? There is literally no mention—or even a trace—of the old social system issues, and no comment on

how the present model is related to earlier theoretical schemes in *Interaction Process Analysis* and *The Working Papers*.

Second, the model is not about the relationship between psychodynamics and group dynamics, as is for example Whitaker and Lieberman's model on personal and group focal conflict. Bales makes no attempt to link group issues and personal issues or to deal with their interplay.

Third, if it is not a model of social system phenomena, nor of person-group dynamics, is it then a model of personality *per se*, or of personality in a social setting as the title of the book implies? On this point the reader can easily become confused by contradictions in Bales' exposition. On the one hand, he states explicitly that while the behavior of the person, how others perceive him, and their expectations of him may be called his social role, this role is not the same as his personality. "You see only his interpersonal behavior which may reflect only one side of his personality, elicited by this particular group. . . ." Even after adding data on the MMPI, fantasies, value statements, and so on, the data are not of the order that would provide an assessment of the actor's personality, which "consists of his relatively enduring characteristics as a total being." Clear enough and agreed. Yet, on the other hand, Bales frequently says that in placing a measure in a certain region in the spatial model one is identifying a feature of the actor's "personality or social role"—as though they were identical. And while he titled his elaboration of the 27 regions, "Types of Group Roles and Their Value-Directions," he actually sketches a conceivably existing personality, or type of person, who could "fit" each region. Does the model depict social roles or personalities? They are not the same.

The fact that the model is not of personalities becomes clear when we compare the basic data and operations with work by other writers who are clearly utilizing a model of personality. An example of the latter is Roy Schafer's use of the Rorschach in assessing personality. Using standard stimuli (the inkblots), Schafer first considers the subject's sense of reality and his conflicts and defenses in the testing situation. He then interprets responses to the inkblots as the interplay of impulses, defenses, and adaptive strivings, using sets of categories for themes (such as sado-masochism) and defenses (such as denial and projection). The output is an assessment of the enduring structure of the subject's personality and an analysis of his adaptive strivings and mechanisms of defense. While much more than Rorschach responses are needed to understand an individual's

personality, the point is that choices in Rorschach testing procedure are made to elicit information on the basic dynamics of personality. How the individual performs in one situation after another and how others perceive him are data of another order which, of course, can be systematically related to his personality. The conceptual distinction should offer no difficulty, and has become clear enough over the past fifty years to give rise to the subfield of social science called Personality and Social Structure.

If Bales says that the data reflect social role, and social role is not the same as personality, then why is "personality" in the title of the book? The following statement may indicate the direction Bales is taking on this question: "If the group role of the individual can be well assessed one can then use statistical information about the kind of personality traits usually correlated to that group role as a source of further ideas in the attempt to understand the particular personality." This suggests that the present presentation is but a first step in a more extensive research program, in which the full set of steps would include: (a) define slots in the group (K, L, M and N); (b) conduct a statistical analysis of the personality characteristics (through application of a personality model) of persons who gravitate to respective slots; (c) discover, for example, that type X tends toward slot M; (d) when in a particular instance one observes person A moving into slot M, then investigate further to see if he is or is not type X; and finally, (e) add this datum to the hypothesized relationship between type and slot. Now, unless "personality" is defined in precisely the same terms as the slot, then one needs an independent model, procedure, and measures of personality. Type and slot are not the same; personality and social role are not the same, as Bales says.

If Bales' model is not of the group as a social system, or of psycho-group dynamics, or of personality in the social setting, then what is it? Let us return to the basic data. They are the perceptions others have of a given member. "These perceptions depend upon his personality, his behavior and his position in the group as well as those of all other members of the group. The basic datum is how a given person seems to *be* and to *act*." This is to be distinguished from "personality" and from role (in the normative sense of one's obligations, privileges, restraints, and awareness of what one is free to do). In my opinion, an additional concept is needed: the ancient one of *persona*. In Cicero's terms, *persona* meant both "as one appears to others (but not necessarily as he really is)" and "the part one plays in life (but not necessarily

the part he most wants to play)." *Persona* also means "around the body," "face," and "mask." *Persona* as "mask" is jointly fashioned by the self and the other; it is a combination of how one presents oneself in everyday life and how others around one (with their own wishes, fears, desires and feelings) perceive one to be. *Persona* as "the part played" is likewise jointly fashioned, being a combination of what one *can* do and what others will give one room to do. Bales' model is a model of social masks and of behavioral roles. From ratings by the self and others, one infers features and contours of the masks; from observations of behavior and from reactions from others to that behavior one infers the set of behavioral roles being played. Certain masks belong in given regions; moving in a given direction is using a particular mode of mask-making. Mask and part played are probably related, but are not identical and on occasions may even be contradictory. The concept *persona* or mask is useful for several reasons. First, masks are real, and although what Bales is getting at through his measures is not the real person, it is certainly real enough. Second, masks influence one another; those masks surrounding one alter the aspect of one's own. Third, the concept of mask affords a degree of freedom between the outside and the inside of an individual—so that both fit and non-fit between personality and social role can be considered empirically. Finally, masks may be switched without implying that an individual's basic personality has changed.

From this viewpoint, I interpret Bales' exercise in fleshing-out the spatial model with concrete existing persons as a process of imagining the type of real person who should fit behind a given mask. His 27 types are then a set of hypotheses regarding the relationship between *persona* or "mask" and person.

The second major question concerns the conditions under which the model should be applied. It arises because here again Bales sends his readers contradictory messages. While he presents the model to the everyday group participant as a potentially useful aid in making inferences about persons in groups (useful to a wide variety of members in a range of groups), Bales is very careful to state what would seem to be highly conservative limiting conditions: operations involved in applying the model are feasible, and the generalizations he makes in presenting the model, hold only if (a) there is consensus among group members' ratings, and (b) self- and member-ratings are related (degree unspecified) both to a given member's personality and to his social role. If these conditions are to be taken seriously, then the would-be model-applier is left high and dry, for while

he may obtain some information on the similarity of ratings across members, how is he actually to know the degree to which a person's personality, his role, and his perceptions of others are integrated? The assurance Bales gives to the novice observer that there is a surprising degree of consensus in ratings, and that ratings, personality, and role are "usually" related, will not satisfy experienced researchers whose empirical tests of such relations frequently fail to show clear results. Their skepticism needs to be counteracted with procedural means to test independently the degree of integration of personality, ratings, and role. Even then, the diligent investigator will need to interpret carefully the other limitation Bales sets, which is the assumption "that the group to which the system is applied is in a task-oriented phase, is oriented to conservative assigned goals and that the task leaders of the group are themselves also likely to be conservative." Bales seems to bend over so far backward in setting limiting conditions that many readers will wonder whether the qualifications are really to be taken seriously. An alternative, of course, would be for the observer to learn what the group goal is, whether conservative or otherwise, and then relate observed behavior to that goal.

If I dwell upon these conditions it is because I sense a danger that in performing the operations for applying the model, practitioners will reinforce impressions that the appropriate conditions do in fact exist. The first impression is that consensus exists among raters, for in determining whether a person is scored *up* or *down* on a given measure an arithmetic sum is taken of other's ratings of him. If twelve rate him *up* and five rate him *down*, then he is scored *up* by seven. The operation is based on the understanding that "as in ordinary three dimensional physical space, movement upward is the opposite of movement downward. They are opposite directions: movement in either direction cancels movement in the other." This, of course, obscures differences among members. Suppose there is a value-split in the group between the blues and the reds. When the blues see person A in one light while the reds see him in another, *direction* is determined by the number of blues and reds and the reality of the split between them is lost. The second impression is that members are unconflicted, for in determining a person's direction through the use of a number of measures, the observer again subtracts indicators in one direction from those in the opposite direction. Thus when within the personality there is a conflict, or ambivalence, expressed at one time by positive reactions and at other times with equal frequency by negative ones, the opposite reactions cancel each



other out and the directional reading appears as zero or of no importance. In eliminating information on differential perceptions, the operation tends to reinforce the impression of group consensus; in eliminating contradictory actions or oscillating expressions of the individual, the operation tends to reinforce the impression that personality processes, role, and ratings are integrated. As the model is presented, it seems that the laws of geometry supersede the laws of sociology and psychology. But, of course, there is no law that says one has to take the arithmetic sum to determine direction in the space. As Bales states, he does so only as a first approximation.

In addressing his book especially to students in self-analytic groups such as those he has been teaching at Harvard (his course outline is given in Appendix 6), Bales engages in a pedagogical experiment. Will students incorporate the model? If so, what will they learn from it? If they apply it systematically from the first through the ninth month of their experience, what developmental trends and what changes in the configurations of interpersonal roles will become apparent? Presumably, in the future we will find out how they respond to his invitation and will hear about their experience in applying the model. In the meantime, from what is already known about students' experiences in such groups we might anticipate some of the issues that are likely to arise in its application. In successful self-analytic groups, the student gradually becomes able to present himself to others more directly—to be less dependent upon his mask. At the same time he becomes a more perceptive observer of others, better able to recognize the common defenses of denial, distortion, and projection, as well as the interpersonal games being played. Together these trends result in greater confidence in assessing others, and in the case of the model, would

result in progressively more valid ratings and therefore a better fit between the mask and the individual. However, the student in the meantime may have made discoveries about group phenomena which complicate the situation. He may have discovered that persons are cast by others into roles that serve the ends of the group—roles such as scapegoat, joker, hero, etc.—or learn that groups unconsciously generate illusions to protect against internal and external threat. For example, the illusion that all members are equal and feel and think alike (consensus), which protects against real and possibly dangerous differences; or the illusion that individuals are unconflicted, which protects against ambivalent feelings toward authority figures and fellow students. He will discover that group processes occur on a number of different levels, and that many of them are still mysteries to us. This is not to suggest that he can not apply the model, but it is to say that in addition to Bales' "interpersonal behavior space," the experienced student becomes acquainted with other "spaces," such as personality dynamics, group dynamics associated with collective defenses, illusions, and myths, as well as the community context in which the whole enterprise is operating. If the student misinterprets the space of the model to correspond to all these other spaces, or as the single space that contains them all, then he is likely to become confused and either close his eyes to the other spaces or discard the model. If, however, the distinctions among the spaces in the situation remain clear, and Bales' model is understood to apply specifically to what I refer to as *personae*, then the more able students are likely to take the next strategical step, which is to establish the connections between *personae* and the other systems, and thereby advance our empirical findings on groups. To the degree that this happens, Bales may very well count his experiment a full success.

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*Personality and Interpersonal Behavior* is a book of many uses. It is a handbook or workbook for use by members of "academic self-analytic groups which deal with their own processes and internal problems rather than with external tasks" (p. vi). It also serves to supplement a "natural observational approach to the understanding of personalities and groups in everyday situations" (p. 3). Of the several ways in which this book may be used, I am worried about the following:

If one proceeds as indicated in a set of simple instructions contained in the book, he may, on the basis of a few observations about the interaction of a member of a task-oriented group, classify that member into one of three categories (high, medium, and low) on each of three dimensions of participation. These are nowhere succinctly defined, although they seem identical with the Couch and Carter dimensions of *individual prominence*, *group goal facilitation*, and *group sociability*—which is perhaps not surprising inasmuch as Couch worked with Bales in developing this three dimensional system. The group member emerges from this process classi-

fied as one of 27 types. Then, by turning to Part II ("Types of Group Roles and Their Value-Directions"), one is immediately provided with an ideal-typical description of such a group member. These descriptions tell how the member sees himself and how others see him, his place in the interaction network, what ideals and values he will express, the quality of his interaction, his conflicts and coalitions with others, his personality traits, how he sees his parents, his effect on group satisfaction, and finally his relationship with each of the other 26 types. This use of the book provides a large return for a small investment, and, unfortunately, may be the way the book is used by many students. Such use is likely to be "prejudice building," giving the student the feeling that he now knows all there is to know about the group members, and that there is really no use in making further observations. Bales does encourage the reader to go beyond such use, but I think this would be difficult for unaided students in the same sense that using answers for the purpose of asking further questions is more difficult than using questions for the purpose of arriving at answers. Too much space in the book is given to classifying and typifying for the student to learn, without help, the more important task of questioning the diverse meanings and implications of what he observes.

Can the book be used to learn how to raise questions about personality and interpersonal behavior? Emphatically "yes," providing one approaches the book in this manner. Beyond the 200 pages given over to description of the 27 ideal-typical group roles already mentioned, there are about 185 pages devoted to training in techniques of observation, as well as some discussion of analysis. These include a chapter on the use of subjective impressions, a chapter on observing who speaks to whom, a chapter describing a revision of the Interaction Process Analysis scoring system and its use in observation, a chapter on interpreting latent meaning in the content of the verbal interaction-fantasy themes, and a chapter on interpreting and understanding the less latent meanings of value statements. Throughout these discussions, the student should be encouraged to follow Bales as he raises a variety of questions in trying to come to grips with what his observations are telling him about groups and their members. This part of the book, full of bits and pieces, sketches, and outlines of insight into the workings of groups, is most valuable and deserves emphasis in classroom use.

For example, in the chapter on observing who speaks to whom (pp. 81-83), Bales discusses the empirical generalization that "the number of acts a person receives from a given other person

in the group tends to equal the number that he gives to that other person." He points out that this generalization is based on much evidence, though it is not true in every instance, nor is it frequently exactly true. Yet "insofar as it is true," he asks, "what does it mean—that is, how is one to understand the reasons for it?" The discussion which follows illustrates Bales at his best: coming to grips with the meanings of his data and their implications for group process, through such techniques as raising questions, checking data, reasoning, raising more questions, and so on. He notes, for instance, that the above generalization does not hold for the top participant in the group. To understand why not, he again checks the data and notes a kind of "directional current" in the way interaction is directed.

It is as if the members of the group arrange themselves into a kind of semirestricted network, such that one person, the top participant, tends toward a monopoly on the right to address the group as a whole. Others tend to address him in disproportional amount, as if he were their spokesman to the group as a whole. But he does not return their specific communications. He is disproportionately low in returning communication to the specific individuals who address him. He continues to speak to the group as a whole. It is as if he and the others understood the arrangement as a dialogue between the top participant, or leader, on the one side, who initiates the actions, and the rest of the group, followers, on the other side, who reply in chorus.

This speculative, "as if" quality of pursuing an understanding of empirical observations lends itself well to further speculation, further inquiry, and further observation. Here the questioning process is well illustrated, and the student can learn much from these illustrations.

Nevertheless, for teaching purposes the apparent emphasis is on learning how the information gained through observations of behaviors, values, and fantasies of group members can be boiled down in order to type the members into one or more of the 27 ideal-types, or, with more quantitative refinement, to locating members in the previously mentioned three-dimensional space. The multitude of things "going on" in the group as it is observed, to which Bales frequently draws attention, may thus be lost if the student is thinking only in terms of classifying. My hope, and probably Bales', is that the student will begin with the classifying activity as a kind of orienting procedure and then go on to seek more information, raise more questions, examine deviant cases, and seek a better understanding of personality and interpersonal behavior. My fear is that he will not do this.

I disparage such classificatory activity as "prejudice building" when taken as an end in

itself. But I do not mean to imply that such classification of individuals cannot provide a more fruitful way of pursuing higher-order questions than does simply thinking about raw observations or using such low-level classifications as are given in who-to-whom interaction matrices or in Interaction Process Analysis categories. Raising such higher-order questions seems to be fairly difficult for most undergraduates, however, and the book offers very little help in this direction, although in one section Bales does use the three-dimensional classification system to pursue further questions as he develops a theory of probable coalitions in self-analytic groups (pp. 36-50).

This theory takes as its starting point the existing role structure of a group, defined by the categorization and placement of individual-member role performances into the three-dimensional framework. Using the Couch and Carter names of the dimensions, a person's role performance, for example, might be characterized as +3 points of *individual prominence*, -8 points of *group goal facilitation*, and +10 points on *group sociability*. Underlying each of these dimensions of role performance, Bales assumes a value-direction—what each individual feels is proper for himself and for movement of the group as a whole. Bales also assumes that each member would like others to support his own value-direction, i.e., be close to him in the three-dimensional space. The theory suggests that people located close to each other in the three-dimensional space—those who have similar kinds of role performances and hence underlying values—will band together in a network or coalition to try to move the group as a whole in their preferred direction. More specifically, "each person in the group will try to form an alliance, or will identify himself in a psychological sense with the one other person who is closest to his position, provided that person is further upward [more prominent] in the power structure, *unless* there is no person close enough, in which case the given person will remain either an individual isolate, or will remain the terminating upper member of a network of those further downward linked to him" (p. 37).

Directions for actually mapping out the structure and assessing probable coalitions in a group are provided, and illustrative use of such a map is provided to help students focus attention on particular aspects of group structure, and to raise questions about group processes. Certain positions in the networks are singled out for study: isolates (those too distant to be linked to anyone else in the group), terminal upper members of networks (the most prominent members of coalitions) and members

at junctions of networks (the more central members who link various subcoalitions within a given coalition).

With respect to this last type of position, for example, Bales noted in the group used to illustrate the method and theory that:

... members LD, JL, and AR were all females. In observing the group one might have gotten the impression that they performed an integrative function within their networks for this reason. Perhaps this was important. On the other hand, their position at junction points may have given them a special opportunity to function in this way. There is no reason why the present method of locating members should tend to put well-liked or attractive members at junction points. Whatever the combination of reasons, there was a visible alliance, and some joint planning outside of sessions between members LD (female) and SP (male) who fall in one network, and similarly a positive joking relationship between member LD and EM (male), and between LD and LWT (male) [these four making up one coalition with LD at the junction point] (p. 43).

We see here the beginnings of a theory of group process in terms of the values of members and their somewhat accidental relationships with others in the value network. The roles people play in groups are seen to be a function of the values they hold, and of the relationships to others in which they find themselves because of these values. Role performance as a link between personality (values) and group structure is seen as the key to understanding groups at this level of analysis. Here is the way in which classification and typification of group members can fruitfully be used. But students will probably need more help than Bales offers if they are to learn how to use these classifications to raise questions about group processes. This discussion might be extended, for instance, to deal with certain aspects of the study of self-analytic or other types of groups that have not been fully covered in the book (e.g., the development of the group over time and the rise and fall of group issues). I am thinking here of the model given by Bennis and Shepard, and of the rich analyses and descriptions of Mills, Slater, Mann, and Dunphy (all referenced by Bales).

This last point really begins to raise the question of what else is going on in self-analytic or other groups that cannot be captured with the three-dimensional system. Although the three dimensions used in this book were the most important ones that emerged from a factor analysis of several hundred variables measuring personalities, perceptions, and performances of group members, there are other dimensions. The proportion of variance accounted for by these three

most important factors is not given, but judging from some of the factor loadings provided in an appendix, my guess is that it runs about 30% for the three. If so, a great deal of variation is not accounted for in the three-dimensional system. How important is this missing information for understanding group processes?

I don't know, but I believe that a warning against premature classification and condensation is warranted. Bales' book has great potential for helping students to understand group processes, but they must be made aware of the pitfalls of premature data reduction and of data reduction as an end in itself.

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For nearly two decades, Bales has been associated with such phrases as Interaction Process Analysis, Observation Room, Phases in Problem Solving, and other aspects of laboratory studies of interaction. His current volume adds further associations, some of which may jar the unprepared: self-analytic groups (the 25 students enrolled annually in Harvard's Social Relations 120), fantasy themes, the "normal cycle of socialization," "psychoanalytic theorist in the personality area," three-dimensional model for group structure, and a typology of group roles (26 types in all), labeled with combinations of six letters indicating the possible directions one, two, or three at a time.

For Bales, there is at least biographical coherence in the earlier and current associations. His earliest research concerned therapeutic groups (Alcoholics Anonymous). He was later trained as an academic candidate of the Boston Psychoanalytic Institute. In teaching, he sought to focus self-analytic classes on observation in the classroom situation, with the aim of linking "relatively enduring characteristics as a total being" with roles (expectations of others) emerging as varying combinations of personalities interact.

The objectives of self-analysis (which Bales sees as an "ethical responsibility," p. 5), its process over time, the role of instructor (on which another book is planned), or sample outcomes of self-analysis are nowhere explicit. Therefore, this book can best be reviewed on its own terms, as a "theoretical treatise in the form of a didactic handbook" (p. 5).

Part I reads like a manual for the course, and is assigned in the course outline (Appendix 6). The writing is clear, with qualifications and references to the eight Appendices (Part III) to indicate to the student that the didactic content has some research basis. (A major difficulty with this organization for me was the continual need to turn to an appendix to discover what such basis might be.) Part II consists of de-

scriptions, one after another, of Bales' 26 role types and an "Average."

Bales is at his best when he turns to problems of observation (Observing Who Speaks to Whom, Part I; Revision of Interaction Process Analysis, Appendix 4). Quite correctly, he emphasizes the necessity in systematic research for focussed observation by trained observers using well-defined concepts. While one may dispute the utility of the particular categories Bales devised (and has here revised), his continuing emphasis on the difficulties and value of training observers is much needed. Observation is a basic method of data collection in the social sciences, yet it is sometimes regarded as easier, hence less deserving of concentrated training than, say, interview methods or questionnaire or scale design—not to speak of data analysis.

Given his intent to utilize "a good *theoretical system*, which requires only a moderate, practical amount of information, which reduces the complexity to a limited number of selected variables" (p. 29), how does Bales treat personality and roles in this book? First, he restricts interest in the personality domain to aspects he considers more salient in interpersonal behavior. This restriction rests, in part, on an assumption that "some subselves in a single total personality derive more or less directly from internalization of parental figures"—an assumption he attributes to "current social-psychological theory" (p. 15), though its correct source appears to be Bales out of Freud.

Next, he "assumes that the way values and group roles are associated . . . is . . . the outcome of the socialization of the child in group settings, and that similar conditions prevail in many primary groups" (p. 176). This assumption, in turn, bolsters one of several dubious interpretations of data used in delineating role types that must be seen to be believed. For example: "the average tendency is for subjects to describe their parents in much the same way that they themselves are described by other group members in the classroom" (p. 198); moreover, descriptions of parents by each of the role types can be inferred from the latter.

Such an assumption can readily be challenged by reference to experimental findings on human perception, judgment, and other cognitive processes, as well as those on the behavioral consequences of varying interaction situations.

The actual focus adopted in the book quickly becomes *value directions*, which "are at the heart of the theory underlying the whole scheme of this book" (p. 398). The theory is a Freudian theme, embellished with sociological terminology. The "critical" concept is that of "normal developmental cycle" (p. 176), during which parents (power-authority) "elicit affectionate feelings by giving affectionate feelings," hopefully with a balanced use of correction that keeps "money in the bank" for the relationship, if socialization is "successful" and complete (p. 256). If the balance falls so low that "insufficient funds" are signalled, the person "is never rescued from his own anxiety and hostility." The result is his failure to emerge from the cycle and value direction in which he is enmeshed at the time, as he would during "normal development" if the bank account had not gone into the red. Thus: "With some imagination, one can understand most major neurotic and psychotic symptoms, as well as major types of deviant orientation of individuals and social movements as a result of failures of the group and its culture to pass through a normal series of developmental cycles" (p. 176).

Given these simplistic assumptions, Bales' "artistic" leap from the data to a three-dimensional model of factor structure for interpersonal behavior and role types is understandable, if not warranted. Bales proceeded to a three-dimensional model on the basis of his concern to "reduce the complexity" and to leave value directions at the "heart of the theory."

I think it is more important to assess the adequacy of the theoretical model and the methods of data collection than to nit-pick over the factor structure Bales decided to use in constructing role types. I will also resist the temptation to expose the good guys and the bad guys that emerge in the role types, noting only that Bales does not hesitate to identify certain roles as prototypes for various social problems (delinquents, creators of social "chaos," etc.)—a rather astounding leap from data based largely on college students.

An over-riding consideration in this conception is Bales' view of personality, especially value directions, as a precipitate of parent-child relationships. If the issues were as simple or as settled as he implies, developmental and personality psychologists should be basking in the warm light of public life as advisors, even as policy-makers, rather than re-assessing—as they are—their own work on effects of early child-

hood experiences, child-rearing practices, peer group membership, neighborhood environments, educational practices, mass media, and other influences from the social setting. Bales is simply in error in taking this set of assumptions as generally accepted or acceptable when, in fact, the assumptions are parochial and not proven.

Perhaps the ease with which this error is committed is related to Bales' conception of the human group. While regarding interpersonal behavior as reflecting only "one side" of personality elicited by a particular group, he seems to indicate that the structure itself is chiefly the "way in which personalities, or selected sides of personalities, fit together" (p. 17). The implication is even clearer in descriptions of the role types, where predicted "conflicts and coalitions with others" rely heavily on mapping proximities and distances between value orientations and personal characteristics of participants. Thus, by implication, power (the upward direction in the three-dimensional model), while a legitimate property of parents, is here a product of dominating individuals and the efforts by those less dominant, but with different values, to check the domination (which, in Bales' classroom, amounts to talking a lot). Aside from personal qualities (tact, warmth, etc.), there is little indication that other resources or social criteria contribute to power. Bales mentions the importance of status outside the group in establishing role relations, but he does not introduce outside status, or male-female roles, or outside group cleavages (such as religion or race) into his role types. The great reliance placed on paper-and-pencil tests of personality and attitudes (the value profile) in constructing the factor structure for roles is a further basis for criticizing the concept of group structure, especially since the validity of such tests in predicting interpersonal behavior is greatly in doubt.

If the concept of group has utility in social science, it refers to *relationships* among a set of individuals developed during interaction over time. Bales recognizes such properties chiefly in discussing fantasy themes (Chapter 7). Fantasy is related to behavior as "unconscious aspects of the mind" are to "conscious aspects, that is through many distorting and concealing defenses" (p. 137). Group fantasy, then, turns out to be a "chain reaction" or contagion with "a mysterious power to drag the interaction of the group into some extreme form" (p. 138). While fantasy is essential to group interaction and, in time, "turns into a new social reality and constrains where it formerly liberated" (p. 154), it is also "sovereign social medicine" whose "proper dosage has often been exceeded" (p. 140). Hence, the mysterious chain reac-

tions can be serious in the "more volatile forms one finds in a riot; the mob-scene; a lynching; a panic"; or "social movements and revolutions" (p. 136).

Thus Bales disposes of the problems of norm formation, normative regulation in groups, and norm change through fantasy. His "value directions" are already properties of persons as precipitates from earlier parent-child interactions. It is important to realize, on the one hand, that Bales explicitly identifies group fantasy as the source of group culture (pp. 151 ff.) and, on the other hand, that he instructs that events should be coded as group fantasy when they do *not* refer to the "here-and-now." For him, group products apparently have little to do with members' attempts to solve problems, achieve goals, face a sometimes hostile environment, or to deal with nature.

One exception to the criteria for fantasy aptly illustrates what I regard as inadequate in Bales' conception of a group. As fantasy, he includes references by participants to the observation room, which is large enough to accommodate 25 participants and 25 observers, and includes a large set of three mirrors for observation, an oval table, and a red rug. His grounds are that the room's features "are not explained by anything about the group. They were there 'in the beginning'" (p. 148).

This curious logic contradicts Bales' own cautions that his role types are applicable only to the situations he has studied. More seriously, it ignores the fact that any group forms in an environment whose features and facilities affect interaction from the beginning. In the present case, Bales is also committing a methodological error that is not easily excused in the light of recent work on the "social psychology of the research situation." Beyond a few hints in the role type descriptions (especially those types who withdraw or remark about feeling like a guinea pig), there is little attention to the possibility that the process of being observed in the elaborate observation room has a significant impact on interpersonal behavior.

Bales also neglects the nature of the tasks and problems faced by participants, another set of variables that affect whether a group will

form in the first place, the criteria for status (power), and conformity-deviation. The tasks in his class are talking about self, others, content of the course—all in the framework of human relations problems. Such tasks and interpersonal problems are by no means typical of human groups, including families. The assumption that one can analogize from interpersonal behavior in self-analytic classrooms to the family or to other human groups is simply unwarranted on the basis of research showing the impact on behavior of the location of interaction (including the methods of study), the motivations of participants in selecting problems and executing tasks, and the kind of normative regulation of behavior and interpersonal relations (status-roles) that arise during interaction.

In short, although one would suspect that over a period of two semesters, groups with distinctive properties (at least a set of status-role relations and norms) would form in the observation room, there is little evidence for such suspicions in this book. There is indication that the relatively large number (25) is conducive to the formation of sub-groups or factions (coalitions). If so, perhaps the outcome is more accurately termed an *intergroup* system, in which the roles and behavior of participants take on different properties, depending on whether interaction occurs within a faction or between factions. If this is the case, several didactic suggestions in Part I would be misleading.

In keeping with Bales' own preference, the didactic portions of this book should be evaluated with reference to the adequacy of the theory underlying them. The result of a great deal of hard work in research and analysis by many persons, this book illustrates once again the futility of analysis of interpersonal behavior that excludes the major properties of human groups, the properties of the setting in which the group forms and functions, and the implicit cultural assumptions that govern both the collective goals and the study of those who share them. It is difficult to believe that learning experiences based on its instructions could overcome such limitations.

## REVIEW ESSAY

**THEORETICAL SOCIOLOGY: PERSPECTIVES AND DEVELOPMENTS**, edited by John C. McKinney and Edward A. Tiryakian. New York: Appleton-Century-Crofts, 1970. 538 pp. \$10.00.

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In the thirties and forties, according to Lipset and Smelser, the main lines of American sociology were laid down by Parsons, on the one hand, and by Lazarsfeld, on the other. Parsons built on the convergence which he claimed to detect in European sociology to produce his structural-functional approach, while Lazarsfeld insisted upon the translation of theory into quantitatively testable propositions. The present volume, edited by McKinney and Tiryakian, shows how, thirty years later, the main theoretical schools of sociology have developed. The best characterization of the book is to say that it involves a critique of the Parsons-Lazarsfeld view of the world in two directions. On the one hand, it criticizes this view for being too insensitive to social change; on the other hand, it accuses their perspective of an inadequate epistemology which leads to methodological oversimplification.

No less an authority than Wilbert Moore, co-author of one of functionalism's most central theses ("Some Principles of Stratification"), leads the attack on the first issue. Not only does he accept the criticism that the mainstream tradition of sociological theory is static, but he also insists that the same static quality characterizes the "progressive refinements in data analysis." What we have to do, therefore, according to Moore—and also according to Marion Levy, in his advocacy of "comparative analysis"—is to work towards a quantitative sociology based upon a "system of sequences."

Etzioni is another major figure who in recent years has dissociated himself from the functionalist consensus, and has expounded what he claims to be a viable alternative to Parsons' system in his book *The Active Society*. The basic concern of this approach is not, as in the case of Parsons, the problem of order. Rather, it emphasises control of the social system in the interest of values. Given that there are shared values, the sociologist may give advice concerning the best way in which such matters as the discovery and channelling of information may be arranged, in order to ensure that values

are realised most efficiently in progressively changing conditions. Etzioni makes his own contribution to the McKinney-Tiryakian volume, and Wendell Bell and James Mau also offer a "cybernetic-decisional" model" similar to Etzioni's, in which they urge that we should include the notion of "images of the future" as a variable in sociology.

Such trends and developments as these in the thinking of leading American sociologists are exciting. They are still too new, however, to have yet been subjected to a thorough methodological and philosophical critique. One would want to know far more about the "why," as distinct from the "what," of social change in the kinds of studies advocated by Moore or Levy, and in the case of Etzioni and his followers one would want to look far more closely at the problem of value judgments being smuggled in as statements of scientific truth. Such issues need to be raised by someone as stubbornly consistent as Robert Nisbet, as in his critique of both classical macro-theories of social development and micro-theories of change as they occur, for instance, in the more sophisticated versions of functionalism. The basis of Nisbet's critique lies in Durkheim's statement that "all that we can observe experimentally . . . is a series of changes among which a causal bond does not exist. The antecedent state does not produce the subsequent one, but the relationship between them is exclusively chronological." It may very well be true that any move towards a theory of social change and development, as distinct from a respect for the fact of historical change as it occurs, must begin by trying, if it can, to disprove this proposition. None of the writers in this volume who advocate a more dynamic sociological theory really appear to do so, however.

Far more fundamental than this discussion of static versus dynamic theories, however—and, one might say, logically prior to it—is the question of the nature of the subject matter of sociology. Neither the static nor the dynamic theories presented in this volume deal adequately with the basic epistemological questions of the human studies, which creates considerable uncertainty as to what it is we are actually

talking about. There seems to be considerable openness about what data can be regarded as fitting various theoretical terms, but little, if any, consideration of the problem of dealing with events that have "meaning." Nowhere is this more evident than in the contribution of Parsons. Despite the fact that his early writings advocated an action frame of reference as the basic standpoint of the human studies, what he is looking at now are simply the attributes of supra-human systems. Moreover, he now proposes to introduce the mechanistic notion of "interchange mechanisms" at the general action level. Thus, the notions of meaningful action and voluntarism are to be replaced. The Parsonian system has turned back on itself, destroying what was most fruitful in its starting point.

Far more hopeful in its approach is the essay of James McKinney, who sets out from the standpoint of symbolic interactionism and phenomenology. He does remind us that sociology, along with the other human studies, makes constructs about the constructs being created by the actors being observed, and one is grateful for this. But the problem is more complex than this, and if one understands this complexity he cannot accept the contentions that Parsons is the greatest typologist of all or that Lazarsfeld has taught us all how to operationalize our typologies. For the real sociological subject matter is encountered only when the sociologist makes constructs of the constructs that actors make about *social relations*, and these constructs themselves involve the assumption of one actor making constructs to explain the behavior of others towards whom he orients his conduct. It must be clear that neither Parsons nor Lazarsfeld is talking specifically about this; and, impressive though the sweep of McKinney's eclecticism may be, one is still left uneasy after reading his argument that the real issues of sociological methodology have not been faced.

At this point the issue remains to be settled between positivist empiricism, as represented by Lazarsfeld and Blalock, and phenomenology, represented in this volume by an essay by the enigmatic Harold Garfinkel. Probably most readers will follow their biases and settle for one alternative or another, and I must confess that my own bias is in the phenomenological direction. Nonetheless, one cannot avoid being struck by the gap that has opened up between these two alternative approaches. Moreover, I would suggest that American sociology would do well to return to the work of some of the masters of sociology in Europe, in which the tension between the two was fully appreciated and maintained.

No one who has read Blalock's *Towards a Theory of Minority Group Relations* could doubt that he offers theoretical and historical insights of a high order, and that he has brought great sophistication into empirical research methods. Nonetheless, there is a huge gap between his intuitive theoretical thinking and his research designs. It is not good enough simply to accuse the great tradition of sociology of forgetting propositions and concentrating on "frameworks and perspectives," and then simply go on to say "I shall be concerned in this essay with causal assertions of the form 'an increase in X will produce an increase in Y'." Unfortunately, the world with which sociologists have to deal is not one that can be reduced to propositions of this order. Nor can one regard Lazarsfeld's claim made here that "variate language" can take account of intentions, process, structure, context, and qualitative factors as having been sustained. The important thing about "intentions, process, structure, context, and qualitative factors" is that they all refer in one way or another to meaning, and one feels sure that the European sociologists to whom Lazarsfeld addresses himself will recognize that he has simply not understood or has avoided the problems posed by the meaningfulness of social phenomena.

McKinney accuses Garfinkel of dealing only with actors' constructs and failing to recognize that sociology must deal with constructs of these constructs. This is probably true. But it may also be that in proposing a new discipline of ethnomethodology, Garfinkel simply fails to rise to the level of sociology, which should be concerned with complex forms of social relations. Nonetheless, set against the crassness of modern empiricism, Garfinkel would appear to be directing sociologists in the direction in which they should go. The structures of assumptions revealed in the meanings attributed to situations by participant actors may well include assumptions about the nature of social relations, and as one stage in sociological investigation ethnomethodology is of the first importance. Because of this importance, American sociology should demand that Garfinkel and his friends strip what they have to say of some of its peculiar phraseology and syntax. The aim of a scientist, sociologist, or ethnomethodologist should, after all, be to influence his contemporaries, not simply to create a cult amongst his consociates.

What seems to be most lacking in this volume is the approach to sociology that was particularly evident in the work of Max Weber and Georg Simmel, but which seems to be missing from most contemporary American sociology.



This approach sees the object of sociology as understanding historical social structures as meaningful phenomena. Once this is understood, the dividing up and trivializing of our methodological heritage will cease. The static quality of social structures will disappear; the fact of social conflict, which Bottomore treats as a special subdiscipline within sociology, will move to the center of the stage; and it will no longer seem puzzling that, as Tiryakian shows, the key to changing social structure will often be found in the new cultural expressions of religious and other movements.

One of the difficulties about this volume is that different phases of a single process of research and logical reasoning are presented as though they were rival schools; what is important in any particular article is lost in the process. Had the contributors shown the modesty of allowing that there are other phases of the research process, their pieces might have added up to a fairly comprehensive account of the major theoretical and methodological problems of the discipline. Blalock is particularly misleading in this respect in criticizing sociological theory for being too much concerned with concepts, frameworks, and the ideas of "specific men." In fact, the beginning of wisdom for contemporary sociology in America would be a return to the first chapters of Durkheim's *The Rules of Sociological Method* and Weber's *Economy and Society*.

What emerges from the classical tradition as developed by, notably, Durkheim and Weber with regard to the relation between theory and research is that

1. Sociology is concerned with the study of structures of social relations.
2. Sociologists can understand these structures only by making type-constructs of the constructs which actors make of the social relations which form part of their everyday world.
3. Such structures may be thought of as constituting a normative order rather than as representing mere statistical trends.
4. Nonetheless, there is a problem involved in the study of social structures. Norms are not universally followed and mere statistical trends do not of themselves constitute a social or normative order. The problem is to discover the incipient social and normative order, different from the formally recognized one, which lies behind the statistics.

If this much is accepted, some of the apparent contradictions between the phenomenological and positivist approaches to sociology would immediately disappear. The value of modern refinements of quantitative methods is simply their suggestiveness, the fact that they get at subtle processes of change in social structures, which the study of legal and moral codes over-

looks. But no statistical association is sociologically relevant unless it can be interpreted in terms of meaningful social relations and action.

There remain two other issues which must be raised in evaluating a symposium such as this: the value of systematic theory and the purpose of sociological inquiry. Several writers have difficulty with the concepts of structure or system; even though they put the concept of *structure* at the center of things, the term is used in widely different senses. What is remarkable is that none of the authors has the classic sociological grasp of the main economic, political, and legal structures that have ordered the lives of men in history.

In regard to the purpose of sociology, the scientific orientation of many of these authors suggests either a world subject to natural law, in which the task of the sociologist is to detect degrees of association between phenomena, or a world following evolutionary sequences, in which sociologists are to tell their fellow men what their future must be. Alternatively, one detects a more ameliorative orientation based upon piece-meal social engineering. Against these views of the purpose of sociology another may be offered which is virtually unrepresented in the symposium: that sociology seeks to understand social structures in order that man should be better able to control them. Such an approach recognizes the brute reality of the social order but, in recognizing it, seeks to order it and, thereby, in some measure, to help man to freedom. Strangely, when this perspective is offered, the true significance and value of many of the articles in the volume emerges. What we are trying to do in sociology is, as C. Wright Mills pointed out long ago, not adequately accomplished either by brick-by-brick empiricism or by grand theorizing. These are the pathologies of sociological method, and it is such pathologies which deform and distort the real insights to which this symposium shows American sociology to be striving.

The fact that the selection of articles chosen by McKinney and Tiryakian raises questions of this kind, however, shows the judiciousness of their selection. There can be little doubt that their book will produce a clash of ideas which will stimulate graduate students for some years to come. Moreover, the argument will be given an extra dimension by virtue of the fact that the volume also includes important pieces on sociology's relations with economics, psychology, history, and cultural anthropology. The problems they raise are all illuminated by the basic theoretical discussions in the more general articles, but these cross-discipline pieces also give the theoretical discussions more specific point.

## BOOK REVIEWS

*History and Social Theory*, by GORDON LEFF.  
University, Ala.: University of Alabama  
Press, 1969. 240 pp. \$6.50.

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Leff, a British medievalist with a philosophical bent who has put the Marxist conception of history to the test of its own pretensions in his earlier publication, *The Tyranny of Concepts* (1961), comes now with a book devoted to the examination of certain enduring philosophical and epistemological issues confronting history as a science. Some of these issues were at one time argued extensively and with great ardor by philosophers of neo-Kantian persuasion, pre-eminently Dilthey, Windelband and Rickert, concerned with establishing the lines of demarcation between the *Naturwissenschaften* and *Kulturwissenschaften*, as well as their specific methodologies and objects of cognition. Some of these issues were introduced into the discourse of modern sociology by Max Weber, whose own position was deeply influenced by these men, particularly Rickert, and in one form or another they are a part of the *fundus instructus* of even contemporary discussions concerning the nature of the social sciences and their interrelationships.

This is not to intimate that Leff's work is in any sense a recounting or rephrasing of the epistemological discussions of the past. Informed by contemporary analytical philosophers and thoroughly acquainted with the corpus of classical Marxism, Leff presents a lucidly formulated and cogently reasoned case for history as a cornerstone of all social studies and as a condition for all understanding which assumes contingency in human affairs. His conception of history is latitudinarian: history constitutes the totality of human experience and "enables us to comprehend what men are through showing what they have been and how they have become what they are."

The book is written calmly and, even when he subjects to searching criticism such conceptions as the Popper-Hempel "covering law" theory of history or the Marxian theory of class, without a noticeable polemical thrust. But no sociologist reading it can possibly ignore those parts of its analysis which contain an immanent critique of the ruling assumptions of much of modern sociology fashioned on the natural science model.

It would be a disservice to Leff's careful argument if one were to read into his essay on historical knowledge too many implications for sociology. But it would be otiose to pretend that historiography and sociology co-exist in watertight compartments and to overlook the many conceptual and epistemological problems they share. The least that a sociological reader can do is to re-examine his often inchoate assumptions about the nature of the social world and of sociological inquiry and to pause in his headlong search for empirical uniformities which on closer scrutiny are so often revealed as merely severely circumscribed coincidences.

The historian is enjoined by Leff to be undaunted by his failure to discover a specific body of universal laws. History is an intellectual process which seeks to represent and reconstruct the world as it was; making the past intelligible should be considered the sole criterion of success of historical explanation. History is not a search for laws of universal development or even a search for causes but rather an endeavor to identify in a welter of possible relations and complexes those factors or elements which produced a particular situation. The circumstances to which historians pay attention are not of men's choosing but are largely of their own making. The element of indeterminacy and uncertainty in human conduct as well as men's capacity to create what previously did not exist, coupled with the lack of structural continuity in human affairs, make it impossible for the historian to reduce the relation of events to one of cause and effect. Whenever causal explanation in history is attempted, it breaks down as it moves outside the simplest sequence; most historical events are indeed multiple and variable complexes which can be only described qualitatively. Thus, historical propositions are of necessity contingent, existential, time-bound, and contextual.

The author's discussion of the nature of historical knowledge and historical explanation is ingenuously organized around three great antinomies of historiography, i.e., that between the incoherence of actual experience and the coherence of recounted history; that between contingency and irrevocability; and that between the universal and epochal. It is in connection with the third of these themes that we are introduced to a brief but incisive treatment of historical evaluation and periodization, i.e., the

framing of historical narration and analysis by epochs, properly seen by the writer as conceptual wholes or ideal types, enabling the historian to break down the generalities of human conduct into the norms of its different epochs and to demarcate one historical context from others, to attain the contextual, intrinsic, and comparative frame of reference which alone gives the assurance of valid historical judgment.

The concluding chapters of the book deal with ideology and social class, neither of which is brought into a sufficiently direct connection with the main body of the book. The student of sociology of knowledge will be somewhat disappointed by the at once apodictical and selective treatment of the complex problem of ideology, but much of it is instructive and illuminating nonetheless. The chapter on class is also rather sketchy but useful as a well-argued reminder of the logical and empirical tenuousness and historical groundlessness of all attempts to use social class as a universally explanatory variable.

Leff's thinking has been evidently inspired by Max Weber, even though he is not uncritical of him. His sympathy is clearly for an empirical approach to the subject matter of history, for pluralistic interpretation, and for conceptual flexibility, which is to say against all unitary, deterministic and universalistic explanations, claiming to explain so much but in reality explaining so little. They will, however, never lack adherents; and books like Leff's will always be welcome to warn us against the pitfalls of all manner of epistemological and methodological primitivism.

*Sociology Tomorrow: An Evaluation of Sociological Theories in Terms of Science*, by PETER PARK. New York: Pegasus, 1969. 181 pp. Clothbound, \$6.95. Paperbound, \$1.95.

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Few sociologists would or could take on the burdensome task of combining in a slim volume a rudimentary discussion of elements in the philosophy of science such as the nature of explanation, conceptualization, and the assessment of evidence; provide examples of their application to the physical sciences, criticize different sociological works in terms of them, determine where sociology is and where it is going; and address the entire statement to an audience of general readers as well as professional sociologists. This is the task Park attempts in this book; and although he makes a noble effort, he is only partly successful.

The book is divided into five parts composed of an Introduction, three major sections and an Epilogue. In the short Introduction Park indicates his concern with addressing himself to sociology as a potential science (it presently is not in that it has been "singularly unproductive" of general, valid propositions) and not to such nonscientific aspects as preaching and philosophizing about social issues. The discipline is at a "pre-scientific" stage of development because of its failure to emulate the natural sciences and because it has learned its "science" from philosophers of science who deal with the evaluation of finished products and not the ongoing task of building a science. Another specific obstacle historically has been sociology's "conceptual heritage." Park hopes to show how a science of sociology can be made possible by dropping this heritage and charting a new course.

The first section, "Science," contains chapters on evaluative standards derived from Park's conception of science. In the second section, "Sociological Explanation," Park applies standards in a critical analysis of existing "sociological theory." He says that he is concerned with the "style of explanation" and not specific "content." A chapter on "Understanding Capitalism" contains a critique of Weber on capitalism and his "methodology." In another chapter a critique of functional analysis is addressed specifically to Davis and Moore's theory of social stratification. The final chapter in this section, "Formalizing Common Sense," criticizes Homans' attempts to develop deductive sociological theories and James Davis' formal analysis of balance theory. The third section, "Measurement," deals with standards in sociology in regard to the assessment of evidence. In addition to a discussion of "Quantification" and a straightforward chapter on "Measurement and Mathematical Expressions," Park argues that dealing with "anomie" involves an exercise in "measuring the unmeasurable."

Park concludes with a call for a sociological "Copernican Revolution" that will offer a new conceptual framework based on something other than Weber's "intuition and introspection" as a method of inquiry, a method which he feels has helped prevent the scientific growth of sociology.

On the positive side, Park's discussion of explanation will be helpful to some in clarifying such matters as the role of indirect evidence in explanation, the relationship between prediction and explanation, and the distinction between universal and statistical generalizations. Park also makes a good case for the view that, although formalizing theories may make the theoretical language less ambiguous and the relationship between propositions clear, this is

of little help if the theories are empirically vacuous.

On the negative side, Park treats "science" as invariable when the process of scientific inquiry may reasonably be identified and characterized in various ways by different evaluative standards. If one uses very "rigorous" evaluative standards, as does Park, then obviously all sociological work falls short, as he asserts. Obviously alternative, less rigorous evaluative standards are more appropriate to sociology's present level of development. Another and related problem is Park's failure to note the *issues* involved in the selection of evaluative standards. Even more critical is Park's failure to distinguish between the *context of discovery* (the thought processes and social influences that give rise to a theory) and the *context of justification* (the formal analysis of the completed work). Park confuses the way a scientist comes to a given conclusion with the empirical claims asserted by the conclusion, apparently failing to appreciate that there is no necessary logical connection between evaluative standards and strategies on how to develop a science.

Despite misgivings about this book, I recommend it because it does address problems of explanation and conceptualization in a much more intelligent manner than do sociologists generally. But one should be mindful of the "programmatically" character of Park's criticism and guard against its potentially damaging effect.

*Value Judgment and Social Science: Structures and Processes*, by EUGENE J. MEEHAN. Homewood, Ill.: The Dorsey Press, 1969. 159 pp. Paperbound. College price, \$2.50.

*Objectivity in Social Research: The 1967 Wimmer Lecture*, by GUNNAR MYRDAL. New York: Pantheon Books, 1969. 111 pp. Paperbound. \$1.95.

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The title of Meehan's book might be somewhat misleading to those social scientists who conceive of values as desirable *states* of affairs, general directions toward which human action is directed. Quite to the contrary, Meehan treats values as rules for action which ideally *should* lead to the fulfillment of human expectations in a given situation. *Value judgment* is the process of *rationality* selecting one of at least two possible courses of action, both of which purportedly would lead to the goal. Meehan criticizes traditional moral philosophy for its irrelevancy, triviality, obscurity, and general

uselessness for the social scientist and his problems. He therefore focuses upon the "way in which value judgments are made . . . , not to make value judgments" (p. 3) and quickly reaches the conclusion that, since value judgments are always made and *tested* with reference to some concrete empirical situation, they must therefore depend absolutely upon empirical evidence and rational calculation. Idealism, emotivism, and rigid empiricism are rejected as bases for value judgment.

Chapter Two is concerned with explicitly defining value judgment in terms of the following set of assumptions about human nature outlined by the author: (1) man can acquire information about his environment only through his sensory apparatus; (2) propositions do not relate to the "external" world but to human perceptions; and (3) man cannot assert anything regarding "truth" about the empirical world and must judge the worth of his assumptions and propositions in terms of their utility or usefulness. Chapters Three and Four elucidate from a systems perspective the factual base and the situational component within which value judgments take place. Both chapters are heavily laden with scientific "jargon" and a couple of the tables defy interpretation within the evaluative context. Chapter Five discusses how value judgment *should* be applied, using several simple examples, and attempts to provide an "ethic."

Meehan's work suffers from a continual shifting in Chapters One and Two from "factual" to normative perspectives on the process of value judgment. Although Meehan is concerned with *how* value judgments *are* made, the remainder of the book is addressed to how they *should* be made. In considering how value judgments are made, he has added little to the ideas and theories of Dewey, Mead, Pierce, W. I. Thomas and Weber; it is somewhat disquieting to see virtually *no* references to their work. Due to the highly technical and scientific nature of the evaluation process, apparently few of us are competent to make value judgments—particularly with respect to critical issues. Indeed, Meehan is quite critical of historical and contemporary examples of value judgments which have been made on any other bases (tradition, emotion, outmoded and perverse value systems) and suggests that it is quite possible to train competent critics to serve as an elite decision-making corps (pp. 42-43). His emphasis upon a highly *situational* ethic assumes that no overall system of value judgment either in terms of means or ends prevails (or perhaps should prevail). Indeed the search for universal rules for evaluation "is as futile as the search for a universal map" (p. 44). The idea of a societal ethic is

denigrated, although Meehan does suggest that social criticism in most parts of the world does focus on factors related to brute survival, good health, food, education, and the general seeking of pleasure and avoidance of pain.

Myrdal places emphasis upon making explicitly clear the value orientations underlying any research endeavor. He points out that most of our actions are directed toward the fulfillment of personal values in concrete situations (opportunistic) which may conflict with values held on a higher plane. This conflict is particularly acute when opportunistic beliefs are false; social science should strive to purge these false beliefs through rational education. To support his contentions, Myrdal draws examples from the circle of economists with whom he was associated in Sweden in the first part of the 20th century and from his own research in the United States and Asia.

Myrdal's little book can be read with ease and profit by undergraduate students from a variety of social science-related fields. Meehan's book, on the other hand, will probably appeal to professionals and advanced graduate students. Both are worth reading—although perhaps for different reasons.

*Social Psychology and Human Values: Selected Essays*, by M. BREWSTER SMITH. Chicago, Ill.: Aldine Publishing Co., 1969. 438 pp. \$12.50.

RICHARD M. EMERSON  
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When an author offers a collection of his own previously published work, a reviewer is tempted to comment more upon the man than upon the book. The temptation is extra strong in this case, for the essays collected here produce a book with a distinctly autobiographical flavor. Technical-substantive aspects of social psychology, while not totally ignored, assume a secondary place behind Brewster Smith's view of his profession, where he feels it should go, and what he has done to move it in that direction.

The first chapter is new. Entitled "Social Psychology and Human Values," it must have been intended to set the tone, professional-autobiographical, for the book. We are introduced to M. Brewster Smith, a liberal and free-thinking man who chose social psychology during his youthful search for "social significance," and follow him on a short but fairly intimate tour retracing his professional development, being introduced to his colleagues along the way. (I am tempted to say to his kinsmen such as Gor-

dian Allport, Henry Murray, Samuel Stouffer, and Arnold Rose, for his feelings come through in filial and brotherly tones.)

We thankfully encounter the question of "human values" in social psychology not through any analytic weighting of the virtues of applied versus pure research nor through direct discussion of academic scholarship versus activist participation. Indeed, the word *relevance* does not occur anywhere in these pages. Rather we are exposed to essays on the nature of mental health, to some of the main questions, partially intellectual-philosophical and partially personal-moral in character, which continue to bug M. Brewster Smith. On the "issue" of human freedom and free-will versus determinism: "In the course of human history most people have had precious little of it. In the affluent societies of the contemporary world they have more of it than ever before, but they enjoy it insecurely. It can be frightening. Too few of them have been raised so as to be endowed with inner capacities for freedom—responsiveness as a person to the wishes of the self—that match their widened range of outer opportunity. Free will is itself caused; it is an achievement, not a paradoxical assumption." From this it follows that Smith should be interested in "value oriented research on socialization," which "can throw light on the circumstances that enhance or hamper man's attainment of freedom." The peculiar, maybe even happy, fusion of value and intellect in Smith's perspective comes across in observation such as this: "How to fit voluntaristic choice into a deterministic science is a truly basic psychological problem, not 'merely' philosophical, one that has perplexed me since my days of college bull sessions."

After 26 previously published essays the book closes with a new piece entitled, "Some Thought on the Legitimation of Evil." This essay is cool and careful in style, detached and analytic in much of its substance. Even so, it is a fairly passionate statement by an old style liberal who is in touch with the turmoil of the contemporary scene. The illiberal character of much current activism comes under ambivalent attachment as one of the troubles of the time: "Illiberal activism may be a healthy moral exercise for the young as they resist coming to terms prematurely with a flagrantly imperfect society, but it is an inadequate stance for political adults in a world that badly needs the committed contributions of people who can bring *both* fervently held principles *and* pragmatic cogency and skill to bear on a host of complex and difficult but immensely challenging problems." This essay, written for a symposium which never came about, by itself makes the book a success. A paragraph is worth quoting:

The line of thinking that I have been pursuing finds no special magic in the concept of "legitimation of evil," no hitherto unseen alternative to the old prescriptions for ethical advocacy: work to make the conceptions of man that prevail increasingly universal, less parochial; work to disseminate increasingly humane standards; and work to bring social practice more into line with the best standards of which we are aware. Nothing psychological about this! In the sphere of social life, where we must reckon with the fact that people will not fully agree with one another about these matters, the methods to which we must turn are those of politics.

Meanwhile, there are excellent essays on "the self and cognitive consistency" and "competence and socialization." These are among the few essays in this book which probe in depth into a substantive topic. There can be little if any classroom use for the book, and few professionals will find it of "professional" value. But they will enjoy reading it, as I did, with no utilitarian end in mind.

*The Wish To Be Free: Society, Psyche, and Value Change*, by FRED WEINSTEIN and GERALD M. PLATT. Berkeley, Calif.: University of California Press, 1969. 319 pp. \$8.50.

DOROTHY DOHEN  
Fordham University

This is a difficult book to read and a maddeningly difficult book to review. Even the blurb on the jacket was no help: it made sense only after a careful perusal of the book. The difficulty may be explained in part by the fact that the Weinstein-Platt approach to social change is interdisciplinary. Though the possibility of being equally skillful in several disciplines may be great, the probability of being able to write lucidly about them may be slight. The reader may be left bewildered as certain terms are carefully defined, while others are used with no effort to make their meaning clear. Thus, "value" is defined; "cathexis" is left for the reader to struggle with. If an interdisciplinary study is the product of joint authorship, the problems of lucid presentation are compounded: the style varies from chapter to chapter; and, since there is no reason to assume that an individual historian and an individual sociologist have equal writing facility, it is not surprising that the chapters analyzing historical cases of social change are beguilingly clear while the theoretical chapter entitled "The Sociology of Value Change" is unnecessarily murky.

The author's intent is clearly stated: "Theories that have been consistently relied upon to explain social change have lacked a systematically integrated psychology of the unconscious—

just as Freud and classical psychoanalytic theory lacked a proper appreciation of the importance of social structure. What we have done is to take aspects of both approaches—mediated through Parsonian sociology—in order to arrive at a more comprehensive historical and comparative view of the social process" (p. 18). Weinstein and Platt do not promise that they will come up with a coherent theory of value change integrating the psychological and sociological contributions, so perhaps it is unfair to fault them that they have not produced a systematic work. That they have produced an insightful work is certain; and it is well to focus on its strength.

Important insights are provided into the relationship of the unconscious operating in the persons who generate ideas productive of change with the social structure in which change is produced. In considering Max Weber's work, Weinstein and Platt note that Weber failed to understand the workings of the unconscious in the personalities of the agents of change. Thus, while Weber analyzed the import of Protestant ascetic doctrines in shaping the personalities of the early modern capitalists, he was unaware of the unconscious processes at work in them, as well as in the men of the French Enlightenment who displayed the same desire for autonomy and inclusion and who—where change was successful in the direction of greater freedom—developed the same personality traits related to self-control and active mastery, without benefit of the Protestant Ethic. (Unfortunately, the authors do not develop their analysis of Weber to account for certain unresolved questions in his theory of religion as related to social change: for example, why does the prophet arise, and can his charisma be explained in other than terms of a "mysterious gift"?)

The authors assume that in the unconscious there are both wishes for dependency and contrary wishes for independence and autonomy; they argue that this ambivalence remains tolerable and that the fantasies concerning freedom are repressed as long as the society provides subordinate persons sufficient rewards so that authority is not felt as frustrating and coercive. However, if through social structural changes the rewards fail, repressions lapse and the fantasy of freedom surfaces and aggression against authority may follow. This theory is tested by an analysis of the French Revolution and the "Introspective Revolution" or father-son conflict in the middle-class family at the time of the development of classic psychoanalytic theory.

Weinstein and Platt are careful to point out that the progression from fantasy to social action is not an inevitable evolution: "But once

fantasy becomes conscious as a function of social structural factors, the conditions exist for efforts to be made toward its ideological codification, and toward its general acceptance" (p. 23). Thus, the son could struggle for freedom from and equality with the father because changes in the economy and social structure wrought by the Industrial Revolution had virtually freed him from his dependence on the father and thus permitted him to face the oedipal conflict. Since these changes had made the mother more than ever the nurturing figure, the source of the child's affective life, the authors note that even Freud dared not examine the mother-son conflict nor admit the preoedipal situation to consciousness. (One wonders if Weinstein and Platt wrote this before Roth got to work and showed us how times have changed. At any rate, now they should produce a sociology of *Portnoy's Complaint*). The stride toward freedom may be arrested if the guilt and anxiety provoked by the wish to be free become intolerable and the actors are forced to accept regressive solutions. (Robespierre is viewed as a revolutionist committed to values of autonomy and inclusion, whose personality could not permit himself or others to be free, and who consequently returned to an authoritarian regime.) The solutions of the radical left and of the radical right are analyzed as unsatisfactory; only democratic moderates come off well as implementors of the wish for freedom. This conclusion may simply betray the authors' liberal bias; or it may be defensible in light of the historical analysis and theoretical development. At the least it is a conclusion that deserves careful consideration in view of the wish widespread at present for radical social change.

*The Illusions of Progress*, by GEORGES SOREL.  
Translated by JOHN and CHARLOTTE STANLEY. Berkeley, Calif.: University of California Press, 1969. 222 pp. \$7.50.

HANS H. GERTH  
*University of Wisconsin*

The translation and careful edition of this important book represents a work of love and great care. The introductions by Robert A. Nisbet and John Stanley are appreciative, scholarly, and sensitive. They aim at defending Sorel as a great, austere "moralist" against his detractors. Naturally, Robespierre, the "incorruptible," comes to mind. This moralist rigorism—so long after Freud—would seem readily transparent as the repressive character-armoring of a patrist peasant society. Fictional Ma-

dame Bovary represents one of the symbolic victims.

The fundamental effect running through Sorel's book is melancholia. Disillusionment was Sorel's central experience. He was disillusioned with the royalist right, with "Marxist" socialism, and with bourgeois liberalism. He cites at length and with appreciation the "farewell letters" of Condorcet and of Proudhon—letters of death-bound agony. Sorel sets out to debunk the concept of "progress" as a false and deceptive ideology of the middle class and its intellectual vanguards. His works represent a veritable catalogue of the arguments which traditionalist writers such as De Maistre and De Bonald and conservatives such as Edmund Burke and his German translator, Gentz, the Catholic convert and Protestant pastor's son, directed against 18th century rationalism and the French revolution. They denied the possibility of rational planning, of rationally enacting a constitutional law, a "law of the land" for a self-determining democratic society.

Sorel identifies democracy with "mediocrity." He sees this forever juxtaposed with "*grandeur*." Mediocre times follow times of *grandeur*. Popularization, primary school education, and vocational training institutions represent so many mechanisms for degrading excellence. Campaign rhetoric of democratic parties is clap-trap and swindle; the present represents the parasitical expenditure of past legacies of *grandeur*, always a levelling down process into mediocrity, a phase of degeneracy in law and in economic life, in the arts, in religion, and in philosophy. Sorel as a "culture critic" will touch on every and anything to "prove" his point. He admires ancient Rome: alas, the Roman Empire is presented without field slavery and expanding latifundias, without man-made desert lands in Sicily, Italy, the Balkans, and other Mediterranean countries. Deforestation and mining of the soil and their results did not enter the bookkeeping of the living. Neither the metric system nor the rational arts of mass organization, logistics, strategy and tactics, or administration would come to his mind when reflecting on the Napoleonic age. French rationalism of the 18th century appears as illusionist and sentimental. The probability calculus of Condorcet seems to him a climactic "scandal" of the age. He insists on the contingent singularity, the "genius," the personality cult. Problems of the political arena, party struggles and pressure group action, the on-going bargaining activities of market-oriented power in workaday life, and the diplomatic coalition strategy of unwitting statesmen preparing for World War I fall outside the observation of the prophet of the myth of the

general strike. He criticizes Karl Marx and borrows his types of capitalism: usury, commercial, industrial. He speaks of Marx' glory and despises "Marxism." He admires Lenin and Mussolini and Fritz Ebert at the end of World War I. These reflections on politics are totally devoid of their respective context and possibly serve as evocative images of *grandeur*.

With Bergson's philosophy and *élan vital* and William James' reflections on world religions, Sorel aims at the "transvaluation of values." The alternative of a silent tarrying "for the days of awakening" (p. 186) and the enthusiastic anarchosyndicalist upthrust motivated by the myth of the "general strike" remain after the debunking of the illusionist construction of a panoramatic sequence of scenic genres of "progress." Sorel affirms the small holding of the peasant and vintner, the small and possibly motorized workshop. Meanwhile "factories in the field" and industrial corporations have come to take their place. The subjection of warfare and of agriculture to industry have taken their course. Sorel represents a cry of petty-bourgeois agony. Said Mussolini: "We have fashioned our myth. A myth is a faith, a passion. It is not necessary that it represents a reality. It is real by being an incentive, a belief. It means courage" (cited by Georg Lukács, "Die Zerstörung der Vernunft," *Werke*, 1952, Vol. 9, p. 34). In an age of chronic warfare and revolutionary upthrusts, the voice of this critic of reason and rationality offers the agonized shriek of the desperado.

*Balance in Small Groups*, by HOWARD F. TAYLOR. New York: Van Nostrand Reinhold Co., 1970. 321 pp. \$8.50.

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Several varieties of systems theory have appeared in the behavioral sciences in recent years. Balance theory is one of these new formulations, and its particular variation is the application of the principles of homeostasis to the dynamics of human behavior.

Balance theory itself, however, has a number of varieties: Newcomb's theory of "systems of orientation," Heider's model of "balance," Festinger's "dissonance" principle, and the Osgood-Tannenbaum formulation of "attitude congruity," all of which have been categorized more broadly as "theories of cognitive balance" or as "theories of cognitive consistency." The common assumption of these dif-

ferent formulations is that human beings are naturally motivated to achieve and to maintain a state of psychological and sociological equilibrium in their everyday, social interactions. The basic concern of each of these varieties of balance theory is the processes by which this equilibrium is achieved—how definitions of social stimuli, particularly conflicting ones, are organized in themselves and in relation to other pre-existing cognitive material.

Taylor's task is to apply the principles of balance theory—or, more particularly, its several variations, to those human relations which are confined to the small group situation. His emphasis, however, is on "balance" rather than on the more commonplace concern of "dissonance." The first three chapters offer a discussion of the basic propositions of balance theory in its various formulations, and Chapter 3 contains an introductory discussion of recent heuristic (graphic-mathematical) formulations of balance theory. Chapters 4 through 7 review and evaluate the research evidence bearing on the theories of balance, with particular attention given to negative findings. In the concluding chapter, "Current Issues and New Directions," Taylor discusses the currently unresolved issues and paradoxes of balance theory (chief among which is that there appear to be a number of situations in which imbalance is preferred and even actively sought), attempts to integrate the theory with other orientations to the study of small groups, and proposes several hypotheses and problems for further research.

The elaborations of balance theory have come mainly from psychology since sociologists, even those involved principally in the study of small groups, have been little concerned with the several types of balance theory. This is due undoubtedly, as Taylor states, to the different orientations which the parental disciplines have taken in the more general area of social psychology. Sociological interests have been oriented in terms of interpersonal and structural properties of small groups, while the psychological approaches have focused more on the intrapersonal perspectives of the members of groups and have shown particular concern for perceptual questions of "cognitive consistency." Yet, as always, one might like to share Taylor's intention that perhaps balance theory could provide a vehicle for a viable rapprochement between sociology and psychology. His discussion is the kind that attempts to integrate the two different areas of interest as well as to synthesize the several psychological varieties of balance theory. For example, he suggests that balance theory could be seen as a specific theory of tension management which has been



a focal problem for the more sociological theories of social action applied principally to complex organizations.

Nonetheless, apart from the question of orientation, balance theory has been limited to the study of small groups and a few broader areas such as role theory, cognition, and attitude formation-change. The key issue, then, is its utility and comprehensiveness. What are the scope and limits of balance theory? While it may be true and desirable that greater sociological relevance can be found in the macro-orientations of systems theory, there are presently a number of methodological problems of measuring the utility and applications of balance theory to more complex sociological and psychological situations. Taylor's presentation is confined by and large to the analysis of the triad. Whatever applications are, or can be, made, appear limited to only three or four variables. The haunting question remaining is how one can expect to get sufficient control over the infinitely complex number of elements involved in homeostasis that would need to be assessed for any tenable verification of balance theory. It may be that some such control can be achieved by means of computer simulation and analysis, but Taylor makes no mention of any promising attempts along these lines.

The viability of balance theory then, particularly insofar as broader applications are concerned, hinges on this problem of verification in more sociologically complex contexts. Yet, as Taylor contends, while empirical studies are increasing, the conditions of non-applicability likewise are increasing, all of which indicates a further restriction of balance theory not simply to small-group situations but only specific types of such situations. Research shows that many of the fundamental predictions of balance theory are qualified substantially by sociological considerations, and that certain socio-structural variables in particular have profound effects on the balance process (see Chapter 5, "Conditional Propositions"). Such qualifications impose a number of limitations on balance theory.

What then is the place of balance theory among other theories in the behavioral sciences, and what particular value does it have for sociology? Much of the appeal of balance theory comes from its lending itself well to quantification and to the development of mathematical constructions and graphic formulations. But, much like various geometrics, one must ask whether or not its neat system of symbolic logic has any relationship to the practical order of social reality. Is balance theory a theory in the proper sense of that term, a set of deductions from other theories, a conceptual orienta-

tion, a heuristic device, or a methodological tool of another type?

Taylor considers balance theory to be a "middle range" theory, one which has limited application but yet one which can be used both deductively and inductively with other "broader" theories. Even so, Taylor is not too optimistic about the impact of balance theory on sociology and psychology. His prediction is shared somewhat by this reviewer, yet I suggest that the reader make his own judgment on balance theory for the following reason: Whatever Taylor himself thinks of the future of balance theory, one cannot fail to be impressed that homeostasis has become somewhat of a well internalized value and psychodynamic for him. His treatment of balance theory is in itself one of the most balanced presentations of any theory and, as such, one which unfortunately is all too rarely found in the behavioral sciences.

Taylor's style is somewhat repetitious and frequently tedious. Much of this, however, is necessitated by the nature of his subject matter, particularly in its mathematical and graphic dimensions. Yet, this usually undesirable element only enhances the clarity of a number of terms, concepts, and procedures, all of which are illustrated by graphs and examples. This volume will be of considerable value to all those involved in the broad field of social psychological theory; and for those not familiar with balance theory and its several variations, here is a volume that will provide a balanced introduction.

*Strategic Interaction*, by ERVING GOFFMAN.  
Philadelphia, Pa.: University of Pennsylvania Press, 1969. 145 pp. Paperbound. \$3.45.

CARL J. COUCH  
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This book attempts to examine the "taken-for-granted" dimensions of human conduct underlying strategic interaction, i.e., Machiavellian behavior. A vocabulary is introduced and used for descriptive purposes. It is claimed that the role-taking process, à la Mead, is basic to strategic interaction. However, how the role-taking process underlies this form of interaction is never developed. Rather, it is asserted now and then that role-taking is basic to calculative behavior. A thorough examination of processes underlying behavior organized to mislead others would be a major contribution. While Goffman neatly traces some of the convolutions of strategic interaction and even asks how far the game can go before society disintegrates, he fails to

introduce a set of concepts or propositions very much advancing the framework for the examination of this form of interaction over that offered by the game theorists.

He does note that the fake in the boxing ring, the bluff in the poker game, the deceptive purchase on the stock market, and spying are all predicated on the taken-for-granted condition that others base their action on what they think we plan to do. He unduly restricts himself to the esoteric world of international spies. Consequently, he inhibits himself and the reader from recognizing the generality inherent in the behavior he describes.

His one advance over game theory is the introduction of the concept of *expression management*. Contestants in strategic interaction not only calculate but also attempt to manage expressions, e.g., they try to control their emotions. While he deals with men as if they were robots, at least they are robots with emotions. He neglects to note that players in the game of strategic interaction can and do become involved with each other; at least they do in spy novels.

The only proposition offered in italics is "the best evidence for him (the observer) is also the best evidence for the subject to tamper with." Or freely translated, the best bluff a player can make is one the opposition knows he would never dare to make. Most two-bit poker players are aware of that principle. Many of his other observations do not border on the trivial; they are trivial. It is easier to hide bills than rolls of coins, the higher the stakes the more difficult it is to control emotion, and it is more difficult to trail someone at night than during daylight.

He raises some fundamental questions: e.g., he asks why we take others at their word. Goffman's suggested answer is that we do so for our mutual benefit. Such an answer assumes that man is primarily a rational, calculating animal, not a being who has his existence embedded in relationships with others. Goffman implicitly recognizes this point when he states that strategic interaction is possible only when there are special interests within society which can be advanced on the basis of someone else's loss.

His dismissal of the role of communication in strategic interaction is premature. The fact that the concept is much abused does not justify the strategy of ignoring communicative processes. Significantly it is a stance that Goffman does not maintain; for, in his own descriptions of strategic interaction, it is apparent that communicating to or "informing" the other is something that routinely occurs when persons attempt to get the better of each other.

Despite the above comments, anyone interested in negotiations, conflict, and deception would do well to read this book. At least the author is looking at joined behavior between interactors. Further, he sees the actions of persons in contests as produced by prior actions of self and others and the anticipated actions of others. Goffman directs his and our attention to social behavior, something far too few sociologists are doing.

*Classic Essays on the Culture of Cities*, edited by RICHARD SENNETT. New York: Appleton-Century-Crofts, 1969. 233 pp. Paperbound. \$2.95.

CHRISTOPHER SMITH  
University of Hartford

This collection of well-known essays by Weber, Simmel, Spengler, Park, Wirth and Redfield—"the greatest essays on city culture yet written"—are arranged under two headings. "The German School" includes "The Nature of the City" by Weber, "The Metropolis and Mental Life" by Simmel, and "The Soul of the City" by Spengler. "The Chicago School" includes "The City: Suggestions for the Investigation of Human Behavior in the Urban Environment" by Park, "Urbanism as a Way of Life" by Wirth, "The Folk Society" by Redfield, and a few shorter essays by the same men.

As recognized "classics," most of the essays have appeared and reappeared in one form or another since their original publication and continue to appear in current anthologies. They have been discussed, analyzed, and summarized many times and will no doubt serve as subjects for discussion in the future. The service provided by this editor is in presenting the essays in a single volume which is a convenience insuring a larger readership in the future.

In appraising urban studies today, the editor, in an introduction to the essays, states that "the work of the professionals in urban studies has become increasingly removed, and in fact irrelevant, to the new search for values. The field of urban studies is now plagued by a kind of superstitious belief in scientific purity of research, as though, like Faust, if we had perfect and pure knowledge the world around us would suddenly change. As a human discipline, urban studies shows all the signs of rapidly dying out." One can quarrel with this assertion on the part of the editor but still agree with his concluding statement that there is "nothing out of date about these essays."

*The Quality of the Urban Environment: Essays on "New Resources" in an Urban Age*, edited by HARVEY S. PERLOFF. Washington, D.C.: Resources for the Future (Distributed by The Johns Hopkins Press, Baltimore, Md.), 1969. 332 pp. Paperbound. \$6.50.

IRVING LEWIS ALLEN  
*University of Connecticut*

This volume represents an appropriate merging of the concern with the "urban crisis" of the 60's and the apparent new concern in the 70's with the "environmental crisis." It consists of nine papers prepared for a symposium sponsored by Resources for the Future, Inc. The first paper, by the editor, develops a decision-making framework for policy makers to conserve the "new resources" in the urban environment. The subsequent eight papers, by regional and urban economists, urban geographers, and urban planners, are more or less addressed to this theme. The volume will interest anyone concerned with the quality of urban life, and several of the papers are pertinent for sociologists *qua* sociologists.

Basic to Perloff's decision-making matrix is a model of the urban environment as a partially closed ecological system or subsystem in a national system of cities. The decision-making framework is an attempt to specify the complex interrelationship between man-made and natural elements in the system, specifically among the "new resources" of air, water, land, open space, and the amenities. Perloff notes that a set of conservation norms emerged that, however much violated in practice, value investment in and development of resources, the development of multiple uses of single resources, their conservation for future generations, and respect for ecological principles. But he argues that there is a lag in conventional policy-thinking concerning the application of these norms to the new resources in an urban age.

Three of the subsequent papers in the volume are sociological in their approach to specific resource problems. Arthur Atkisson and Ira Robinson develop a functionalist or behaviorist model to define amenity resources in terms of attitudes and behavior of humans in "interaction" with the urban environment. Brian Berry and Elaine Neils describe the national system of cities and show how environmental factors influence location, size, and other factors in the ecology of single cities. Stuart Chapin, Jr. and Thomas Logan apply an urban-behavior hypothesis—daily household activity patterns—for its implications in the use of the new resources. Other articles of less sociological interest, but of no less importance for policy-making, con-

cern environmental pollution, urban space resource, and the related problem of transportation.

Perloff and the other contributors have outlined principles of policy-relevant theory of the urban environment that deserve extensive research and development. The volume has limited use as a text in sociology courses, but is a useful addition to the professional bookshelf of the urban sociologist.

*Long-Range Planning for Urban Research and Development: Technological Considerations*, by the COMMITTEE ON URBAN TECHNOLOGY. Washington, D.C.: National Academy of Sciences, 1969. 94 pp. Paperbound. \$3.50.

*A Strategic Approach to Urban Research and Development: Social and Behavioral Science Considerations*, by the COMMITTEE ON SOCIAL AND BEHAVIORAL URBAN RESEARCH. Washington, D.C.: National Academy of Sciences, 1969. 100 pp. Paperbound. \$3.50.

ALBERT N. COUSINS and HANS NAGPAUL  
*Cleveland State University*

Engaged by HUD in 1967 to advise on "strategies, capabilities, and technical considerations for long-range planning of urban research and development," these two committees of the National Research Council may have done the nation a great service. If so, they will also have contributed to the further development of sociology as a policy science. At any rate, they have rejected the simplistic philosophy of dealing with the urban ills of congestion, decay, slums, crime, and the foul physical environment principally by means of bricks and mortar. Instead, these panels of scientists and administrators have urged upon HUD that its R&D effort have "a systematic concern with people, their condition, institutions, and social change." They conclude that even transportation, public utilities, and other facilities all presuppose a sufficient knowledge of social and behavioral requirements. Only after the social benefits are specified explicitly can technology be selected or applied.

The conferees are emphatic on the point that the social and behavioral sciences are not tools providing ready-made solutions to urban problems. There is no airy optimism. Embedded in their compact prose are any number of sobering insights: e.g., they note the shortsightedness of transposing the systems approach—even with computer-aided simulation—from the mechanistic aerospace industry to the city with "the dis-

orderly and extremely ramified character of collective life"; that the academic disciplines are not structured around the solution of social problems; that urban information setups tend to be geared to the "specifiable needs of a limited group of decision-makers"; and that the necessary resources are not immediately available for the magnitude of the effort required to reclaim America's cities.

The focus of their many recommendations is probably that HUD keep R&D capabilities, manpower, and funding all in proper perspective. To accomplish an orderly but progressive expansion of resources, the committees recommend a network of university, nonprofit, industrial, local government, and inhouse agencies working in concerted fashion. Organizational forms include urban institutes, municipal development centers, and community information systems. Though visionary in outlook, the committees touch upon but do not come to terms with the factors of social life that militate against dispassionate inquiry, such as mutually exclusive interests, "passionate debate," and "the formal, aggregative mechanisms of the political process." The Committee on Urban Technology informs us that the acceleration of the urban programs that may be called for is contingent upon the termination of U.S. participation in the Vietnam war.

Raymond A. Bauer chaired the social and behavioral committee, James F. Young of General Electric that on technological considerations.

*Change and Renewal in an Urban Community: Five Case Studies of Detroit*, by ELEANOR PAPERNO WOLF and CHARLES N. LEBEAUX. New York: Praeger Publishers, 1969. 574 pp. \$19.50.

R. J. DUNNE  
Colorado College

This book includes five studies of urban problems carried out in Detroit during the early 1960's. The material is divided into six sections: (1) a study of a white middle class residential area which is experiencing the early stages of invasion by Negroes and of the efforts made to retain white residents; (2) a study of a middle-class "renewed" residential area which appeared to be racially stable, i.e., no noticeable tendency toward exclusion of either whites or Negroes; (3) a study of a lower class, overwhelmingly Negro, area slated for "renewal"; (4) a study of individuals who had been relocated following "renewal"; (5) a study of

small businesses that had experienced removal from an original area that had been "renewed"; (6) the concluding section, best described as commentary on urban problems.

The substantive questions addressed and the data gathered are pertinent. The writers apparently resisted the urge, now a fad in sociology, to take up half their book with the pretentious display of research technique. There are, however, some problems—such as the lack of a conceptual scheme which might have served better to relate the various sections. The effort to provide such a scheme in the concluding section is inadequate and too late. Also, the book is cluttered with marginally relevant data. Though the professional reader can be expected both to provide his own conceptual scheme and to relish the excess of data, this reviewer has doubts about the book's value as a text. A shortened and popularized version of the same material would serve this purpose.

This reviewer was very much impressed with the "acceptability" of the interpretations found in *Change and Renewal*. For example, the writers question the justifiability of saving the whiteness of middle-class residential areas. This questioning, however, does not stop them from offering suggestions as to how it might be done. Further, they are at pains to point out that such a desire does not reflect race prejudice (bad), but rather social class prejudice (good). Though it is obvious why middle-class home owners would dislike an increase in the rowdy population of their children's schools, it also seems obvious that such behavior is not good for those rowdy, lower class negroes either. However, such an evaluation was expressed by neither the writers nor the middle-class residents (of either color). This reviewer was continually amazed by the narrow focus displayed in what seemed like an effort to compartmentalize ideas which, when compared with each other, seemed absurd, e.g., that we will be concerned only with "our" residential area, or the schools "our" children attend, or with racial integration "within" the middle class. It would seem that a more wholistic perspective might suggest that "inundation by Negroes," or "flight to the suburbs," or "anxiety over future home values and conditions of schools" will forever remain as felt threats as long as existing social forces continue to generate undesirable behavior.

In their concluding section the writers speak of urban renewal as a promise which has been distorted. Yet, they disclaim any interest in placing blame (but might not such an effort assist in locating causes?). They then make a statement commonly encountered in sociology today: "Without attempting an analysis of the political

forces. . . ." Thus, they are acceptable and step on no one's toes. However, despite this lack of a political analysis, they present a political solution to the problem: coalition politics (and they warn lower-class Negroes against making demands which might alienate the white working class or the middle class of either color).

Nonetheless, *Change and Renewal* is full of meaningful data and could be most useful to those who will sort out the status quo propaganda.

*Readings in Community Organization Practice*,  
by RALPH M. KRAMER and HARRY SPECHT.  
Englewood Cliffs, N. J.: Prentice-Hall, 1969.  
458 pp. Paperbound. \$5.95.

GURMEET S. SEKHON  
*Indiana State University*

This useful book reflects the perpetual concern of social scientists working in the area of planned social change, focusing primarily on the practice of community organization in the United States. The editors have organized the selections in two main sections: the contextual and intellectual bases of community organization, and the processes of directed change.

Kramer and Specht consider the practice of community organization to consist of techniques of intervention by which individuals or collectivities organize and engage in planned collective actions to achieve defined goals within a democratic system of values. They seem to be rather critical of the meager contribution of basic social science research to the theory of community organization, and hold that so far as the processual aspects of community organization are concerned, there exists an adequate body of knowledge from which workers can profit both in terms of theory and practical application. From the above left-handed compliment, one would expect some statement about specific inadequacies of the basic research or suggested directions for collaborative effort. Instead, the editors attempt theory formulation through delineation of a typology of community organization, a typology constituted by the following elements stemming directly from the practice of community organization: character of "action" system(s), territoriality, nature of problems handled, character of issues involved, kind of "target" system, nature of organizational structures, and the role of the professional worker. Surprisingly, only two typological

models are generated for consideration, viz., community development and social planning.

The readings focus on a variety of action systems and strategies that are available to the practitioners of planned change. The material in these well-chosen selections highlights problem solving at macro and micro levels, roles and dilemmas faced by the professional practitioners, problems of intra- and inter-group tensions, and the practical problems and processes of social planning. These selections provide a very handy compendium of well-integrated material for use by both practitioners and their trainers.

The value of this important and timely volume is slightly reduced by two minor shortcomings. Although the selections more than adequately cover the historical background of community organization and processual aspects of community organization practice, the nature of the change proposal as a crucial determinant of strategies and their success has not received sufficient attention. In addition, the numerous insights could use the support of additional empirical selections.

*American City Planning Since 1890*, by MEL SCORR. Berkeley, Calif.: University of California Press, 1969. 745 pp. \$17.50.

ROBERT HIRZEL  
*University of Maryland*

This text was commissioned by the American Institute of Planners as a part of its 50th anniversary program, and produced under the auspices of the AIP and the Institute for Governmental Studies and the Department of City and Regional Planning of the University of California, Berkeley. The author is a research associate for the Institute of Governmental Studies.

A misleading feature of the text is the disparity of the title on the dust jacket and on the title page. The former does not carry the qualifier, "Since 1890." The hasty reader may quarrel with the author for failing to consider the planned cities of the colonial period, e.g. Savannah, and the efforts of the early 1880's, e.g. milltowns, utopias, and state capitals. Indeed, a weakness of the text is its failure—beyond marginal mentions of Washington, D.C.—to acknowledge in any fashion earlier efforts at city planning. Even a text celebrating the 50th anniversary of the Institute should spend some time with its progenitors, albeit they were much less "professional" planners than they were

speculators, town builders, city fathers, religious zealots, and "men of cultivation." Still, they left a rich tradition whose influence stretches from the gridiron pattern of unrelenting residential blocks to the pleasure parks of the early 1800's.

Nevertheless, the author has placed a tremendous wealth of material in the reader's hands, materials ranging from newspaper editorials to committee minutes and governmental reports. Those materials are ordered in a sequential fashion with branching treatment of a major theme of planning in principal locales. The weight of the material is relieved and enhanced by a superior selection of photographs of notable persons, buildings, and vistas, and plans and drawings of towns and residential and business districts of cities. Non-planners will be overwhelmed by the volume of material, and they are well advised to begin with a briefer text such as Tunnard and Reed's *American Skyline* which will give them a skeleton to be fleshed-out by Scott's text.

Of particular value for the general reader is the great attention given to the development of planning since the First World War, with 470 of the text's 652 pages being used for this purpose. Great attention is given to the emergence of regional planning, planned communities, re-housing, the greenbelts, TVA government pump-priming—e.g. FHA—World War II industrial expansion, urban redevelopment, the housing acts, and the suburbanization of industry, residence, and business. The text ends with a brief introduction to the Model Cities Program.

Scott's form of presentation is that of secretarial reporting. We are told of the events and decisions, the people involved, and sometimes even their conversations. There is never a critique of ventures nor an evaluation of consequences, and Scott's personal reactions are not revealed. Thus, the book is an encyclopedic resource—as the author intended—rather than an evaluation of the product of a discipline.

In general, the author's presentation of the social, economic, cultural, and technological detail underlying the phases of planning prior to the Great Depression takes the form of broad platitudinous generalizations. Once Scott comes to his own time, he succeeds in making intelligible the body and substance of the voluminous detail that characterizes urban planning. There is little reference to the large body of sociological community research. The treatment of local social organization is limited to a short reference to Jane Jacobs' criticism of urban planning. Thus, for the sociologist the text is limited to a source book on the history of urban planning in the United States.

*Urbanization in Nigeria*, by AKIN L. MABOGUNJE. New York: Africana Publishing Corp., 1969. 353 pp. \$8.50.

*Yoruba Towns and Cities: An Enquiry into the Nature of Urban Social Phenomena*, by EVA KRAPP-ASKARI. New York: Oxford University Press, 1969. 195 pp. \$5.95.

WYATT MACGARFEY  
Haverford College

Anthropologists discussing urbanism in Africa usually find they do not know what they are talking about. As Eva Krapf-Askari points out, to the urbanists Yoruba cities have always seemed anomalous or "not urban in the usual sense of the term." The "usual sense" refers, however, not to an analytical concept but to a "myth in terms of which Western man has seen his own social life" (p. 22). In the myth, industrialization creates the city and thus sacrifices the community; Wirth, Kroeber, Redfield, and many more locate urbanism as a way of life somewhere between Marx's "alienation" and Durkheim's "anomy." Yoruba cities, however, existed before colonialism and industrialization and were not characterized by secularization, depersonalization, superficiality, and the rest of the gloomy catalogue.

Defining cities as large, dense, permanent nucleated settlements, Krapf-Askari describes the way of life in traditional Yoruba cities for which the people themselves use the term *ùú* and which centers on the kingship; the *oba*, his palace, the market, descent groups, residential wards and compounds, shrines, and the city walls are all part of the complex. Kinship, descent, and residence are discussed in a separate chapter, followed by one on interest groups and associations. Most of the material is taken from published sources, but the author's time in the field has helped her to rewrite the material in a lively and convincing style. It is only the penultimate chapter on social stratification that reveals that she has herself been entrapped by the myth and in consequence is unscientifically eager to show us that life in Yoruba cities is characterized by the ideal rural virtues. The presence of descent groups is implicitly taken as evidence of some kind of unspecified and untested integratedness; "solidarity" and "vitality" are among the author's favored words in this context, and throughout a perceptive and well-organized account of the emergence of a managing class she repeatedly raises and as often discounts the possibility that social cleavages have developed or might develop. In the end (p. 160), to describe the increasing diversifica-

tion of modern urban society she finds it "tempting to borrow Durkheimian terminology once again and call it 'organic.'"

As a geographer Akin Mabogunje is little interested in the quality of the urban way of life; his description of social structure is much thinner than Krapf-Askari's, being almost entirely restricted to quantitative data. He is equally dissatisfied, however, with the obscurities and prejudices in the prevailing concept of urbanization. Having identified cities in terms of their density and concentration, he applies Christaller's central place theory and its derivatives and asks what functions cities fulfill in the spatial economy. Pre-colonial Hausa and Yoruba cities are seen as serving a system of long-distance trade and performing administrative functions for large surrounding areas. What happens to such a system when "a new, faster, more effective, more capacious transportation system is imposed [and] the new spatial economic integration which is achieved [thereby] focuses the flow of commodities within the system on a few selected points, with a view to facilitating the export of these commodities out of the country" (pp. 141-142)?

Asking this question, Mabogunje handles the historical perspective more sensitively than does Krapf-Askari. He concludes that the "problem" of the predominance in Yoruba towns of agricultural occupations ("agrotowns" has been offered as a label for such concentrations) is a false one; "nothing constrains communities irrevocably on a path of increasing specialization" (p. 42), and in fact, he asserts, it was only in the nineteenth century that townsmen were forced by civil disorder and the influx of cheap manufactured articles from Europe to abandon their specialized crafts and *revert* to agriculture (pp. 85, 120, 198). This reversion is one aspect of a complex, long-term process of "urban readjustment" to new economic and political conditions that Mabogunje seeks to evaluate quantitatively. Ideally a diachronic series of statistical surveys would be desirable, but by an ingenious factor-analysis of the 1952 census the author extracts confirmation of his basic hypothesis and simultaneously provides a relatively precise description of urbanization in Nigeria. The results are not entirely surprising; but they provide a valuable foundation for future research, especially when the political situation makes possible a reliable new census.

The second part of the book treats Ibadan and Lagos as case-histories of urban development, the latter being a relatively new center whose existence responds primarily to the new, imposed pattern of spatial integration. The author documents in detail the specific economic

and political functions of the modern city; he regards the presence of traditionalistic "sons of the soil" as an impediment to the rational development of these functions (pp. 202, 317).

Besides their individual contributions, both books offer valuable syntheses of scattered and often inaccessible materials with appropriate bibliographies.

*Social-Psychological Barriers to Housing Desegregation*, by BONNIE BULLOUGH. Special Report, No. 2. Los Angeles and Berkeley, Calif.: University of California Housing, Real Estate and Urban Land Studies Program and the Center for Real Estate and Urban Economics, 1969. 134 pp. \$4.50.

WILLIAM A. OSBORNE  
*St. John's University, Jamaica, N.Y.*

It is commonly presumed by social scientists as well as interested laymen that residential segregation patterns derive from fear or prejudice-inspired behavior by white people and the concurrent dynamics of social mobility and the housing and mortgage markets. More specialized students of the problem, however, were always aware that there was some element involved of preference or self-segregation by Blacks. Many Negroes would still prefer Harlem or Pacoima—no matter how easy it were to move into a white neighborhood. But precisely how or to what degree such preferences operate as a brake on the process of residential integration is a question that has received little or no attention. Suppose, for example, nationwide fair-housing laws were effectively re-written and enthusiastically enforced. Would the integration process surge headlong toward its "salt and pepper" goal?

Simply articulating the question suggests there are factors at work inside the Black ghettos about which we have no scientific knowledge. We don't know (to borrow the author's phrasing) "the special strength or unique characteristics" of those who break through the barriers into white neighborhoods. Nor conversely do we know much about the psychological make-up of those who remain.

This competent study zeros in on these questions as they apply to a Negro middle class population in the Los Angeles environs. Two samples of 104 and 120 persons from integrated neighborhoods and one of 106 persons from the solidly Negro Central Los Angeles provide a vast store of data. Bullough uses the beleaguered Srole anomia scale and a ten-item scale for powerlessness borrowed in part from Seeman.

As one might expect, the ghetto sample in this study scored higher than the integrated residents on both anomia and powerlessness—with all other variables controlled. This evidence is offered not as the basic cause of segregation but as an explanation of the weak response to the crumbling barriers to integrated housing. "The old housing patterns remain," asserts Bullough, "because of the alienation of the ghetto residents" (p. 62).

Bullough's study breaks new ground in its operationalization of the concept of alienation (anomia and powerlessness) as a barrier to housing desegregation. It reinforces the Coleman report in that it finds that past segregation experiences in ghetto homes and schools produce high scorers on anomia and powerlessness scales. There seem to be contradictory implications for the Moynihan report on the other hand, but they are not so clear. Only 39% of the mothers of ghetto respondents were employed while their children were growing up, while the corresponding figure for the mothers of the integrated sample was 58%. It seems to this reviewer that one of the deficiencies of this otherwise commendable study is that the author makes no comparisons with the Coleman data.

The interview schedule and questionnaire, a map of the areas sampled, and data on the universe are not included. Although footnotes are ample, there are no bibliography or index. Editor and publisher seem, in my view, to be responsible for these and other minor, but aggravating flaws in a study that makes a significant contribution to the accumulating knowledge about desegregation.

*Racial Policies and Practices of Real Estate Brokers*, by ROSE HELPER. Minneapolis, Minn.: University of Minnesota Press, 1969. 387 pp. \$9.50.

NORMAN J. JOHNSON  
Carnegie-Mellon University

This volume reports Helper's dissertation research conducted in three areas of Chicago in 1956—one characterized by recent black emigration, another a relatively established black settlement, and the third seen as a potential black target area. Using 121 interviews with white real estate brokers selling or managing residential property in one or more of the three areas, Helper attempts to delineate the ideology, policy, and practice of real estate brokers in

regard to the selling of property to blacks. On the basis of 40 preliminary interviews, she hypothesizes that the ideology and the practice of exclusion of Negroes from white residential areas are positively associated. She also seeks to determine why there are brokers who sometimes do sell property to blacks. The study is plainly designed to show how real estate brokers are socialized to develop patterned ways of excluding black people from certain residential areas.

While Helper attempts to demonstrate the existence of the exclusion ideology by focusing on beliefs about, for example, Negroes, whites' attitudes toward Negroes, Negroes' effects on property values, Negroes' effects on neighborhoods, and the possibility of residential integration, she does not quite pull it off. For example, in several places throughout the text she points out that few brokers have any personal aversion to dealing with Negroes. In fact, she points out that legitimate brokers do not urge whites to move when Negroes enter their neighborhood. The hypothesized pattern is found only in the neighborhood of recent impact. However, there is compelling evidence of the socializing impact that organizations such as The National Association of Real Estate Boards, the Chicago Real Estate Board, the lending agencies, and the real estate agencies themselves have on the uninitiated—the new broker, whether or not he is a member of the Chicago Real Estate Board.

Contrary to the author's thesis, the ideology that is effectively taught to all brokers is that the property owner's satisfaction and the protection of his property and neighborhood are the first responsibilities of the broker. Though the data do indicate that there is exclusion, it is the result of the ideology which gives supremacy to the attitudes and feelings of the property owner. Brokers have tended to believe that white property owners demanded the exclusion of blacks, and moving-out rates tend to support the brokers' conception. Unfortunately, Helper does not check the validity of this belief although it could have been done by focusing on the real estate man both as a broker and as a white property owner. Alternatively, and perhaps even better, a sample of whites might have been interviewed to determine the extent to which they wanted blacks excluded and a sample of blacks interviewed to determine the extent to which they wished to reside in white neighborhoods.

There is conceptual confusion, e.g., such concepts as *fair housing* and *open occupancy* are often used interchangeably. Nevertheless, the study does have practical value and does



suggest areas for further research. What results in a neighborhood where whites are trapped and can't leave while black people "pour" in? A confusing discussion suggesting a war in which the broker is a foot soldier and/or referee leads me to conclude that the confusion might have been avoided if the framework for analysis had been black-white relations with the broker viewed as middleman.

Despite its conceptual and methodological weaknesses, this is a compelling piece regarding the effect of social structure. Moreover, it provides keen insights for beginning social practitioners working in the area of housing. Regrettably, as is often the case, it comes much after the fact. What would have been its impact on social policy if this volume had been published at the beginning instead of the end of the decade?

*Institutional Racism in America*, edited by  
LOUIS L. KNOWLES and KENNETH PREWITT.  
Englewood Cliffs, N.J.: Prentice-Hall, 1969.  
180 pp. Clothbound, \$5.95. Paperbound (Spectrum), \$1.95.

FREDERICK L. WHITAM  
*Arizona State University*

It is not often that a sociology professor—long accustomed to assigning books to students—has the opportunity to read one written by students. This volume is primarily the work of a group of Stanford University undergraduates who in 1967-68 engaged in a "work-study" seminar sponsored by the University Christian Movement and the Mid-Peninsula Christian Ministry of East Palo Alto. The authors describe their work—primarily a collection of research papers—as a presentation of the "fruits of our 'intellectual revolt,' our struggle to define racism not as Negro Pathology, but as White and Institutional and all too American." The core of the book consists of systematic, well-documented, although mostly familiar, catalogings of examples of racism as imbedded in major American institutions. A chapter on racism in economic life, for example, describes the exclusion of the black man from free enterprise, the plight of the black worker, and the woes of the exploited black consumer. Other chapters deal with racism in our educational, legal, political, and health-care systems. The final chapter, for an undisclosed reason pub-

lished as an appendix, offers a rather repetitious discussion—again institution by institution—of racism in an urban setting.

The professional sociologist is not likely to find in this volume original insights or new information with respect to racism. What is perhaps more interesting—especially to the university teacher—is to read, sometimes between the lines, of the efforts of a group of morally committed "students at a prestigious, private, and white university" as they have tried to "evolve a strategy and set of tactics for social change in the white community." The results are not entirely successful, frequently reflecting frustration and even contradictions. For example, while declaring emphatically that "only something *unusual* will reverse the direction our nation is bent upon," the authors offer only the somewhat feeble and certainly traditional suggestion that a grass-roots, person-to-person, educational campaign should be organized to combat institutional racism. It is paradoxical that while the focus of this analysis is correctly structural-institutional, the means offered for achieving racial justice are individualistic, calling for changing attitudes through verbal persuasion.

The authors proclaim that they "refused to be programmed by the university"; rather they "used it as a giant consulting firm for our group goals," arguing that the "intellectual and technological powers of the university most often appear in community conflicts as allies of governing boards and large corporations." Yet, interestingly enough, their publication is itself academic, relying heavily upon the research and publications of academics, and indistinguishable from material presented for the past twenty years in social problems and race relations courses in traditional university classrooms. Furthermore, the means suggested for eliminating institutional racism would probably be regarded as conservative by most "old fashioned" liberal professors and downright reactionary by the young turks in the radical caucuses.

Despite shortcomings, the reader cannot fail to be impressed by the commitment, careful research, and more than competent writing style reflected here. The core chapters constitute an excellent summary of institutional racism and, with respect to detail, go beyond the analyses found in some widely used classroom texts. The teacher of social problems or race relations might provide his own students with a surprise by assigning them a supplementary text written by their fellow undergraduates.

*Race and Poverty: The Economics of Discrimination*, edited by JOHN F. KAIN. Englewood Cliffs, N. J.: Prentice-Hall, 1969. 186 pp. \$5.95.

*Poverty and Discrimination*, by LESTER C. THUROW. Washington, D.C.: The Brookings Institution, 1969. 214 pp. \$6.75.

JAN M. HOWARD

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San Francisco*

These are two approaches to a common set of problems. The first is a book of 22 readings concerned with the economic condition of the Negro, the labor market, the housing market, white and black attitudes, and policy alternatives. The second is a concise econometric analysis of poverty and discrimination in America with a policy guideline for eliminating both.

The book of readings is rich in data, debate, and suggested solutions to the twin problems at hand; but like many compilations it suffers from fragmentation, redundancy, and apparent inconsistencies. [Compare, for example, the optimism of the Bureau of Labor Statistics report on Social and Economic Conditions of Negroes in the United States (pp. 33-37) with the pessimism of Bachelder's discussion of the Decline in the Relative Income of Negro Men, (pp. 45-51).] Kain's lengthy introduction gives the gist of the various presentations to follow and provides a central focus for the volume as a whole. He examines historical and contemporary factors responsible for the income discrepancy between Negro and white Americans and concludes that the economic condition of the Negro lags far behind the white and that "the gap has closed very little" (p. 2).

Five of the readings are particularly valuable as research contributions. Alan Batchelder demonstrates that, except for high rates of Negro migration from the South, the earnings of Negro males would have declined relative to whites during the decade 1950-1960. Barbara Bergmann determines that the value of "human capital" (schooling and on-the-job training) embodied in the average Negro male adult is about \$10,000 less than for the white. Paul Siegel concludes that "two-fifths (38.5%) of the difference in average earnings of whites and nonwhites is what it costs to be black" (p. 67), independent of regional, educational, and occupational effects. Karl and Alma Taeuber analyze recent trends in racial and ethnic segregation to see if Negroes should be viewed as an immigrant population. They conclude that, for im-

migrants, residential segregation has decreased with increasing economic status. Negroes show the opposite pattern; segregation has increased despite economic advances. Chester Rapkin finds that Negroes get less housing for their money than whites and that price discrimination increases as rent increases.

Two other pieces are important to read because they speak from two sides of the integration-separation dilemma. In "Alternatives to the Gilded Ghetto," Kain and Persky advocate programs in housing, education, and employment aimed at weakening rather than strengthening the ghetto. They feel that ghetto gilding is morally wrong and costly in terms of economic inefficiency, and that it exacerbates the solution of many social problems. In "Desegregated Housing: Who Pays for the Reformers' Ideal?" Piven and Cloward advocate a policy of separatism. They contend that efforts to attain open housing have failed and "have imposed heavy costs on poor Negroes and whites" (p. 175). Proponents of desegregation should redirect their energies toward rehabilitating the slums and providing cheap, decent housing in the ghetto.

Thurow's book is aimed at readers with a background in econometric techniques. To understand and evaluate the argument, one should be able to interpret the complex formulas and plow through the weighty analyses. The average sociologist will be in unfamiliar territory. Yet, the author's purpose and conclusions ring clear. In his introductory chapter Thurow contends that "discrimination and poverty are as intertwined as Siamese twins," but "they need two policies, one to fight the causes of poverty and another to reduce discrimination" (p. 1). He then examines the extent of American poverty and discrimination over the years, 1929-1967, sets forth a detailed analysis of the causes of poverty, investigates the theory and practice of discrimination, and considers various means of intervention. Ten appendices delve more deeply into theoretical and methodological problems.

Thurow concludes that "general economic growth and foreseeable changes in the structure of the private economy will not eliminate either (poverty or discrimination)" (p. 154). Special programs must be designed to change the distribution of income, and these approaches must be "color conscious" because Negro poverty will not be cured by palliatives for white poverty. To eliminate poverty and discrimination, Thurow proposes a multi-faceted approach: creating tight labor markets, improving the distribution of human capital, increasing labor mobility, ending discrimination in all levels of government, and meeting the special needs

of persons outside the labor force. The only single-shot program the author recommends is one of direct income transfers, but he suggests this would be politically unacceptable.

A central argument of the book is that the monopoly powers of whites result in Negro losses and white gains of approximately fifteen billion dollars a year. This contradicts Gary Becker's theory (*The Economics of Discrimination*) that discrimination is economically costly to whites but that they are willing to pay a premium not to associate with blacks. According to Becker, discrimination can only be eliminated by changing white attitudes or bribing them enough to forsake their aloofness. According to Thurow's theory, Negroes and the various branches of government can attempt to break down the monopoly powers of whites without first having to change their prejudicial attitudes.

With the exception of a few comments in Kain's anthology, both books present a dismal picture of the past and present situation of the American Negro. And since there is no discussion of the means by which any of the proposed solutions might really be effected, the future also looks grim.

Thurow's book has the advantage of being integrated and consistent in the thrust of its argument. But in spite of his white monopoly theory, he expresses a certain naiveté about social power and social forces that weakens his presentation. He assumes that the elimination of poverty and discrimination are really important national goals and that all would be well if only our leaders would apply the formula he suggests. He fails to consider the simple and complex links between white power, white prejudice, black power, government action, and the persistence or amelioration of discrimination. In this respect, several pieces in Kain's volume are relevant: Jack Rothman's portrayal of "The Ghetto Makers," Paul Sheatsley's discussion of attitude change among whites, and the Kerner Commission's Recommendations for National Action. Like Thurow, the Kerner report recommends a national system of income supplementation; but this proposal, along with many others, has yet to be implemented. Small wonder that we are beginning to smell the smoke from "The Fire Next Time."

*Soulside: Inquiries into Ghetto Culture and Community*, by ULF HANNERZ. New York: Columbia University Press, 1969. 236 pp. \$5.95.

*White Ethics and Black Power: The Emergence of the West Side Organization*, by WILLIAM W. ELLIS. Chicago, Ill.: Aldine Publishing Co., 1969. 190 pp. \$5.95.

DONALD S. BRADLEY  
Emory University

Among the recent spate of books appearing on ghetto life, Ulf Hannerz's *Soulside* stands out in its readability, rich detail, and deep insight. Grounded in the tradition of the best of the urban ethnographies, this Swedish anthropologist gives us a detailed description of the "ghetto way of life" based upon two years of participant observation of a Washington, D.C. black neighborhood. Choosing as his central problem, "What is different about ghetto living?" and defining the ghetto as "a web of intertwining but different individual and group life styles" he purposively neglects most occurrences of mainstream culture and sets out on the ambitious and largely successful task of describing the black expression of community "soul."

The book paints in broad outline the visual impact, social ecology, and institutional framework of the Winston Street neighborhood and introduces us to many of the residents. It also serves to describe the ties that this local neighborhood has with the wider world—the white world in general but more specifically the ghetto establishments of black Washington represented by the churches, theatres, bars, and pool halls found on the main thoroughfares.

Unlike most past studies, this book recognizes and sets out to portray the diversity of life styles that exists in a lower class, black neighborhood and to account for the basis upon which people of different life styles interact with one another. Four "idea types" of life style are identified and described: mainstreamers, swingers, street families, and streetcorner men. These are presented as neither exhaustive nor mutually exclusive categories but rather as types of social interaction that result from the intersection of age and sex, peer group and family alignment, and career stage and economic relationships to the wider society.

Although the book stresses how the ghetto community is differentiated, it does not exclude those things ghetto dwellers have in common—things that create a shared perspective toward the ghetto conditions. This set of conventional understandings of ghetto life finds expression in the descriptions of black humor such as playing "the dozens," black music represented by gospel singing and blues, and religious faith. The shared perspective and the philosophy of "I walk my walk and talk my talk"—a ghetto phrase for minding one's own business—is a

partial answer to the question of how people with these diverse life styles manage their social relations with each other.

In addition to the preceding issues, the book deals with various facets of ghetto sex roles and, specifically, sex role socialization for males. Covering a period prior to, during, and after the racial turmoil of 1968, the book includes some insight into how ghetto dwellers define their discontent with their relationships to the outside world, how they react to the prospects of turmoil, and how they view the insurrection when it comes.

William Ellis's *White Ethics and Black Power* is a disappointment. The work of a black political scientist on a lower-class black community organization on the west side of Chicago, it tends to reveal more about the author than it does about the organization under study. Openly hostile to the social sciences and sympathetic to the West Side Organization (WSO), he presents such a caricature of both that the impact of his observations is lost.

Characterizing modern political science as preoccupied with "objective social realities" and "primarily concerned with describing a given situation in terms of how he (the political scientist) sees it," Ellis advocates the partisan investigator who, forswearing objectivity, acts as translator for the group under study. In his partisan report, however, we are given no means to judge how accurate a translator Ellis is. His objective would have been better served had he allowed the individuals of the WSO to speak for themselves. Suggestions for improving the social sciences and ideas for their role in solving societal problems are certainly in order. Such discussions, however, will have to be carried out with greater sophistication than in the book if they are to be useful.

The analysis of the WSO is, as stated in the book, "a celebration of the things I have found exceptional about the men and women of the Near West Side." But his portrayal is so one-sided that the organization and the individual in it are one-dimensional. The central question of the book is why the leaders, "many of whom had been engaged in criminal activities," made the decision and the sacrifice to work for WSO when they could have achieved wealth and status by going with the organizations that are currently major powers in the Near West Side ghetto, the political and criminal syndicates.

The answer: "I am forced to fall back on the notions of strength, self-understanding, and insight in trying to explain why these men, and others like them change their means of livelihood in the absence of a strong appeal from a social movement."

To the growing social science portrait of

ghetto life this book could have made but fails to make a significant contribution. Its chief utility lies in the portrayal by a black social scientist of the discovery of a portion of his own history.

*Coming Up Black: Patterns of Ghetto Socialization*, by DAVID A. SCHULZ. Englewood Cliffs, N. J.: Prentice-Hall, 1969. 209 pp. Clothbound, \$5.95. Paperbound, \$2.45.

RICHARD A. LAMANNA  
*University of Notre Dame*

This book is a welcome addition to the literature on the black family in the U. S. Unlike many recently published works on the black community, this is a carefully written, intellectually stimulating, and empirically based work which significantly adds to knowledge of the life style and problems of disadvantaged and handicapped black families in an urban environment.

The book is based on detailed case studies of ten black families who resided at one time in the Pruitt-Igoe public housing project in St. Louis, and is liberally supplemented with material from related studies carried out in the project by the Social Science Institute of Washington University. The ten families included a total of 108 persons and ranged in size from 6 to 18 persons. Five of the families were "complete" and five were headed by women. The mothers ranged in age from 31 to 50 and the fathers from 37 to 55. All of the families had at least some children in school.

All are caught in the tangle of pathology and disadvantage that plagues so many black urban families. Ten of the 15 parents were reared in the rural South. The average number of years of formal schooling of the five fathers is 4.4 years; the mothers average 7.8 years. Their estimated family income averaged \$4,475. Eight of the ten families are on some form of welfare although employment accounts for at least part of the family income in nine of the households. Three of the five men are physically disabled. Seventeen illegitimate children have been born to ten of the 15 parents. Sixty-one percent of the teen-age boys and 29% of the girls have police records. In sum, these families are not only black and poor, but older, physically handicapped, unskilled, poorly educated, burdened with many children, and new to the city. The cumulative effect of these handicaps is a crushing burden that Schulz describes with compassionate understanding.

The book is organized into six chapters, including a policy postscript. The heart of the

book consists of a chapter on male socialization, one on female socialization, one on marital conflict and the role of the adult male, and finally an effort to explain why stable marital and familial relations are so difficult an objective for these families. An unfortunate result of this organization of the data is that one never gets a comprehensive picture of each family as a unit—only fragments here and there that must be mentally pieced together—not an easy task even with a limited number of cases.

Although Schulz emphasizes the resourcefulness and adaptive capacity of the families, his findings belie the romanticism some militant blacks and white intellectuals engage in about Negro family life. We confront not simply an alternative way of life or a set of variant moral values but the real pain and suffering of family living that is built into the daily lives of these unfortunate people trapped and overwhelmed by the situation they must cope with. Moreover, the prognosis is not good. Schulz reports, "It does not seem likely that the parents ever will know any type of living much different from what they now experience." None of the older married children has done much better to date than his parents. Some of the younger children show somewhat more promise, but few are likely to overcome the difficult barriers to upward mobility.

Schulz concludes that ghetto families respond to the internal and external forces that threaten their very existence in a variety of ways, thus producing a variety of family life styles; but

Until the lower-class Negro man's status has been greatly improved in the world outside the ghetto through better training, better education, and above all, better jobs, it does not seem likely that men in the project will be able to assume a more central role in the life of their families, nor that they will desist in training their sons in the only techniques of survival that seem to work in their experience—even though these techniques of survival have a tendency to be detrimental to their family and to perpetuate its problems (p. 188).

The book is subject to criticism on several grounds. It is not made sufficiently clear either in the title or the text that the families studied are not representative of all blacks, low-income blacks, or even the 11,000 residents of the housing project. The study might better have been called "Family Life and Socialization in a Situation of Extreme Deprivation." Little is told about how the families were selected although the claim is made that they represent the most common types of families in the project. Regarding the problems of data collection, especially the participant observation by white

researchers in a black community, it is related only that some 250 ten-hour days were spent interacting with members of these families over a 3½-year period from 1963–1967 and that the families were willing to "tell it like it is." These ambiguities are coupled with misplaced efforts to achieve a level of abstraction and generalization not justified by the data.

At the theoretical level the author does develop a typology of male marginality that is descriptive of modes of adaptation but gives us little insight of a predictive nature—that is, why some adapt one way and others develop a different way of coping with their circumstances. Schulz also makes some effort to grapple with some of the opposing explanations of the origin and determinants of black poverty—that is, the extent to which it is a class phenomenon or a race phenomenon, culturally determined or economically determined. Unfortunately he sheds little light on the complexities of these relationships although he does in effect conclude that all the factors are of some significance.

Another disappointing feature of the book is its failure to explore the implications of the findings for public housing policy and practice. Although Lee Rainwater in the foreword expresses the hope that the findings "will prove useful in avoiding the kinds of public policy errors which Pruitt-Igoe represents and will point the way toward alternative policies that truly improve the situation of the poor generally, and the Negro poor in particular," the author does not note the errors of Pruitt-Igoe or the relevance of the findings to the specific problems of public housing. In fact, the project plays a very incidental role in the presentation of these case studies.

The book includes a six-page bibliography and an adequate index.

*The Affluent Worker in the Class Structure*, by JOHN H. GOLDTHORPE, DAVID LOCKWOOD, FRANK BECHHOFFER, and JENNIFER PLATT. New York: Cambridge University Press, 1969. 239 pp. Clothbound, \$7.50. Paperbound, \$2.25.

GORDON FELLMAN  
Brandeis University

Those who esteem sociology for its capacity to illuminate dimensions and recesses of real human issues will admire this book. It is in a series of papers and two books—*The Affluent Worker: Industrial Attitudes and Behavior* and *The Affluent Worker: Political Attitudes and*

*Behavior*—ensuing from research begun in 1962 in England by the two senior authors.

The book considers the common idea that as workers become more affluent they become more middle class in aspirations and behavior. This *embourgeoisement* thesis includes the claim that alienation diminishes with affluence and that the progressive absorption of the workers into the middle class lessens their potential as agents of social change.

Goldthorpe, *et al.* selected populations at three large factories in the town of Luton for their study. Virtually all their data derive from two long and largely open-ended interview schedules on work and home. The authors find that affluence for most workers means little more than living according to a standard of consumption they seek and enjoy.

Specifically, work is found monotonous and unabsorbing; and most interviewees admit preferring their present job over previous ones because of income rather than for intrinsic job satisfaction. Work seems to be endured rather than enjoyed, and even relationships with fellow workers and others at the factory are not considered important or particularly worth pursuing. Workers are not "integrated" into their firms; for instance, many do not prefer a salary to a wage and do not mind status barriers in canteen arrangements. Few expect to rise in the job hierarchies of the companies they work for: "instead of aspiring in white-collar fashion to make a 'good career' *within* their firms, these men hoped rather to gain a 'good living' *from* their firms—the typical aim was not a progressive series of jobs but the wherewithal to sustain a progressive advance in the material conditions of their out-of-work lives." They conceive of conditions bettering on a collective level rather than an individual one but not for reasons having anything to do with efforts by workers now or in the future.

The affluent workers' home lives indicate continuity with traditional working class life style. The workers and their families, in a traditional working class way, still prefer to spend time with relatives and nearby neighbors rather than engage in the middle class practice of choosing associates deliberately and selectively. Unlike most middle class people, and like workers in many other studies, they belong to few formal organizations. The exceptions to both findings tend to be people with white collar backgrounds, either in their own work histories or those of parents. Spare time is concentrated on home and family life. Some differences from standard working-class patterns emerge: sibling ties are often weak or non-existent, and many marriages approach the "companionate" form. But most

relationships are with other working-class people, with little or no aspirations to friendships with white collar people or emulation of their status-seeking efforts and symbols. The workers' families engage in little long-range financial planning, but many hope their children will attend college (although the parents in a white-collar control group have a much clearer sense of what college education is than do the affluent blue collar workers); the children do not perform exceptionally in school.

Most of the workers see society in terms of two classes or three and few see the relations of classes in terms of power or prestige. Rather they tend "to regard social class as being primarily a matter of *money*; or, to be more precise, of differences in the incomes, wealth and material living standards of individuals and groups."

So what? The *embourgeoisement* hypothesis is dealt a heavy blow. And the authors conclude that basic changes in the pattern of British social stratification would seem to depend not on consumption patterns but on radical changes in the structures of institutions. They recognize that workers may rest content with a rising standard of living, but speculate that the absence of satisfaction in work and the separation of work life from private life may reflect inherent dissatisfactions that could eventually erupt into efforts to change those conditions. The decline of interest in the Labour Party is seen as a function of the conservation of the Party as much as, or more than, a function of the growing affluence of the workers. The implication crucial in American stratification studies as well as those in Britain is that consumerism may preoccupy the working classes and political parties that claim to speak for them but that the sense of deprivation in terms of "other wants and expectations" ought not to be assumed dead.

Unlike many sociologists, the authors are fully willing to applaud the traditional Western liberal (and radical) values of human development and fulfillment as worthy and proper, to measure a real situation against that standard, and to find it sorely wanting. Their work leads them to questions about leadership toward structural changes in the directions of greater human fulfillment. It also leads logically to questions about the dynamics of "false consciousness," awareness of life's possibilities, and the possibilities of any person's gaining a little more of life. Students will find this book providing them with a remarkably clear, compelling statement of the weaknesses in the *embourgeoisement* thesis and a credible reality with which to contrast it.

*International Community Power Structures: Comparative Studies of Four World Cities*, by DELBERT C. MILLER. Bloomington, Ind.: Indiana University Press, 1970. 320 pp. \$11.50.

TERRY N. CLARK  
*University of Chicago*

This is an important book, synthesizing masses of information collected during seventeen years of research in Seattle, Washington; Bristol, England; Cordoba, Argentina; and Lima, Peru. Through systematic cross-national comparison, a veteran of American community power research illuminates a considerable number of issues which earlier were often more obscured than clarified by ethnocentric debate. *A priori*, many will regard this continuing reliance on a reputational methodology as cause for serious skepticism. Indeed, the early study of Seattle has been replicated in each subsequent setting. The remarkable cross-national differences in findings, however, forcefully demonstrate its utility. Criticisms that results deriving from standardized reputational questions are predetermined and that they inevitably overrepresent the business sector, are effectively countered: leaders from business and financial institutions were shown to receive 67% of the mentions in Seattle, 43% in Cordoba, and 7% in Lima; while analogous figures for governmental and political leaders were 50% in Lima, 21% in Cordoba, and only 8% in Seattle (p. 215).

The core of the analysis consists of comparing percentages of this sort, not only for leaders but also for community and national institutions. In this way Miller shows that there is a generally high correspondence (.84 or more) between rankings of particular institutional sectors at the national and local level and that most (60% to 100%) reputedly powerful individuals are drawn from these same institutions. That only 60% of leaders in Cordoba were drawn from the five most important institutions (p. 217) illustrates a problem in the general approach, as does Miller's solution.

Tied to a standard methodology in order to maintain quantitative comparability, Miller makes considerable efforts to complement it with alternatives. As other researchers made advances or when Miller was dissatisfied with his own findings, he showed continual ingenuity in incorporating new tools and methods of analysis. His candid self-criticism and adaptability in this regard are admirable. Thus, when the military and Church were ranked high in Cordoba but few leaders from either institution were named, Miller portrayed inter-institutional linkages with

extended quotations from informants. Elsewhere, there are sensitive and detailed materials on clique behavior (pp. 190-194), extended family ties, (pp. 168 ff.), the role of North American executives in Cordoba (pp. 131-144), fifteen value scales (pp. 228-258), and predicting decisional outcomes (pp. 82-102).

A modified decisional methodology generated results roughly similar to those produced by the reputational questions, but more detailed analysis of specific decisions would have helped provide a better feeling of relationships among leaders and of the local political culture. The suggestive typology of issues (p. 49) might have been elaborated for these purposes. Local informants supplied lists of important recent issues; but since these were not comparable across communities, detailed comparisons were impossible. It might have been preferable to study a few more comparable issues in each community.

There is also very little concerning specific institutional structures. More traditional legal analyses in most countries supply some information on these matters which would have been useful to build upon more systematically. Issue areas where the national government influenced the local community through established institutions could thus have been distinguished from more informal influence relationships. But even in seventeen years, one man cannot do everything.

Throughout there are useful discussions of alternative methodologies that were explored, and why they were or were not used. There is frank recognition of gaps where they appear, and suggestions are offered as to how they might be filled. There is ingenuity—sometimes daring—in quantification and use of tables and charts. The last chapter presents a demanding program, with detailed suggestions, for future research.

As the first systematic investigation of community power in four countries and the first to grapple from this perspective with the impact of national factors on local elites, the book is both innovative and provocative. It will provide a bench mark for study, debate, and criticism for years to come.

*Pressure Groups and Power Elites in Peruvian Politics*, by CARLOS A. ASTIZ. Ithaca, N. Y.: Cornell University Press, 1969. 316 pp. \$12.00.

JOSE MORENO  
*University of Pittsburgh*

Astiz uses a conceptual framework of modified class conflict to analyze the structure and process

of political life in Peru. He is well aware of the dangers of macropolitical studies, but also knows that micropolitical research has often led to distorted conclusions or to trivialities. His study of Peru is, consequently, an attempt to present the reader with a total, although at times impressionistic, picture of a political system in action. In contrast to other studies of Peruvian politics, which are mainly descriptive and emphasize the "what" and "who," the present book purports to be analytic and concentrates on the "why" and "how" of the rather complicated maze of underdeveloped politics.

Interclass and intraclass conflict and interest articulation are the two main processes the writer seeks to understand and explain. It is emphasized (p. 190) that in the Peruvian situation interest groups are identifiable and dominated by the social class to which they belong and that, consequently, they articulate the interests of that class in opposition to those of other groups or classes. However, it seems that the concepts of class and conflict are relegated to a less prominent position than they deserve in analyzing the Latin American context. As pointed out in the foreword (p. ix), in Latin America "class conflict, is a real experience for millions, as is the exploitation of class by class." It seems that Astiz is as shy of openly and fully using the concept of class as those intellectuals whom he blames for keeping to themselves the perception of the existence of class conflict "lest the professional anti-Communist should pronounce them guilty" by associating them with the Communists (p. 24).

Nearly two-thirds of the book describes the various classes, interest groups, and power elites that struggle for economic and political power in Peru. Two such groups, the Church and the Military, are cast into special relief since they are seen, not as pressure groups, but as power elites that permeate all aspects of the political structure. The Church is credited with having helped create the philosophies of the various classes, i.e., conformity for the lower class, mediocrity for the middle class, and supremacy for the upper class. The three philosophies furnish religious underpinnings to the ideology of the traditional upper class which seeks to maintain the status quo. Competing political ideologies, such as those of APRA, Communism, and even Christian Democracy now represent a threat to the influence of the Church, particularly over the lower classes. However, contrary to what is happening in other Latin American countries, in Peru, with the exception of a few priests turned revolutionaries, the Church is an ideological monolithic structure that supports the traditional status quo.

The other pillar of the status quo, the military, offers channels of upward mobility to children of middle class origin, particularly from the country and small towns. Within Peru's tormented history of military governments, the military is the referee of political disputes and the "balancer" of interclass and intraclass conflicts, always blameless and always bound to throw their support to one of the groups struggling for power. Since 1958 a new breed of military officers has emerged. They were instrumental in the coups of 1962-1968 and oppose the generals of the old school concerning the function of the military in political matters. Astiz concludes, however, that "the military continue to be the most influential political party, the most effective trade union and probably the key power holder in Peruvian politics" (p. 161).

The last three chapters of the book analyze in greater detail the problems of interest articulation of the various interest groups, the external factors influencing Peruvian politics, and the possibilities for revolution. A postscript ventures some premature views on the coup of 1968. The reader is given a peek into the highly complicated network of groups competing, conflicting, and articulating with each other. Astiz agrees that foreign "penetrators," particularly the U.S., through military and economic penetration have had a share of internal political power, but disagrees with what he calls "exaggerations" of some writers who suggest that Peru is only a province of IPC. As for revolutionary possibilities and sources, either from above (oligarchy, military, church) or from the bottom (peasants, reformist officers and priests, new middle class), neither seemed to be around the corner for Peru in 1968. The coup of 1968, according to Astiz, has not basically changed this outlook. It is clear that, for Astiz, revolution is equal to "substantial redistribution of power and social rewards" (p. 249). It is not clear, however, why the concepts of revolution and reform are often used interchangeably, as when APRA is said (p. 181) to have abandoned its *revolutionary ideals* and later (p. 182) to have changed from a "*leader of reform*" to a party that subscribes to the *status quo*. (Was APRA ever revolutionary?) Similarly, some priests and officers are said to be revolutionists and later are called reformists if not revisionists (pp. 181-187; 275). To confuse revolution with reform could bring serious consequences, as witnessed by the so-called "revolutionary middle class" (p. 258). With such minor reservations this book should be welcomed by all students of Latin American politics. Carlos Astiz deserves to be congratulated.



*Personality and Politics: Problems of Evidence, Inference, and Conceptualization*, by FRED I. GREENSTEIN. Chicago, Ill.: Markham Publishing Co., 1969. 200 pp. \$5.95.

JERRY B. MICHEL  
*Memphis State University*

Assuming that personality characteristics influence political behavior, Greenstein sifts the personality and politics literature, answers some objections which have harassed this line of study, and offers a schema and suggestions to guide future research. He argues (1) that the notion that personality characteristics are less important for behavioral outcomes than social traits depends on the analytically possible but empirically impossible feat of distinguishing the relative influences of social and personality characteristics; (2) that the assertion that personality factors are randomly distributed and, hence, neutralize each other begs the empirical question of the relationship between personality and political behavior; and (3) that the argument that individual actors or specific actions are dispensable is countered by instances such as the Cuban Missile Crisis and Woodrow Wilson's failure to obtain ratification of the Versailles Treaty. Greenstein's position is that research should delineate the conditions under which specific men and/or behaviors are indispensable.

Greenstein suggests a three-fold classification of personality and political behavior studies: investigations of single political actors, analyses of types of political actors, and studies of the aggregative effects of personality characteristics on political systems. He adopts and slightly modifies M. Brewster Smith's depiction of the interrelations of social environment, personality development, and political behavior, and proposes three foci of attention for individual psychological biographies: descriptions of patterns of behavior, interpretations of these patterns and their interrelations, and explanations of geneses of personality dynamics. These foci were used to organize a brief discussion of Alexander L. and Juliette L. George's psychopolitical analysis of Woodrow Wilson. To illustrate the typological approach to personality and politics, Greenstein discusses authoritarianism and James Barber's typology of Connecticut legislators. Regarding the aggregation of personality traits, he clarifies the difficulties and possibilities of linking personality structures with large-scale political structures and processes through intermediating, individual political beliefs and actions. The final section, written by Michael Lerner, consists of bibliographical notes which parallel and supplement each chapter.

Several flaws in Greenstein's analysis should be noted. First, in declared deference to much available literature, the important topic of political attitudes is not treated. Secondly, of all the dimensions of personality available for study only ego-defensive functioning is considered. Finally, the reciprocal influence of politics on personality is recognized, but neither explicated nor explored. Nevertheless, this work offers a plausible schema to which a disparate, growing body of personality and politics literature may be oriented and by which its future development may be guided.

*Politics and Change in Developing Countries: Studies in the Theory and Practice of Development*, edited by COLIN LEYS. New York: Cambridge University Press, 1969. 289 pp. \$7.50.

GEORGE HELLING  
*St. Olaf College*

For an American sociologist this book is a cross-cultural experience in some depth as it consists of a series of papers—by British social scientists reacting to an American perspective and treating problems in several countries of the developing world—first presented at a 1968 conference at the University of Sussex. The editor calls the American "behaviouralism" to which they are responding "the revolution in political science," though it seems to draw its major theoretical inspiration (Weber, Parsons, Merton) and empirical orientation from sociology. In consequence, though these papers represent only one end of the dialogue, readers who remember when functionalism was revolutionary here will be able to reconstruct the rest of it.

Behavioralism thus defined has acquired a somewhat stodgy, establishment look in the U.S.; but its fertility and power as theory come through impressively when it is juxtaposed with British explanatory resources from history, political philosophy, and practical experience in colonial and other cross-cultural administration. The writers vary considerably in their capacity to synthesize the two perspectives, but the book as a whole demonstrates that the attempt is worthwhile.

It is probably not surprising that the writing itself is characterized by a more humanist, less technical vocabulary than Americans use on similar topics. Although the papers leave one unconvinced that "plain English" is wholly adequate as an analytical tool, this use of vocabulary from the common domain might be especially helpful in interdisciplinary courses.

A bibliography just short of 600 items, including a number of authors and British journals that rarely come to the attention of the American student, make the book useful despite a neglect of writers interested in social psychological factors.

It is impossible to summarize papers of this variety, particularly as each makes a point of establishing its author's independent stance *vis à vis* numerous others on his topic. A partial focus is provided by Leys in the final paper "The Analysis of Planning." Here Tinbergen's "synoptic problem solving" model—in which state planners are expected to analyze the economic structure, set growth rates, and select the manpower programs to achieve them—is contrasted with Lindblom's "strategy of disjointed incrementalism" model—in which responsibility for planning is divided among many agencies and groups, and continual reformulation of programs goes on, aimed at solving immediate problems and achieving small increments of change. In developing countries where private investment is stressed and the rights of citizens to a wide range of alternative choices are protected, synoptic planners have proven unable to predict or make allowances for the indirect effects of the plan's operation (i.e., new political alignments or a change in birth rates), to suggest optima among alternative mixtures (all involving values), or to deal with such factors as international price levels and aid flows. Consequently, "the plan"—one carefully measured ingredient in a vast interacting mixture—has rapidly become "irrelevant to conditions" and has been scrapped or ignored.

One way that synoptic planning can be *made* to work is by means of a "dyarchy" where party organs duplicate state organs at all levels. Rejecting the Djilas assertion that it is merely parasitic, Alec Nove sees the Soviet Communist Party as replacing prediction with its functional equivalent: control. The party gives encouragement, recognition, and the practical rewards and punishments that replace the profit motive; and it only possesses the discipline to put broad and long range interests ahead of parochial ones. Liberalization in such a regime has limits, and economic development seems to have high non-economic costs.

The polar alternative, incremental planning, is compatible with democracy at least for a time and is responsive "to the complexity and unanticipatable character of social change"; but adjustment taken as an end leads in a developing country to such painfully familiar problems as misallocation of scarce resources, programs that pre-empt future development, a tendency to deal with symptoms while basic problems

accelerate, and, typically, a widening gap between expectations and capacity to satisfy them. These and the attendant effects on governmental stability are dealt with by several authors in different contexts.

Considering synoptic planning unworkable and incremental planning mere muddling through, Leys suggests a middle course based on an analogy to a business firm. Instead, for example, of establishing a particular percentage increase in GNP as a goal, he advises setting practical and achievable objectives. The political leader may, but need not, consult experts; for in Leys' opinion setting goals is "essentially an act of will" and not an optimal solution to a problem. After considering the evidence now available on the limitations of other models, a no-nonsense acceptance of the political nature of "what can be made to happen" seems refreshingly simple. It is, however, less refreshing and too simple to assume that the kinds of alternative economic and social consequences planners face can be avoided if decisions are made without thinking much about them. Leys has not resolved the synoptic-incremental dilemma though he and his collaborators have posed a challenge to a key assumption of the social scientist as advisor—namely that he has the capacity to solve problems, not merely suggest "acts of will" complete with legitimations.

This book faces a hard audience. "Behaviouralism" as interpreted by its authors (and those Americans they quote most frequently) is functionalist with heavy admixtures of humanist practicality. Marx is mentioned but not by his friends, and sociology of conflict is essentially unrepresented. Most of those committed to action in the developing world probably already have their comments on their placards.

*Socio-Economic Change and the Religious Factor in India: An Indian Symposium of Views on Max Weber*, edited by CHARLES P. LOOMIS and ZONA K. LOOMIS. New York: Van Nostrand Reinhold Co., 1969. 140 pp. \$5.00.

WILLIAM Y. DESSAINT  
*The New University of Ulster, Northern Ireland*

In February 1966, more than thirty social scientists—about two-thirds Indians and one-third Americans—met for four days in Hyderabad under the auspices of the Indian National Institute of Community Development to discuss "the strains of conservatism, liberalism and radicalism in the Indian culture, its worldliness

and other-worldliness and the consequences of such attributes for social and economic change" (p. vi). They were particularly concerned with the role of religion in facilitating or hindering socio-economic change.

The editors consider the views of Max Weber on early ascetic Protestantism and on Hinduism with special reference to their relationships to the degree of development (or lack of development) of modern western-type bureaucracy and economy in India. They discuss Weber's ideas on these matters under a number of headings: belief, sentiment, goal (end or objective), norm, status-role, rank, power, sanction, facility. They also consider comprehensive processes (communication, boundary maintenance, systematic linkage, social action, socialization, and institutionalization) and conditions of social action (territoriality, size, and time). Each section is followed by comments made by social scientists attending the symposium and by the editors' assessment of these comments.

On the whole, this method of presentation is instructive; but it is sometimes inadequate and even irritating because Weber's ideas tend to be chopped up to fit the editors' own categories, and there is not sufficient coherence due to frequent jumping from one kind of criticism to another. What is badly needed is a general synthesis of all the scattered strands of thought. As it is, one closes the book with the feeling that the basic question has not really been answered. In any case, several objections to this question may be raised. One of its most obvious defects is that the religious picture of the Indian sub-continent during the last three thousand years is vastly more complex and heterogeneous than the "ascetic" Protestantism which developed in Europe at the time of the Reformation. Thus, Hinduism is not directly comparable to the Protestant ascetic sects. Besides, the picture having altered considerably over the centuries, much of what may be true of India since the British conquest does not necessarily apply to India before the British occupation. Another major fault is that causal relation between religion and economic development is not by any means a one-way process. Hinduism is said to have restricted spatial and social mobility, inhibited identification with large non-kin groups, and hindered the transmission of knowledge. To the extent that this may be true, how much is due to social factors such as the existence of a caste system and the constant struggle for power between rival states? Would it not be more useful first to seek the purely economic reasons for the relatively slow rate of economic development in India, especially under the British *raj*? A group of social

scientists examining China in the same perspective would have been more likely to approach the problem initially from an economic point of view.

As one might have expected, some of the participants in the symposium repeatedly accuse Weber of having based his views on an inadequate knowledge of Indian society and frequently contradict him on specific points. However, it is unfortunate that the comments, though sometimes interesting, are seldom constructive and add very little of importance to Weber's analyses.

*Social Change in Modern India*, by M. N. SRINIVAS. Berkeley, Calif.: University of California Press, 1969. 194 pp. Paperbound. \$2.45.

ROY DEAN WRIGHT  
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Srinivas has presented an excellent discussion of historical and contemporary social processes in India. The student of social change may, however, find the work a disappointment. Except as the processes of social change relate directly to stratification or westernization, little analysis is undertaken.

*Social Change in Modern India* generally considers one basic theme, the means through which a caste group changes its position in the traditionally closed status system via a process involving, chiefly, the emulation of traits considered the domain of higher caste groups. Through sanctioned emulation a lower caste group may improve its status over time. Thus the caste structure, traditionally considered to be unflexible, allows status mobility. Srinivas herein presents his most important ideas: (1) mobility is allowed in the caste system through emulation of traits traditionally considered the sole purview of higher caste groups (i.e., *sanskritization*); (2) such mobility does not change the structure of the caste system, only the positions internally held; and (3) the process of westernization (especially the pervasive influence of the British) added to the acceleration of *sanskritization*. Srinivas culminates his discussion with an examination of the validity of research within India.

Despite the strong points of the book, one cannot help noting several deficiencies. Perhaps the process of *sanskritization* is not unique to the India caste system but is little more than a general process of social mobility underlying all stratification systems of both emerging and

emerged societies. Or perhaps it is nothing more than Veblen's basic idea of "conspicuous consumption" put into the Indian context. In any event, the concepts of sanskritization, westernization, and secularization are left excessively vague. Yet, as Srinivas states, "Vague terms have their uses."

*The Politics of Untouchability: Social Mobility and Social Change in a City of India*, by OWEN M. LYNCH. New York: Columbia University Press, 1969. 251 pp. \$10.00.

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Of all India's social institutions the village, the joint family, and the caste system have attracted the greatest attention of western social scientists. In recent years these have been studied within the framework of the evolving theories of modernization. Despite the ethnocentrism which often mars the studies of post-colonial, non-western societies—a blemish from which Lynch's work is happily free—a healthy feature of this area of investigation is its interdisciplinary character. Lynch, drawing concepts from the theory of reference groups, seeks to find out how Indian independence and the subsequent commitment to a democratic government have affected an untouchable caste's style of achieving upward mobility.

The practice of untouchability is undoubtedly the most pernicious aspect of the caste system. But the poverty and plight of untouchables are common knowledge; what the untouchables themselves are doing to raise their status and power is a subject about which less is systematically known. Lynch makes a distinct contribution in enhancing our understanding of the nature, extent, and efficacy of these efforts.

After setting forth the purpose of his study and introducing his core concepts, the author discusses the forms and functions of protective discrimination in favor of the *scheduled castes* in such fields as education, government employment, and political representation. This is followed by a brief account of the Jatavs, a shoe-manufacturing caste, and a description of the complex structure of their economy. Chapter 4 is devoted to an analysis of the political organization of the untouchables in pre- and post-independence periods. Here Lynch undertakes a functional analysis of the Republican Party and points out such political liabilities of the Jatavs as factionalism, inadequate and weak leadership, paucity of financial resources, and a

rather widespread sentiment of narrow communalism. How Buddhism has become a revitalization movement, how it provides both continuity and change for its untouchable adherents, and how the late Dr. Ambedkar (the untouchable law minister and architect of India's constitution), became the culture hero of the Agra Jatavs are discussed next. Lynch also speculates on the future of Buddhism as a revitalization movement in India.

Lynch does not subscribe to the so-called textual view of caste as a totally rigid and static system. He suggests, rather, that it is an adaptive structure "through which its members can relate and re-relate themselves to members of other castes in terms of the potentialities provided to them by the socio-political environment in which they interact." He concludes that group mobility on the part of some of the lowest castes in contemporary India takes place not through their adopting the life style of the twice-born castes but through political participation, which is a functional alternative to the phenomena of Sanskritization and Westernization.

However, as Lynch himself passingly notes, the case of the Jatavs of Agra may not be quite typical: the majority of India's untouchables still live in villages; and they, unlike their urban counterparts, have not so clearly taken to political action as a means of upward social mobility. Nevertheless, it is quite likely that, as India enters a phase of the politics of instability, organized—if opportunistic—involvement in the political arena may come to be regarded by all social classes as the most effective way of climbing the status ladder.

Although Lynch was a participant observer among the Jatavs in Agra for 16 months, he says little about his method and techniques of investigation, making it difficult to assess the credibility and accuracy of his findings and conclusions. Thus, when he refers, for example, to the report of "one reliable informant," we do not really know how reliable his reliable informant is. Since his book throws so much light on the general process of group mobility, it is a pity that it does not include a statement of the design and method of inquiry.

Despite this omission, Lynch's work has considerable merit. It studies stratification and social mobility within the larger context of a traditional system, national planning, and the politics of scarcity. Careful analysis is capped by a restrained advocacy on behalf of the study population. Lynch's concept of *elite emulation* has greater explanatory fertility than Srinivas' useful, but culture-bound notion of Sanskritization. This study is, in sum, both competent political sociology and relevant urban anthropology.

*Brazilian Secondary Education and Socio-Economic Development*, by ROBERT J. HAVIGHURST and APARECIDA J. GOUVELA. New York: Praeger Publishers, 1969. 321 pp. \$15.00.

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The purpose of this study was to examine the "middle schools" of Brazil to learn if they are meeting the needs for Brazil's social and economic development. The "middle schools," attended between the ages of 12 and 18, are equivalent to the junior and senior high schools of the United States and to the combination of academic secondary schools, vocational schools, and terminal primary schools of several European countries.

The following research questions were asked: Who goes to what type of school? How does the pattern vary in different parts of the country? How does middle schooling relate to the labor force of Brazil and to its development? To what extent and in what ways does middle schooling contribute to social mobility in Brazil? How are attitudes toward social change and modernization related to the middle schools? How is the economic production of the changing Brazilian society related to the system of middle schools? In what way do the middle-schools respond to explicit economic-educational planning and to the broad but unplanned demands of the developing economy? The first chapter "lays a basis of fact about the recent economic and social development of Brazil." The guiding assumption of the research is that

As a society moves from an underdeveloped to a developed economic state, its labor force must shift from predominantly unskilled, to semi-skilled, and technically trained persons. This means that its primary and middle schools must expand to serve much higher percentages of the population. Large numbers of people must be trained to work at the middle-level positions of commerce and industry.

Some of the conclusions of the study are that (1) although the attitudes of a traditional aristocratic and manual work-demeaning society are visible, these attitudes are not widespread among middle-school students; (2) some planning must be done to foresee the need for trained manpower in certain jobs and certain sections of the country and to develop middle schools to meet these needs; (3) the Brazilian middle schools do fairly well on the criterion of social justice applied to the two middle classes, but they fall down when this criterion is applied to school attendance of working-class youth; and (4) middle schooling in Brazil is quite different from that of other developing

societies in that it meets the immediate needs for commercial-industrial growth.

Havighurst and Gouvela have obtained valuable information on the functioning of the middle-schools system in relation to the socio-economic development of Brazil. But the principal merit of their work is the ample and relevant description they give of the various alternatives in use for recruiting and training people, including (1) expansion of enrollment in middle schools; (2) immigration of foreign workers with a requisite level of education and skill; (3) informal education "on the job"; (4) educational services outside the regular schools system; (5) employing students part-time; and (6) the flexible use of educational resources all over the country: a given school building may be used as an academic school in the morning, a normal school in the afternoon, and a commercial school in the evening; and teachers may teach in two or three different types of school.

Indeed it does appear "that the Brazilian middle schools may function rather well in a flexible and changing system of commerce and industry that is not highly efficient but keeps on moving ahead."

*Proper Peasants: Traditional Life in a Hungarian Village*, by EDIT FÉL and TAMÁS HOFER. Chicago, Ill.: Aldine Publishing Co., 1969. 440 pp. \$9.75.

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*Proper Peasants* is the result of over 14 years of field work done by two Hungarian ethnographers and published in cooperation with the Wenner-Gren Foundation. The authors' stated objective is to describe the "traditional peasant culture" of the Hungarian village of Átány. The finished work attests to their success.

As the authors point out, Central European ethnography has its roots in the humanities and in museum-based studies of material culture, which usually focus on a description of specific aspects of rural European peoples and are not cross-cultural in nature. These traditional areas of concern come out very clearly in the book.

The organization of the material strikes the reader used to American and British ethnographies as strange: Part One, "The Village and its People," reviews the broad outlines of the village past, describes Átány's place among Hungarian villages, gives demographic characteristics, describes landholding and tenure systems, and contains an interesting section dealing with the seating arrangement in the local church which forms a sort of microcosm of the village

social structure. Part Two, "The Family," includes sections describing the construction of the houses and farm buildings, family membership, division of labor within the family, and marriage. Part Three, "The Network of Social Relations," reviews kin ties, neighborhoods, age groups, and wedding and funeral observations. Part Four, "Ways of Life and Social Strata," covers levels of subsistence, occupations, mobility; and the rise and decline of particular families in the village. The last part, "The Community," deals with religion, government, relations between Átány and the outside world, and ends with an epilogue that briefly reviews some postwar changes in the village. As the range of topics indicates, the book goes beyond the traditional Central European concern with folklore and material culture and tries to emphasize the social organization of the village. The authors are to be congratulated on their successful adaptation of Central European ethnographic traditions to American and British interests.

The book will prove helpful to researchers interested in comparative sociology. Its depth and general descriptive orientation is often lacking in contemporary ethnographies, which tend to focus on a specific topic or theoretical problem. Theoretical works on narrow topics need to be done, but not to the exclusion of generalized descriptive reports.

Critically evaluating such a general work is difficult. However, I would like to have seen a more comprehensive review of contemporary conditions in the village. For example, table 9 (p. 67), indicates a dramatic shift in the occupational structure of the community after World War II; but this change is not discussed in the section on Átány today. A number of forces apparently at work in Átány after the war are alluded to but not systematically followed-up. Thus, the local Communist Party is referred to in the text (p. 305), but no mention is made of it in either the section on politics or the epilogue. A cooperative weaving shop is also mentioned (p. 203), but no other information is given on it. Certainly omissions such as these are minor and understandable given the emphasis on traditional culture, but this sort of information would be useful in understanding the changes which have occurred in Átány and much of rural Central Europe since the war. Also, the emphasis on traditional culture, which even when the fieldwork began was rapidly changing or already dead, gives parts of the report a "memory culture" flavor. The authors do recognize this problem (p. 382) and seem to have made real efforts to overcome the misrepresentations which often occur in such monographs.

These comments are made not so much in

criticism of what has been done as in anticipation of what might be done in other studies in the future. It is to be hoped that similar cooperative projects will be conducted again. Such efforts are to be encouraged, not only because they give English readers access to the relatively inaccessible fruits of Central European ethnographers' labors but also because the cross-fertilization enriches both ethnographic traditions. *Proper Peasants* should be read by anyone with an interest in Europe or in peasant societies.

*Latin American Peasant Movements*, edited by  
HENRY A. LANDSBERGER. Ithaca, N. Y.: Cornell University Press, 1969. 476 pp. \$12.50.

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An analysis of agrarian movements in Latin America is long overdue. The case studies, selected from Cornell's 1966 seminar on Latin American peasant movements, included in this volume are from seven nations (Bolivia, Brazil, Chile, Guatemala, Mexico, Peru and Venezuela); they vary from an analysis of the Zapata Movement in Mexico to a vineyard workers' strike in Chile. All include extended analysis of background factors and a common core of analytical foci which provide some continuity to this collection. The guiding theoretical model is developed in Landsberger's opening chapter, the most penetrating analysis of peasant movements this reviewer has yet seen. Utilizing data from agrarian movements spanning six centuries, Landsberger has produced a felicitous union of history and sociology. The relationship of peasant movements to development and modernization, as well as to urban labor movements, is explored. By emphasizing the rational elements in peasant movements, he generates fifteen hypotheses that constitute part of the model utilized by the authors of the case studies. Major analytical foci include historical conditions making the occurrence of peasant movements probable; the nature and function of goals; the composition and characteristics of the leadership; the ideology, organization, and methods of the movement; the societal base which causes the movement to flourish or decline; its allies and enemies; and the nature of its successes and failures.

A collection of articles usually lacks consistent quality, and this effort is no exception. Some of the case studies are exceptionally well done, including those by Robert A. White (Mexico), John Powell (Venezuela), and editor Landsberger (Chile). Some of them, however, suffer from unfocused generality (Dwight B.

Heath's description of Aymara Indians) or from partially digested details and "acronym-itis" (Neale J. Pearson's recounting of the Guatemalan Movement of the 40's and 50's). In chapter after chapter the repetition of unfair and unjust conditions forced on peasants becomes redundant. Lengthy background descriptions in some chapters make reading the book a tiring experience. It is unfortunate that Landsberger was not as creative in his editing as in his own writing. But perhaps one of the strengths of the volume is the psychological effect it produces in the reader: frustration. The final clincher is Ernest Feder's concluding chapter on "Societal Opposition to Peasant Movements," which systematically explores the "syndrome of frustration" until the reader has an urge to revolt!

Although an excellent beginning, this volume is more suggestive than helpful in understanding Latin American peasant movements as a sociological type. Much of the data question Landsberger's hypotheses (such as his assertion that regional and local leadership will come from local bourgeoisie, craftsmen, shopkeepers, lawyers and lower clergy). The irrational element is obvious in some of the case studies, and the "great man theory" will surely not die from the evidence in this volume. Yet the theoretical model seems helpless in handling such phenomena. How can such facets be woven into a revised model? In spite of this, both scholars and students will find Landsberger's model stimulating though probably only students will receive much assistance from most of the case studies.

*Movement and Revolution*, by PETER L. BERGER and RICHARD JOHN NEUHAUS. Garden City, N. Y.: Doubleday and Co. (Anchor Books), 1970. 240 pp. Clothbound, \$5.95. Paperbound, \$1.45.

*Blueprint of Revolution: The Rebel, The Party, The Techniques of Revolt*, by RAYMOND M. MOMBOISSE. Springfield, Ill.: Charles C Thomas, 1970. 336 pp. \$9.75.

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Though both these books seek the meaning of revolution, *Movement and Revolution* is analytical. Its authors discover that they are in different subcultures of the revolutionary tradition by analyzing their respective positions. Berger is a rationalist and a neo-Calvinist, believing that men should not suffer though the world is harsh and prone to violent revolution because of social organization. He finds the *status quo*

unsatisfactory, though because of the number of corpses in history he is suspicious of progress doctrines, violent innovations, and anointed leaders. Though he predicts that scholarship will retreat before either violent revolution or the fusion of students and establishment, he avoids condemnation of this retreat even if it be against his personal interest. One questions exactly how he may come to such a conclusion, however, after seeing youth through his eyes—alienated, intolerant, violent, and often "sick."

It is Neuhaus who has the revolutionary's views, imbued with the mindless synthetics of pure activism. He begins his analysis of revolution by revealing the antithetical relationship of mindless action and action after analysis. A careful evaluation of the ethics of violence leads to familiar systemic dilemmas: the problem of goals arises since the bloody pursuit of ill-defined goals generates unanticipated consequences and articulated goals divide member groups. Neuhaus also questions backroom planning, considers the uses of ignorance, and ponders the ethics of murder, torture, and hell-to-end-hells. In general, his conclusions seem musty, often boring, and sometimes dangerous since, as a revolutionary, he selectively ignores critically relevant facts from either the past or the present. The neoplatonic One, long familiar as a university goal and, later, as a goal of scientific research, becomes the ideal goal of The Movement. Meaningful developing social change theories and technologies, according to Neuhaus, do not exist. He spends pages on the question of the uniqueness of violent revolutionaries, saying that they only "want change" (he convinced me only that they want blood). Whatever his own interest, he builds an ethical case against violent revolution in the U. S.

Thus, Berger and Neuhaus analyze the consequences of two choices at today's universities: continuation of the Enlightenment or violent revolution. Neuhaus' analysis of the ethical implications of violence by one who is sympathetic with it is almost as rare as Berger's defense of reason, making the book unusual enough to assign as supplementary reading to students of change and theory.

Momboisse's *Blueprint of Revolution* attempts a synthesis of the above views by describing to the "average person" the phenomena of revolt and relating otherwise irrational and disconnected events. In 25 chapters divided into three sections (revolutions, revolutionary party, and tactics), the author discusses such topics as party organization, who joins, prison, psychological warfare, nonviolent techniques, sabotage, mob, and urban guerilla warfare. In these chapters 292 additional subheadings discuss such subjects as fanatics, tiers, cadre, and party mass.

Thus, the book reflects impressive completeness, presenting many of the current beliefs about the revolutionary process. If the reader wishes, he can find in *Blueprint* why reforms are detested by activists; why later violent stages of revolution call for execution of liberals (Neuhaus lists on p. 221 those who might need to be shot in the second stage: "Galbraith, McGovern, McCarthy . . . Chavez and, if someone had not already seen to it, Martin Luther King"); why demonstrations, riots, and mobs usually seem more than spontaneous; and why activists call for demands that cannot possibly be met. In order to simplify the book—or appear to do so—Momboisse does two things which make the book unacceptable as a text. He presents a bibliography of five hundred sources while his text has not one footnote or reference, thus presenting all hypotheses as facts. Many familiar observations of such activists, terrorists, or theorists as Trotsky, Engels, Bakunin, Mao, Ghandi, or even Linebarger, appear verbatim as if written by one author. When Momboisse thus chooses to present many writers' insights as those of one man, he also must present as one their many frames of reference. Even more distracting is that in page after page masses, jungle camps, barricades, and troupes are encountered but never explicitly in their ideological, ecological, or time contexts. The author's refusal to identify even his own hypotheses make one wonder if other writers have also perceived all the "symptoms" of revolution listed by him. Have minorities shown patterns of ignoring countless opportunities and of feeling sorry for themselves? Without references one never knows. The reader not accepting the printed word as authority may not accept Momboisse's anti-revolutionary message either. Momboisse could even appear to be authoring propaganda—which the book definitely is not. The preface of *Blueprint* is by Governor Reagan of California. To meet the need of public safety officials for more knowledge of the revolutionary process seems to be the objective of the book. It would probably serve that need about as well as one can hope today.

*Rebels Against War: The American Peace Movement, 1941-1960*, by LAWRENCE S. WITTNER. New York: Columbia University Press, 1969. 339 pp. \$10.00.

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As this book's title indicates, historians rush in where sociologists fear to tread. Despite the

subtitle, Wittner, a Vassar history professor, actually covers pacifism from its popularity in the thirties, through its wartime collapse, and on into its resurgence throughout the sixties, beginning his investigations where Merle Curti's end. However, Curti's two main works on the peace movement from 1636 to 1936 had the advantage of antiquarian charm. For instance among the justifications advanced by early pacifists was that war was "unnatural," i.e., man is the only animal which fights its own species. In contrast, because of its recency and widespread publicity, Professor Wittner's material is often tired and, with the exception of two excellent chapters, uninformative.

These, chapters three and four, discuss the treatment of conscientious objectors during World War II, little-known because of General Hershey's policy that the less the public heard of CO's, the better. CO's were hidden in Civilian Public Service camps. Despite their above-average education and intellect, the presumably constructive alternative to war they found was "manicuring trees." Dr. Don DeVault, a research chemist in a CPS camp, was doing independent research on penicillin molds. Refusing to stop his research when assigned to digging ditches, he was transferred to jail. Protesting circumstances such as these, the CO's began humor-tinged slowdowns and strikes, reporting the concurrent sicknesses of men whose last names began with the same letters of the alphabet.

These non-violent resistance techniques were the seeds from which grew the revolution which by 1941—after pacifism's disastrous decline facing Nazism—was necessary to save the peace movement from extinction. Traditional pacifism, largely religiously-based, split into two post-war camps: (1) scientist-led, world-federalist "nuclear pacifism"; (2) politically-oriented demonstrations of "non-violent resistance" and "radical pacifism," using techniques developed in the CPS camps. Pacifism reborn had political, as well as moral relevance.

Wittner's patent unfamiliarity with social scientific methodology, the logic of experimental design, and even the logic of defensible argument may dismay the sociologist. Whereas the assessment of changes in the public acceptability of pacifism over decades might strike the sociologist as a problem of exceeding complexity, to this historian it is apparently relatively simple, requiring almost no time even to clarify basic concepts. Frequent statements such as "A newspaper poll found that Negroes rejected a 'non-violent, civil disobedience campaign' by an almost three to one ratio" (pp. 65-66) appear, a presumably meaningful statement despite the absence of a control group. Unfortunately, also,



Wittner depends heavily on Cantril and Gallup's *Public Opinion, 1935-1946*, which in turn rests lamentably but exclusively on single question polls rather than on batteries or scales. (Cf. Quinn McNemar's "Opinion and Attitude Methodology," *Psychological Bulletin*, 1945, for shortcomings of this procedure.)

Wittner's data derive also from popular and esoteric books and magazines, pacifists' letters in the Swarthmore College Peace Collection, as well as interviews with prominent pacifists. Given the substantiation's divergence from sociological norms of adequate evidence, Wittner's more general statements, inferences, and conclusions—low-level and unassuming as they are—leave one wondering: Plausible, but is it true? Two votes from *Harper's* and one from Dwight Macdonald's *Politics* usually do not convince me. Nonetheless, with data other than "statistical" references (as mentioned above and, for reasons of courtesy, best not mentioned again) and other than matters requiring some speculative, generalizing ability or logical bent, Wittner is more in his element, as he is in dealing with documents (e.g., the Swarthmore set) not readily available to the general public. Unfortunately, even men as brilliant and otherwise creative as the pacifists quoted have little new to say on a subject so well-chewed.

Leaving aside control groups, attitudinal scales, sampling procedures and other methodological niceties which we might justly forgive a historian for ignoring, we nevertheless encounter Wittner's weakness *vis-à-vis* logic even in an uncomplicated section involving just plain, garden-variety valid argument. Wittner asks whether pacifism is a practicable, politically realistic position, and, to answer this, lists thirteen pacifist political positions. Without a nod toward the practical consequences of any of the positions listed, he nonchalantly concludes that the peace movement often shows "considerable political astuteness." Unaided by even a fundamental grasp of logic, "historical objectivity" at times deteriorates into elegantly elaborated vacuousness. (Cf. pp. 183-185, where three successive topic sentences more or less contradict one another and a three-page discussion's most accurately detailed possible summary is, "Opinions vary, don't they?").

*Rebels Against War* is a collection of "facts": quotations, figures, "statistics," single-question responses, events, all chronologically arranged and not readily transformable into rigorously specified but tenable generalizations. Nor, in general, are stimulating intellectual problems posed by the material or its treatment.

While it may be pedantic to mention pedantry, the book's footnotes are exceptionally copious but ill-arranged so that they hinder

rather than facilitate exploration of data's sources. In this regard, the Modern Language Association of America comments: "The conventions of documentation are largely a means to an end—enabling the reader to check you with ease—and any practice which ignores this end may result in pedantry" (*The MLA Style Sheet*, revised edition, 1951 p. 10). Similarly, *Rebels'* 42-page bibliography overwhelms you with its 42 pages but is less than maximally useful because unannotated.

*The Elusive Revolution: Anatomy of a Student Revolt*, by RAYMOND ARON. Translated by GORDON CLOUGH. New York: Praeger Publishers, 1969. 200 pp. \$6.95.

*The University and Revolution*, edited by GARY R. WEAVER and JAMES H. WEAVER. Englewood Cliffs, N. J.: Prentice-Hall, 1969. 180 pp. Clothbound, \$5.95. Paperbound (Spectrum), \$1.95.

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For those familiar with Aron's prolific and usually cautious scholarship, this little book will be a profound disappointment. Eighteen pages of preface and introduction are followed by a 132-page interview in which journalist Alain Duhammel puts the questions to Aron. The remainder of the book is filled out by the republishing of fourteen of Aron's articles which were originally printed in *Le Figaro* during May and June, 1968. Aron himself says that the book does not claim to be an "objective study" but is instead a "personal book, which is less concerned with the student troubles as such than with the national crisis of May to June, 1968 and with the enthusiasm shown by grown men who thought they were living, or re-living, a revolutionary epic" (p. xviii).

Aron's few moments of penetrating analysis are dominated by the book's angry reactionary tone. Page after page seethes with invective against the "groupuscules" or Trotskyite students, "pseudo-students," and other revolutionary youth; the "mad" faculty members who, after years of turning a deaf ear to pleas for university reform, suddenly discovered their own revolutionary yearnings and joined the students at the barricades; a "permissive society" weakened by parents treating children as equals; the "biased" "radical" press which ignored the "great many" "silent" faculty who shared Aron's views to spotlight the "anarchists"; and even an incompetent government administration which was hard when it should

have been soft and soft when it should have been hard. Sound familiar?

Aron can't quite make up his mind whether to be contemptuous of the non-violent and unsuccessful "psychodrama" masquerading as revolution or to be outraged at the collective blow which betrayed the "shocking indifference of the French people to the fate of their country," exposed "the fragility of the modern order," and jeopardized France's hard-won "prestige" (p. xv).

Aron describes himself as "one of the severest" critics of the archaic French university system (p. xiv). Nevertheless, fifteen years of relatively futile efforts to reform the system through conventional means were not enough to exhaust his loyalty and good manners. So, even though in sympathy with the students' and teachers' "legitimate grievances" and angered at "General de Gaulle's arrogance," the "violence of his [de Gaulle's] divided opponents" filled Aron with such "physical revulsion" that he rallied to his President's side during the crisis (p. xv).

In between attacks Aron constructs some interesting, but teasingly cursory analyses of certain problems of de-personalization, association, participation, and decision making which are peculiar to "consumer" or "techno-bureaucratic" societies. He lauds the material abundance created by the consumer society while regretting its lack of wisdom and spiritual direction. He sharply criticizes the highly stratified university system which prevents any meaningful exchange between students and faculty, but deplores the "Maoist type revolutionary university" in which teachers and students debated goals and strategies in plenary assemblies (p. 171).

Aron often refers to Toqueville's description of the abortive French Revolution of 1848 as his model of analysis for the "events of May." Another metaphor to which he alludes is that of the university as a factory, processing human commodities for the techno-bureaucratic society. This metaphor is featured more explicitly in the book edited by the Weavers.

*The University and Revolution* features twelve essays from a wide range of contributors. The book grew out of an independent research course initiated at the American University in the fall of 1968. The course was conceived by American University professors Gary Weaver and James Weaver in response to concerns aroused by a campus pot bust in which the police were aided by the administration. Some 250 students were divided into 17 sections, each moderated by a faculty volunteer. The editors state, "The broad purpose of the course was

to assess the impact of the university on the revolutionary movements sweeping the world today, as well as the impact of the revolutionary movements on the university" (p. 165). During the year twenty-five guest speakers accepted invitations to speak on a variety of related issues. The book apparently represents a selected cross-section of those addresses. In addition to the essays, there is a seven-page Selected Bibliography which would be especially useful to anyone planning a similar course at his university as the editors encourage.

The articles by Gary Weaver, Seymour Lipset, and Jack Newfield present a comprehensive and informative summary of much of the major sociological, psychological, and political thinking on student radicals. Samuel Sharp and James Weaver contrast typical images and expectations of the university [Sharp: "combination babysitter, chastity-watcher, matchmaker, and provider of glamorous jobs" (p. 56); Weaver: "the modern equivalent of the nineteenth century factory" training workers for the more complex but still boring tasks of the new technology (p. 60)] with the university's ideal objectives [Sharp: "a provider of critical knowledge" (p. 58); Weaver: the cultural "liberation" of the mind (p. 64)]

Robert Theobald issues a penetrating indictment of the present university system and proposes seven basic changes designed to bring a new level of learning into being. Most of the changes involve replacing arbitrary administrative criteria (for determining course formats and content, student body composition and evaluation, and university relations with the surrounding community) with a criteria based on what we now know about student needs and the learning process.

The most controversial articles are by radicals Mark Rudd, Ewart Brown, and Eldridge Cleaver. Rudd reviews the Columbia University revolt and explicates the students' position. He claims that the country is run by a ruling elite with the active complicity of the university in forms ranging from weapons development to area-studies research. He calls for a "mass democratic socialist movement" which "can be defined in terms of being against racism, being for national liberation in the third world, being against the system of capital and private profit" (p. 138).

Brown proposes the establishment of Black universities to rebuild Black identities out of Negro identities and to serve the needs of the Black community. Cleaver fluctuates between angry epithets and the call for proletariat revolution and a plea to the "power structure" to "start doing their job" and give Black people

their fair share of American wealth and power (p. 158).

This book provides an interesting panorama of points of view, but sociologists conversant with the basic issues will find little new. Insofar as the authors address themselves to different segments or manifestations of the whole malaise of problems which beset all young people, students, academicians, universities as institutions, poor people, Black people, and America in general, the book fails to define or clarify the fundamental sociological issues and their inter-relationships. Nevertheless, the concept of the course deserves vigorous applause; and books such as this can serve as provocative jumping-off points for class discussion.

*The Educational and Vocational Development of College Students*, by ALEXANDER W. ASTIN and ROBERT J. PANOS. Washington, D.C.: American Council on Education, 1969. 211 pp. \$6.00.

E. JACKSON BAUR  
*University of Kansas*

"(S)tudent achievement is little affected by the characteristics of the college environment, including those characteristics traditionally associated with institutional quality or 'excellence'" (p. 83). This is one of the conclusions contrary to the prevailing folklore of higher education reached by Astin and Panos in the report of their survey of students for the American Council on Education.

The ACE survey is the most extensive yet carried out on college students. Information on 127,000 freshmen in 246 U.S. institutions was obtained in the fall of 1961. Follow-up studies of over 30,000 of these students were made in the summers of 1962 and 1965. An accurate assessment of institutional effects was made possible by the longitudinal design. Data collected from the students by the three questionnaires were supplemented by scores on the Area tests of the Graduate Record Examinations and the National Merit Scholarship Qualifying Test. Institutional data were obtained from both students and publications including the Cartter report.

The analysis was planned "first, to measure changes in the students during their undergraduate years . . . and, second, to identify characteristics of the institution and of the students' educational experiences that may have contributed to these changes" (p. 13). Valid comparisons

of institutional effects must take account of differences in the kinds of students who enroll. Astin and Panos accomplished this by statistically equating students entering different institutions in terms of their input characteristics before correlating the adjusted output characteristics with the characteristics of the institutions.

Aspects of educational development considered in this report include student attrition, graduation, aspiration for a higher degree, achievement as measured by tests, choices of career, and undergraduate major field. Some of the findings especially interesting to sociologists are (1) that the dropout rate is low in selective institutions where the peer environment is cohesive and the number of employed students is low, (2) that marriage increases the dropout rate, (3) that the strongest environmental influences on choice of major and career came from peers, and (4) that career choices in the social sciences were unstable and showed a large net gain during the undergraduate years.

Some of the findings suggest that current trends in higher education are having consequences which many educators will consider undesirable. The increasing proportion of students enrolled in universities and the lower proportion in liberal arts colleges will tend to increase dropouts and lower aspiration for graduate education. Dropouts will also tend to rise with the increase in public institutions and the disappearance of non-coeducational schools. The tendency to convert technological schools and teachers colleges into more generalized and heterogeneous institutions will reduce the number of potential engineers and teachers. On the other hand, the findings imply that some trends within universities will accord with educational policy such as the development of honors programs and research participation and the movement to establish residential colleges, semi-autonomous subcolleges, and various clustering schemes.

The methodology of the data collection and analysis is impressively sophisticated. Ingenious techniques were used to maximize returns to the follow-up questionnaires. Non-response bias was reduced by an elaborate weighting system. The statistical design for testing the general hypotheses was a three-stage stepwise linear regression analysis. One possible weakness lay in an area remote from control by the directors of the research. Although they report an 81% response rate by institutions to the initial questionnaire, they say nothing about the rates of response within educational institutions nor any word about how the questionnaires were administered on the campuses. The reader is

hardly reassured as to the validity of the questionnaire data by the dubious assertion that, "Our starting point is the assumption that the student is willing to cooperate: that is, to be sincere and honest" (p. 22).

This study is an empirical work guided by administrative and policy questions and is not intended as a sociological study in the sense of having been designed for the purpose of developing or testing theories of social behavior. Nevertheless, some of its findings such as those concerned with cohesiveness and peer influences have sociological relevance.

*Cliques, Crowds and Gangs: Group Life of Sydney Adolescents*, by D. C. DUNPHY. Melbourne, Australia: F. W. Cheshire (Distributed by Lawrence Verry, Mystic, Conn.); 1969. 170 pp. \$7.50.

WARREN D. TENHOUTEN  
*University of California, Los Angeles*

This participant-observation study of informal adolescent groups was carried out in 1959-60 in Sydney, Australia, where the author went to youth clubs and contacted youth through "dancing, games, and general banter." He was then able to join groups in "milkbars and coffee lounges, on street corners, and in homes." Four short questionnaires were administered to some 300 youths, who showed little consensus on who belonged to their cliques as they identified persons in higher status reference groups as well as persons in their membership groups. However, the author states that participant-observation and respondents' diaries enabled him to identify 44 cliques within 12 crowds and exactly one leader within each clique.

The study was intended to evaluate a Parsonian model of peer group socialization. Dunphy hypothesized that the primary drive in adolescence is toward attainment of an "advanced heterosexual role" which is learned via the peer group. Pre-adolescent unisexual cliques combine through their leaders' initiatives to form a larger crowd containing both male and female cliques. Within the crowd the cliques change composition, become heterosexual, and are connected by an elite clique consisting of the clique leaders. This new crowd structure has one instrumental leader and one sociocenter. The instrumental leader socializes group members to higher levels of heterosexual role performance; the sociocenter reduces tension created by identification with the instrumental leader.

Numerous studies (e.g. Sherif and Sherif's

*Reference Groups*) have found leadership, status, and membership in adolescent cliques depending on competence in the current central activity of the group. Dunphy invokes Freud in asserting that sex-role learning is the organizing principle of peer groups. There may be severe sampling biases resulting from using youth clubs as the data source; for the clubs are designed to facilitate just such activities. Moreover, leadership status, performance of heterosexual roles, tension within crowds, and other variables are not consistently defined or measured. Consequently, the model is only partially illustrated by the study. For example, if leadership results from superior sex role performance, leadership status and sex-role performance should be positively associated within crowds. The author suggests that leaders are "skillful" in these roles because they "went steady" more often and dated more frequently." Since every clique has just one leader, it follows that in all 44 cliques the leaders date more frequently and go steady more than do non-leaders. Such claims are illustrated rather than examined.

The youth in the cliques face the challenge of leadership in dramatic ways: Will putting snails down girls' backs vault one boys' clique beyond latency? Will "Prick" recover from his wounds incurred playing "chicken" with automobiles in time to save the leaderless Rockers? Will Brian's "lesson" in "advanced heterosexual behavior" enhance Barb's status in the Cools? These questions and many more are answered in this study.

The middle-class Australian youth in the sample seem immature compared to middle-class American and European youth. Their description resembles that of English youth and American youth in the 1950's, i.e., no involvement with education, politics, or community. A replication might show today's Australian youth less preoccupied with dating and more involved in external social forces. Perhaps the most striking finding is that clique membership was prerequisite to crowd membership.

*The Feminized Male: Classrooms, White Collars and the Decline of Manliness*, by PATRICIA CAYO SEXTON. New York: Random House, 1969. 240 pp. \$6.95.

T. NEAL GARLAND  
*University of Akron*

When so much energy and ink has been expended upon only one side of a particular topic,

it is a welcome change to discover a book which deals with the "other side" of the issue—in this case, the male side of the sex-role controversy. *The Feminized Male* represents a needed addition to a neglected area of sociological research in spite of the fact that this reviewer finds much about the book which weakens its basic position.

*The Feminized Male* is a curious combination of editorial essay, unsubstantiated assertions, empirical research findings, and personal opinions of the author. The basic theme is that American men are being "feminized" through the influences of the major institutions of our society, notably the family, the school, and the occupational. At the root of the feminized male's problems are women. Denied the power to give orders on the job, women concentrate on dominating those they do have power over: husbands and sons at home and children in school. Sexton's thesis is not merely a restatement of the "Momism" theme, however; for she also attacks the white-collar society which "frequently demands unmanly amounts of submission and inactivity" (pp. 17-18).

When the author attempts to define what it means to be "masculine," her conclusion is that "It means, obviously, holding male values and following male behavior norms" (p. 15). These norms "stress values such as courage, inner direction, certain forms of aggression, autonomy, mastery, technological skill, group solidarity, adventure, and a considerable amount of toughness in mind and body" (p. 15). The concept of masculinity, for her, also appears to include a considerable component of nostalgia, and herein lies one of the weaknesses of her argument. Scattered throughout the book are comments noting that it is essential to avoid the stereotyping of sex roles which has placed each sex in its respective straight-jacket in the past and that society must learn to respect each person for his individual talents rather than judging him on the basis of how well he or she fits a standard sex role image. Yet she laments that "On the farm, males can more easily be men. The farm boy does useful and manly work at the side of his father. . . . He is close to nature and the ways of men" (p. 35). Such statements are typical of the author's view of masculinity, which leans heavily toward the blue-collar, outdoors type male; and she is vicious in her overt and covert disdain for males who do not fit the hairy-chested cowboy image of masculinity, even though she notes that such an image is incongruous with today's society. Such contradictions are typical of the author's reasoning. What she really seems to be saying is that *women* should not be forced to fit the

standard feminine stereotype of the past but that men must retain the characteristics of masculinity appropriate to a frontier America of 100 years ago. She is upset because men do not fit the masculine stereotype of yesterday but deals very inadequately with the larger question of the appropriateness of that stereotype for today's world.

The sociological research upon which this book is based consisted of a study of school children in a small city ("Urbantown") located close to an urban center. Analysis centers on 9th graders who were given a "lengthy questionnaire" and a masculinity-femininity scale. Those desiring a detailed statistical analysis are referred to the original NIMH report since the book is aimed at a non-scientific audience and the presentation of research results geared accordingly. Nevertheless, the presentation is disappointingly weak. Urbantown is said to be one-third Negro, yet race is not utilized as a variable in the analysis. Also omitted are religion (Urbantown is one-third Jewish), SES, and, indeed, all standard sociological variables with the sole exception of sex. Since both Negro and Jewish subcultures supposedly have strong matriarchal components, omission of such variables looks like either very poor sociology or very good propaganda in light of the author's central thesis. The findings themselves contain few surprises. Boys who score low on the masculinity scale show less interest in sports and do better scholastically.

In the last two chapters Sexton attempts to suggest solutions to the problems which lead to the feminization of males. Included are vast reforms in the schools and full integration of women into every level of the business, economic, and political worlds. Such reasoning sounds vaguely like that of the suffragettes at the turn of the century who were claiming that, if women could vote, the feminine influence would end all wars, crime, and corruption. Russia is noted as a society in which the feminization of males has been minimal, presumably because of the more complete integration of both men and women into a broader spectrum of society than the United States has achieved.

*The Feminized Male*, despite its shortcomings, will prove valuable to those interested in the topic of sex roles in modern society if for no other reason than that they will find something on every page—and perhaps in every paragraph—with which to disagree violently. No major theoretical issues are dealt with and no surprising findings are presented; but if the author's intent in writing the book is to raise questions and stir the minds of her readers, she has succeeded admirably.

*The New Female Sexuality*, by MANFRED F. DEMARTINO. New York: The Julian Press, 1969. 236 pp. \$7.50.

ALBERT D. KLASSEN, JR.  
*Indiana University*

Given the Janus-faced character of sex research, this book is both an anachronism and a near epitome: for those bringing to sex research the careful application of the most recent methods, this book is anachronistic in its lack of proper application of current research techniques and standards. But the book comes distressingly close to being an epitome of the other face of "sex research," which carelessly produces any speculation the author chooses while giving a nod to any data he sees fit to introduce.

The survey reported here uses one aggregate of 102 female "social nudists" and another of 73 female "potential nudists" who were persuaded to respond anonymously to an eight-page open-ended response questionnaire. "Social nudist" refers to those 102 mostly "liberated" females recruited over a three-year period through their involvement with organized nudism—reportedly somewhat rare in the nudist world of predominantly asexual (or "anti-sexual") orientations (pp. 3-7, 10-11).

Unfortunately, the recruitment determinants of the "potential nudists" are far from clear; recruitment seems to have been mainly by access to college students, the Mensa organization (members limited to 98th and 99th I.Q. percentiles), and the Daughters of Bilitis (lesbians). Aside from "access" determinants, inclusion was determined by a willingness to respond to the survey and responses that they "would . . . , [or] might like to . . . , or were undecided about . . . becoming a member of a nudist group" (p. 11).

One may contrive a rationale for the study, ruminating beyond a screen of ambiguous suggestions (in the Albert Ellis "Foreword," the "Preface," and Chapters 1 and 2) about what populations the "samples" represent (the American female, the female social nudist, etc. While the term "sample" is never relinquished, any notion of what is represented is later abandoned; see pp. 218-219). At rock bottom, the rationale might be: (1) modern society is now approaching the Age of Nudity and sexual liberation, especially on the part of females (pp. 8-9); (2) one may glimpse what sexual liberation will be like by considering the self-reported sexual orientations and experiences of sexually liberated nudist and potential nudist females (pp. x-xi); and (3) supposedly, these survey data are very encouraging in showing a great variety and freedom of sexual expression to be antici-

pated as these developments proceed to their prophesied culmination (p. 2).

The first chapter is a respectable sketch of organized nudism in the United States; the second is an inadequate description of the survey, including distributions of the "nudist" and "potential nudist" cases on several social variables never used in later analysis, and two personality measures—Maslow's "dominance (self-esteem)" and "security-insecurity" scales.

The 23 chapters, 3 through 25 (Chapter 26 is the final summary), follow a routine pattern: they include 34 sections, each dealing with a facet of sexuality (e.g., masturbation, sexual techniques, extramarital and group sex, incest, sex with animals). Each section begins with a discussion averaging one and one-half (maximum of three and one-half) pages, usually comparing percentages of both groups having a given kind of experience with Kinsey's published percentages, and reporting phi correlations between experience and "self-esteem" score, followed by four or five pages of verbatim quotes of open-ended responses, ending with no further comment. There is some evidence of categorization of quotes (in the discussion) but generally nothing explicit on how such classification was done. The only organization of these quotes consists of keeping separate the responses of three groups: nudists, potential nudists, and the lesbian subgroup of potential nudists. But for the possibility that this mass of quotations (a total of about 150 pages) might someday be of socio-linguistic value in a more adequate study of female sexuality, one might suspect that this extensive quoting serves either as filler or to generate a wider market through titillation.

Throughout the book no provision is made for the reader to verify the conclusions presented. We are told that for all the phi coefficients (a statistical equivalent of the Pearsonian product moment correlation between two dichotomized variables) calculation is based on cases in the high and low categories, *excluding* the middle one-third of both groups (p. 18). We are never told the number of lesbians among the 73 "potential nudists," but presumably there are about 10 (or more) which are included in the percentages and correlations for potential nudists and at least two among the nudists. One might assume that the tests of significance are based on a chi square derived from the phi ( $\chi^2 = N\phi^2$ ); in the absence of an "N" for each phi, and knowing that the middle one-third of both groups is omitted, this cannot be checked. Finally one cannot check the content of the responses against the generalizations and percentages reported; although total  $N = 175$ , the highest number of quotes found is 133, and the next highest is less than 100 (see Ch. 19

and first section of Ch. 4, respectively; responses were not counted in all 34 sections).

While the answers to such basic questions of method might undercut nearly all the study's findings, the major findings are (1) in reporting all kinds of sexual experience, both actual and potential nudists in general yield much higher percentages of ever having that experience than the percentages yielded by Kinsey data; and (2) having had a particular kind of sexual experience generally correlates positively with "dominance (self-esteem)" score; however, the only report of a correlation with "security" score indicates it is positively related to subjective report of "above average sex drive."

This book purports to describe a survey research effort, but it stands on the threshold of—if not a little below—what is currently accepted as survey research. Amid claims of "scientific method" we are offered a piece of research in which the "homework" has not been done and naive methods of presentation prevent checking the soundness of what little has been done. Nevertheless, this is an excellent book for the bedside table—the male or female side of the bed, depending on which is the more dominant.

*Sexuality and Man*, edited by the SEX INFORMATION AND EDUCATION COUNCIL OF THE UNITED STATES. New York: Charles Scribner's Sons, 1970. 239 pp. \$6.95.

CARLFRED B. BRODERICK  
*Pennsylvania State University*

There is a great deal of useful information between the reinforced cardboard covers of this volume. SIECUS here reprints the first eleven Study Guides which have been prepared under its auspices. They are aimed at a general adult audience and include responsible summaries of current professional opinion on various aspects of human sexuality.

It begins with a chapter by Kirkendall and Rubin which summarizes sex over the life cycle. Together with Chapter 9 ("Sexual Life in the Later Years," also by Rubin) this chapter does a good job of putting sex in a life time perspective without falling into the Freudian trap of pansexuality nor the more conventional one of seeing sex as the special preserve of the teenager and youngish adult.

Four of the most useful chapters are those dealing with what might be viewed as "problem areas": sex relations during pregnancy and the post-delivery period, masturbation, homosexuality, and sexual encounters between adults and children. Each covers its subject matter with

humanity and competence, challenging commonly held myths and suggesting practical guidelines for dealing with problems in these areas. These chapters and the three on sexual values—"Premarital Sexual Standards" (Ira Reiss); "Sex, Science, and Values" (Harold Christensen); and "The Sex Educators and Moral Values" (Isadore Rubin)—are written from the value stance of the impartial sociological analyst. Thus, nothing in them will raise an eyebrow among the readers of this journal. It has become increasingly obvious, however, that among the general public, for whom these materials were prepared, these essays will be criticized by some for not unequivocally supporting conventional ethics and by others for not unequivocally repudiating conventional morality.

The book also includes a history by Kirkendall of the sex education movement which unhappily was written just before the Great Sex Education Controversy of the last two or three years erupted. This chapter is supplemented by an appendix of annotated films for use by sex educators.

Most sociologists would not need this book for their professional libraries, but many will find that it makes rewarding and sensible reading in an area of perennial interest.

*Extramartial Relations*, edited by GERHARD NEUBECK. Englewood Cliffs, N. J.: Prentice-Hall, 1969. 205 pp. Clothbound, \$5.95. Paperbound (Spectrum), \$2.45.

PHILIP J. ALLEN  
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The central question in this book to which all contributors address themselves is, put at its best, "Can we learn to understand that it is possible to love more than one person, to be loved by more than one person?" (p. 198). Put at its worst, the question is: "Can adultery be good?"

The majority of contributors answer "Yes," the minority "No." The emphatic "Yes" comes from Albert Ellis and John Cuber, as well as from Kinsey's co-worker, Wardell Pomeroy, who participated with Neubeck, Jessie Bernard, and others in a "seminar on adulterous sexual behavior" at the 1966 Annual Groves Conference on Marriage and the Family. Said Pomeroy of a wife-swapping group, "as far as I could determine, these people, from other criteria, had a good marriage, in the sense of responsibility, raising children, having good jobs, being happy, etc." (p. 34).

Stephen Beltz, on the other hand, concludes from his five-year clinical study of five couples

engaging in wife-swapping behavior that his "evidence is overwhelmingly in the opposite direction" (p. 187) since "three had ended in divorce and one additional divorce was not unlikely" (p. 186).

William Graham Cole contributes an excellent summary on sex in the Bible; Kinsey's relevant findings are summarized by Yoon Hough Kim; empirical material on attitudes of Greek spouses toward marital infidelity is presented by Constantina S. Rothschild; Hyman Rodman writes on consensual unions socially accepted in Trinidad; Robert Whitehurst reports on extramarital sex and alienation; Albert Ellis lists "some of the main healthy and disturbed reasons for extramarital unions" and, as expected, argues for the validity of certain "standards of healthy adulterous behavior" (p. 160). John Cuber repeats arguments based upon the well-known Cuber-Harroff findings that affirm how "significant Americans" do it and get away with it.

It's strange that, in a book whose focus is upon extramonogamic sex, the Oneida experiment goes unmentioned, even in "cross-cultural" and other reporting. Those who think "wife-swapping" is new may be surprised to learn that by 1846 John Humphrey Noyes had years earlier persuaded his mother, his wife, his brother and wife, his two sisters and husbands, plus other males and females from New York to join him in his "wife-swapping" Utopian venture. His was an honest experiment in communitarian living, based upon the religiously buttressed belief that all mankind's troubles issue from jealousy over private property and private mates. Share and communize both, said he, and "Perfectionism" will be realized. There isn't room, here, to present the entire argument of Noyes for non-monogamous sex union, made brilliantly, religiously, and persuasively. He made a stronger case for it than any of the arguments found in this book.

Alas, the ideal visualized from the arm-chair is often impossible fully to implement with actions of mortal men and women! While Noyes persuaded over 200 to join him at Oneida and elsewhere, the experiment, though seemingly successful for a time, finally foundered and collapsed from growing internal discord over legitimacy of rules governing non-monogamic sex union. (Incidentally, several communitarian experiments now in progress should provide excellent empirical material for long-range research that might answer the central question in this book. A foundation should generously support a well-designed, longitudinal project that could yield a mine of information.)

So far no one has amassed the necessary empirical knowledge to claim the authority to

prescribe or proscribe a particular kind of sex behavior for another. The best possible at this stage may be to continue to accumulate knowledge that yields firm generalizations that permit terse propositions of the "If this . . . , then that . . ." variety, knowledge that would permit us to say with confidence: If spouse A cohabits with non-spouse B, psychic-emotional investments of A in non-spouse B will start erecting a wall between A and his spouse, which may subsequently reduce the degree of *total* gratification (sexual, emotional, sentimental, etc.) that A can obtain in interaction (sexual and other) with his spouse. Mutual exclusiveness seems to be required for the optimum commitment that yields the maximum gratification basic to the self-renewal possible through total fulfillment (Fletcher's "situation ethics" notwithstanding).

*Comparative Studies in Kinship*, by JACK GOODY.  
Stanford, Calif.: Stanford University Press,  
1969. 261 pp. \$7.50.

JOHN A. BALLWEG  
*Virginia Polytechnic Institute*

This book attempts to do more than present comparative kinship studies. It involves an effort to develop a scheme for the fusion of anthropology and sociology into a single discipline of comparative sociology. While it is difficult to fault the objective, the attempt fails.

In Chapter One ("Comparative Sociology and the Decolonization of the Social Sciences") Goody attempts to describe a form of comparative sociology that includes elements of both sociology and anthropology. Subsequent chapters are not directly concerned with the attempted merger but purport to show how it works. Relying heavily on his African field work, Goody discusses "A Comparative Approach to Incest and Adultery," "The Mother's Brother and Sister's Son in West Africa," "A Classification of Double Descent Systems," "Inheritance, Social Change and Boundary Problems," "Marriage Policy and Incorporation in Northern Ghana," "Indo-European Kinship," and "On Nannas and Nannies." Two additional chapters, co-authored with E. N. Goody, deal with "The Circulation of Women and Children in Northern Ghana" and "Cross-Cousin Marriage in Northern Ghana." In most instances, the chapters amount to an updating of reports presented over the past fifteen years.

The author appears confident that the reader will recognize the fulfillment of his anthropology-sociology merger thesis in the ethnographic accounts as they are presented. No attempt is made to demonstrate the existence



of such a merger as the accounts unfold, and no summary statements are presented. It is true that the text presents a more sophisticated effort at bringing together comparable ethnographic material; this represents a forward step in presentation for social anthropology but fails to reflect other techniques currently in use by sociologists.

The plight of the field anthropologist is clearly evident in Goody's statement that "throughout the anthropological world, the supply of old men is running perilously low; without their memories, primitive societies, in the strict sense, would no longer exist" (p. 10). Thus, he proposes three fields of comparative sociology: (1) the sociology of developed societies, (2) the sociology of developing nations, and (3) the sociology of simpler societies. For the first two he proposes both field and historical research, while the third would become a branch of the historical sociology of pre-industrial societies. Goody appears hard pressed to find contemporary examples of his technique and suggests that the intensive field work of William F. Whyte's *Street Corner Society* (1943) deserves wider use. What Goody fails to recognize is the change in focus for the discipline of sociology over the past generations; social psychological dimensions have been added to the more traditional consideration of structure and function. Similarly, historical generalizations in sociology have been displaced as more sophisticated methodological techniques develop.

While the stated goal of the book is not achieved it is a useful review of selected subject matter and could easily serve as supplementary material for courses in comparative institutions.

*Black Carib Household Structure: A Study of Migration and Modernization*, by NANCIE L. SOLIEN GONZALEZ. Seattle, Wash.: University of Washington Press, 1969. 163 pp. \$7.50.

KEITH F. OTTERBEIN

State University of New York at Buffalo

The monograph both describes the composition of Black Carib households in Livingston, Guatemala, and develops a hypothesis that purports to explain the particular type of households which characterize that community. The Black Carib, a hybrid population of Caribbean Negroes and American Indians known as Caribs, inhabit the east coast of British Honduras and Guatemala. Eighteen hundred Black Caribs reside in the community in which the anthropologist carried out her field work in 1956-1957. Of the 362 households in her census, 45% are "consanguineal households"; the remaining 55% are "affinal households." A con-

sanguineal household is "a coresidential group of people who live under one roof, who eat and sleep together, and cooperate daily for the common benefit of all, and among whom there exist no conjugal pairs." Affinal households contain conjugal pairs; that is, a cohabiting man and woman. The occurrence of a substantial number of consanguineal households is explained in terms of migratory wage labor and male absenteeism.

The hypothesis developed in the case study is tested by examining similar communities in Africa, Oceania, North America (including both Negro and American Indian groups), and the Caribbean. From her cross-cultural comparisons Gonzalez concludes that there are three factors "which operate to maintain the consanguineal system through time": "(1) Migrant wage labor. (2) A 'neoteric' quality to the society. (3) An imbalance in the sex ratio, resulting in an excess of adult females over males." *Neoteric* means that the society has been "placed in the position of having to adapt to an economy dependent upon industrialization through the mechanism of migrant wage labor, while being denied full admission to the industrial society as a whole."

As pointed out in the Foreword by David Aberle and in the Preface by the author, this monograph is a revision of her doctoral dissertation. Since the book has been available in manuscript form for ten years, an unusual situation has arisen; namely, both the author herself and several social scientists have utilized the data and the hypothesis in research which has already been published. Although this research—cited by Gonzalez in the published revision—has tended to confirm her original hypothesis, the reader is not made aware of the impact which the hypothesis has had upon studies of New World Negro family organization nor is he apprised of the current status of the hypothesis.

*Family and Kinship in Modern Britain*, by CHRISTOPHER TURNER. New York: Humanities Press, 1969. 118 pp. Clothbound, \$2.50. Paperbound, \$1.25.

*The Family in the Caribbean: Proceedings of the First Conference on the Family in the Caribbean*, edited by STANFORD N. GERBER. Rio Piedras, P. R.: University of Puerto Rico Institute of Caribbean Studies, 1968. 147 pp. Paperbound. \$4.00.

HELEN M. HACKER

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The two volumes under review have little in common except "Family" in their titles and

their modest length. The Turner book, combines three purposes: (1) a simply written introduction to the field; (2) a review of the available literature; and (3) the provision of an idiosyncratic conceptual framework for the organization of congeries of empirical data. The second aim is most satisfactorily realized, both in the text itself and in the suggestions for further reading. The writing at times seems only deceptively clear, and rereading is frequently required to fix the terms of an argument. This problem arises in part because much of the substantive material appears to be related to the conceptual framework more by a process of free association than by theoretical necessity.

Turner's eclectic conceptual approach represents a fusion of the developmental with the institutional and structural functional as exemplified in his two key concepts. First, the analysis of "kinship positions as ego-centered networks of kindred and affines, and as sets of interlocking household and nuclear family units" lays the structuralist foundation for charting changes through time in kin universes and effective kin networks of households. Second, the concept of the developmental cycle of the domestic group provides structural underpinning for tracing both normative and deviant cycles of development of the household which contains only nuclear family members as a special case of domestic group. This rapprochement between the social-psychological and social-anthropological emphases often adds a new explanatory dimension to familiar phenomena, as witness the discussion of the ideology of romantic love as implementing the general norm of marital stability within a monogamous system, or the relating of variations in the style of marital relationships to the structure of kinship and wider social networks along the lines first suggested by Bott.

Indeed, the most important contribution of this introduction is found in the restatement and refining of problems and in indications of where the frontiers of knowledge most urgently need to be pushed back—such questions as: What interplay of factors underlies the processes of selection of effective kin? For whom and under what circumstances are kinship positions relevant for non-kinship positions? What is the number of empty shell families and at which phase does this structure develop? Turner calls for systematic, including longitudinal, data collection to bear upon these and other significant problems.

The ten papers included in the Proceedings of the First Conference on the Family in the Caribbean vary widely in focus, generality, originality, and interest. Sidney Greenfield

("Culture-Historical and Structural-Functional Orientations and the Analysis of the West Indian Family") urges an examination of the differential histories of the islands, including their ties to Europe, to overcome the previous Redfieldian isolation of the folk—with common characteristics largely African in provenance—from the elite and to find a viable alternative to viewing the folk family as disorganized. Morris Freilich ("Sex, Secrets, and Systems") shows how the fiction of secrecy maintains a sexual system in which men and women have opposed goals—that of the Negro peasants of Anamat. Norman Ashcraft ("Some Aspects of Domestic Organization in British Honduras") compares the pattern of conjugal relationships in urban and rural areas in terms of economic opportunities, especially for women. June Nash ("Family Authority and Community Power Among Contemporary Maya Indians") links variations in kinship systems to settlement patterns and size of landholdings and finds that, while unilinear ties, patrilineal inheritance, and patrilocal residence are adaptive in solving problems of internal local control and evasion of dominance by non-Indian authorities in the village of Oxchuk, relative age and residence in a territorially defined group are more important in structuring social relations in Amatenango.

In her intensive study of twenty Puerto Rican students, Ursula von Eckhardt challenges some North American ethnocentric assumptions of what hangs together in functional analysis. Chaperonage in Puerto Rico implies neither sexual segregation nor very close supervision of unmarried girls. Rather it serves as a "way of mingling the generations" and "of giving social prestige." One interesting consequence of Puerto Rican dating patterns is that conflicts of values and striving for popularity devolve upon boys rather than girls. Closer to continental patterns are the separation of love, sex, and friendship, and the satisfaction of these needs with different persons.

Of the remaining contributions, two deal only marginally with the family: (Robert Brazelton, "The Caribbean: A Survey of Economic Problems and Policies"; C. J. Austermiller, "Labor Force Participation Rates in Various Societies"). One is historical, rather than analytic (Emilio Corfresi, "Birth Control in Puerto Rico"). Della Walker's "Family and Social Structure in Anguilla" describes the rather special situation of an island which can support its population only through migration; Ramon Fernandez Marina's "Psychological Functions of Puerto Rican Families" uses Puerto Rico as a convenient illustration for some general, largely Sullivanian propositions concerning personality development.

*Deviants*, by JERRY L. SIMMONS. Berkeley, Calif.: The Glendessary Press, 1969. 134 pp. Clothbound, \$5.00. Paperbound, \$1.95.

CYNTHIA S. KRUEGER  
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This addition to the literature on deviant behavior may be viewed with some merit as a primer in the mode of Dick and Jane. Perhaps one should overlook the author's apparent and complete disdain for such academic establishment hang-ups as footnotes, bibliography, or systematic review of previous contributions to the field.

Simmons was armed with several small studies; his central study involved 180 respondents varied in age, sex, education, religion, race, and locale. He asked people to "list those things or persons they regarded as deviant"; needless to say, that list proved to be too varied to be more than only vaguely meaningful. In addition, he was fortified by the desire to "tell it like it really is." As a result, *Deviants* displays a *potpourri* of thoughts, judgments, and sentiments concerning the stigma of deviance, labeling, and power differentials with respect to the deviant and straight "worlds."

It is possible that Simmons has fallen into the mind-paralyzing, but seductively Quixotian error, of trying to do too much. To presume to review the history of societies' attitudes towards deviants, to present a personalized account of various theoretical approaches to deviance, to "tell it like it really is" from the viewpoints of the deviants themselves—is ambitious for anybody and patently absurd in a slim volume of 134 pages; and the effort to accomplish all this is a total failure. In reviewing the historical reactions to deviance, he simply recites, rarely bothering with references, the old saws of deviants as evil, deviants as products of heredity, deviants as sick in the head, and deviants emerging as one consequence of social processes.

As for the different sociological theories, this was the most sketchy treatment I have seen. Although his theoretical underpinnings remain ambiguous, Simmons seems passionately linked to the "labeling" school of sociology. Indeed, one of the vulnerabilities of the labeling approach is quite apparent in this book: a tendency to differentiate *only* between the "good guys" *versus* the "bad guys," resulting in the pedestrian stance of the sociologist doing his own "labeling" in the guise of analysis. Whether or not "labeling" is yet a theory or ever can become one invites another work. The question of whether all forms of deviance should be included under the same theoretical umbrella (whatever that might be) would also require another in-

vestigation. Such questions do not appear to have hampered Simmons; but had he taken them seriously, his book might have been more valuable.

In "telling it like it really is," Simmons tends to underestimate the personal cost of a deviant pattern; for example, anxiety because of the fear of exposure; the dissonance of laughing publicly at the form of deviance one "really" is; the inescapable pain which is linked with the knowledge that you really "can never go home again"—try though you might, you'll never be so "straight" as those who have never been deviant.

*Deviants* informs us that society labels certain behaviors as deviant, a consequence of which is a certain amount of alienation from the society or, minimally, from the labelers of that society. Further, the powerful exercise their power to engage in a considerable amount of labeling. In this complicated process some people get hurt, some feel righteous, some make money. The sensitivities of humanitarians everywhere are abused (even at their own "safe" social and psychological distances), but the labeling continues.

Most professionals would agree that each of the above topics is worthy of serious investigation. Many would concur that a five million dollar grant from HEW might not be the most fruitful condition of research. Clearly at this point the author of *Deviants* might be said to have many fellow-travelers. However, *Deviants* reveals neither a free spirit, a moralistic Durkheim, nor an anguished Weber. Rather, Simmons appears to be quite satisfied with such observations as "The simplest and truest reason the person accepts recruitment into deviance is that he finds it rewarding" (p. 63); or "So deviance, like beauty, is in the eyes of the beholder" (p. 4).

Perhaps the most conclusive subtitle in this book is "*The deviant is a human being.*" That we ALL are human beings would appear to be a common predicament most individuals recognize—those whom many of us may call bastards as well as those of us who would prefer to be regarded as "hip intellectuals." Thus, that deviants are human beings does not strike this reader as a particularly novel observation; it may, however, document a lack of intellectual commitment to this area of study *qua* study.

It is difficult to recommend this book to any specific audience. *Deviants* assumes the readers are familiar with the writings of Lemert, Merton, Becker, Goffman and Gouldner. Thus, a potential high school audience is probably eliminated. It is my opinion that college students would be insulted by the constant reminders of facts of life they have already experienced.

This book gives one the impression that the author attempted to enter the emotional and social "Cave" of the deviant world and that he emerged much as he entered.

*Practice and Theory of Probation and Parole*, by DAVID DRESSLER. Second Edition. New York: Columbia University Press, 1969. 347 pp. \$10.00.

BADR-EL-DIN ALI  
*University of Louisville*

The present volume reflects a gradual elaboration and formalization of Dressler's previous writings and ideas on the subject, and attempts an updated "restatement of where we stand and what we are about in probation and parole." It deals basically with the same topics covered in the first edition, expanding from 252 to 347 pages, omitting the first edition's rather meagre and unnecessary chapter on "etiological factors in delinquency and crime," developing the underfed chapters (especially the one on "research on selection") that needed it most, and reorganizing and enriching the chapters dealing with help and supervision.

In sixteen chapters and an extensive nineteen-page index, designed as a textbook, the story of probation and parole is told in terms of history, technique, administration, and expectation. By briefly reviewing the penological schools of thought, the first chapter sets the stage for the next four chapters on the historical origin and evolution of probation and parole, both as treatment philosophies and operational procedures. Pre-investigation, selection, and prediction studies are discussed and analyzed at length in two chapters. Three chapters are devoted to "supervision in probation and parole" utilizing the three major techniques of social work, followed by a chapter "summing up the helping process." Rules and outcomes of probation and parole are then treated in two chapters; two others on "some administrative considerations" and "new directions" terminate the volume.

Throughout the book, Dressler writes with the orientation of a social worker rather than a social scientist. While he resorts occasionally to factual data and scientific research evidence to substantiate his hypotheses, his basic methodological device is that of case-histories, personal interviews, experimental projects, and illustrations, reinforced by an insightful, critical analysis and able command of the field practice. To him "probation and parole should be regarded as social work" and no wonder that almost one-fourth of his book deals with the

helping process involving casework, group work, community organization, and community-centered programs. However, the impact of psychology on his work is quite obvious, especially when he indicates his "eclectic orientation" in casework (chapter 6), and when he discusses "psychological-psychiatric therapy" as applied to the correctional field (chapter 12). His scattered observations relevant to social values, social systems, and socialization add a sociological flavor to his text.

Aside from a stimulating, lucid, independent, and sensible writing style, the author deserves credit for a valuable contribution to the penological body of knowledge. His special coverage of the historical trial-and-error development of probation and parole is quite comprehensive and informative. His well-organized discussion of the various techniques of helping probationers and parolees and his critical review of rules and regulations is very enlightening. He "knows how" and thus calls optimistically for various modifications and implementations within the administrative structure and rehabilitative function of probation and parole. His suggestions and remarks on correctional workers' "unity inherent in their social system," the public's "possession of the facts," and the offender's chance for "annulment of conviction" are among the many constructive and often well grounded proposals aiming toward the betterment of the field. In spite of his personal authoritative and operative approach he is often flexible and humble enough to take a middle position and/or emphasize the need for further research on several controversial issues and uncertain resolutions.

This volume suffers somewhat from the lack of an explanatory introduction and/or a summary. The sixteen chapters are filled with material that could be better communicated if they were reorganized into four or five major units. This way the rather thin and inconclusive chapter 6, "Probation and Parole: Companion Services," could be totally or in part incorporated elsewhere. In several chapters the author disperses his subtitles generously but inconsistently to head many pages at times and just a few lines at others.

Realizing that "we are far from having a truly professionalized field," Dressler falls rather willingly into several speculations and generalizations. In chapter 6, for instance, his argument that "probation and parole are basically similar in their rationale and objectives" is not well documented nor strongly convincing. However, he goes on treating probationers and parolees as being "much alike in background and attitudes" throughout the whole helping process. As a result of his obvious at-

tention to practice, the author leaves several theoretical and research problems without systematic investigation. But even in practice, he seeks to emphasize the positive constructive aspects and to overlook or rationalize some of the negative and unpleasant sides of the game. His provocative and enchanting ivory-tower therapies need to come down to the earth of reality.

This book is readable enough to interest practitioners and laymen as well. It is most valuable to social and correctional workers and should have a place in the personal library of probation and parole officers. It is recommended as a textbook for college courses on probation and parole and as additional reading for criminology and penology courses. While it is basically commensurate with the undergraduate level, it is informative and motivating enough to be useful to graduate students as well.

*The Police: Six Sociological Essays*, edited by  
DAVID J. BORDUA. New York: John Wiley  
and Sons, 1967. 258 pp. Clothbound, \$7.50.  
Paperbound, \$3.95.

ROBERT M. TERRY  
*University of Iowa*

The use of sociological concepts in analyzing police and the salience of the police as a research site are exemplified in the essays comprising this volume, some of the most profound sociological analyses of police produced thus far. Methodologically there is something for all but the most rigorous empiricist, although several of the essays may even overcome this deficiency.

Werthman and Piliavin in "Gang Members and the Police" reject the commonsense "detective model" of police behavior in favor of one in which "The police attempt to solve particular crimes either by going directly to a population of previously located suspects, or they first locate 'suspicious' individuals and then attempt to link them with some item in the set of previously committed crimes." This frame of reference leads to an analysis of police behavior wherein the detection of crime, development of suspicion, handling of encounters, and discretion used in the disposition of offenders create and foster feelings of injustice on the part of those suspected. The incompatibility of what is thought to be "good" efficient police work with the good will of disadvantaged segments of the community has never been more fully exposed.

John McNamara in "Uncertainties in Police

Work: The Relevance of Police Recruits' Backgrounds and Training" spells out in detail the uncertainties of police work. Using data on New York City police, he shows how the backgrounds of officers prepare or fail to prepare them to cope with these uncertainties. Longitudinal data are presented to demonstrate the discrepancies between the ideal practices fostered among recruits in the police academy and the orientations developed by patrolmen after undergoing field experience. Not only is this an excellent organizational study, but it also bears on a fundamental sociological concern: how the position makes the man.

Skolnick and Woodworth report a study of a morals detail of a California police department. Most informative is their discussion of the identification of males as statutory rapists. Arguing that social control is a function of the nature of bureaucratic organization as well as of access to information, the authors assess the roles of poverty, pregnancy, seriousness of the offense, and righteous indignation in the *construction* of statutory rapists. This study is an excellent illustration of the necessity to study the role of social control agencies in order to understand how offenders become criminalized.

Organizational concerns enter even more explicitly into Reiss and Bordua's analysis of "Environment and Organization: A Perspective on the Police." They are concerned especially with how the nature of the legal system, the nature of violative activity, and civil accountability affect the nature of the police organization. The orientation of the essay is that the police primarily adopt a reactive, rather than a proactive, strategy, making the police heavily dependent upon adequate community ties and the good will of the judiciary for enforcing the law or solving crimes. The low visibility of much violative behavior, coupled with reliance upon the citizenry, result in the police organizations being structured so that disproportionate resources are allocated to patrol, discretion becomes a virtual necessity, and adequate crime intelligence becomes essential. The nature of command processes are also analyzed in terms of environmental constraints.

James Wilson presents an analysis of the impact of reorganization of the Chicago police department upon police morale. Using longitudinal data, he demonstrates that, although departmental management became more favorably perceived by the men, morale was not appreciably affected. Data dealing with the perception of citizen respect and hostility account for the lack of morale change.

Allan Silver relies heavily upon historical data to demonstrate that riotous behavior formerly served to communicate specific

grievances to the elite. The spatial insulation of the elite from the masses and the advent of the modern police are among the conditions that have led to a restriction upon normative violence in recent decades. Rioters are now more likely to be seen as comprising a "dangerous class," and their behavior is more likely to be viewed by the elite as meaningless. This essay has great applicability to current issues of violence, dissent, and collective behavior.

While dealing with these essays in decreasing order of what I see as their sociological merit, others would surely rank them differently. Although the key contribution of this volume consists of adding to the understanding of police organization, it should not only be read by those interested in the police. There is much in it of significance for most sociologists.

*Crime in England and Wales*, by F. H. McCLEINTOCK and N. HOWARD AVISON. New York: Humanities Press, 1969. 317 pp. \$18.00.

JOHN H. MCNAMARA  
*Michigan State University*

Students of comparative criminology and criminal justice will be quite interested in this extensive descriptive study of "indictable crimes" in England and Wales, focused primarily on the period between 1955 and 1965. The mass of data presented for 1965 is fortunate for the American criminologist in view of the extensive amount of data on crime and criminal justice collated by the President's Commission on Law Enforcement and Administration of Justice for that year.

The data used are almost exclusively administrative and crime statistics collected by the British Home Office from local police agencies, courts, and penal institutions. The authors at a number of points in their presentation indicate the data validity and reliability problems associated with reporting policies; they nevertheless attempt to portray trends in reported crime, arrest rates, and dispositions of cases either "cautioned by the police" or brought before magistrate or higher courts for crimes of a more serious nature.

Differentials in reported crimes, arrests, and dispositions are shown for different crimes, sex, age, region, population size and density of given police jurisdiction, residence of offenders relative to the location of their offense, and recidivism. The authors present the findings as a descriptive study, eclectically invoking explanatory ideas instead of adopting or developing a theory of crime causation or control. Their explanatory

efforts include references to the increased mobility of the population, the rise of organized crime, the increased affluence of the general population, changes in the sex role of the female, and some minor excursions into, for example, the role of the mass media in fostering crimes of violence. Perhaps one of the most striking features of the study is the lack of radical differences between crime in the United States and crime in England and Wales. The major difference is the much smaller annual increase in reported indictable crimes between 1955 and 1965, as contrasted with the FBI Uniform Crime Reports figures.

Throughout the study a number of references are made to other research which is either completed or in progress concerning crime and criminal justice in England and Wales. In large part this work is either produced by the Home Office or by staff of the Institute of Criminology at Cambridge University. Little work done by American criminologists is referenced in this study. The authors are careful to point to a number of areas for future research which they consider necessary to interpret properly the trends and crime differentials reported in the study. They are particularly concerned about the problem of typifying the offender or the offense by legal labels rather than by situational or behavioral characteristics associated with the offenses and the offenders.

The authors include a chapter entitled "The Challenge of Crime to the English Penal System" in which they relate major and central findings from their prior analysis to existing conditions of law enforcement and the administration of criminal justice. A number of recommendations are put forward as well as criticisms and endorsements of changes presently being implemented or planned.

In general, this work will serve well as a descriptive reference work for comparative criminology. It remains for others and other research to interpret these data so that they can be incorporated into criminological theory.

*Is Scientific Management Possible? A Critical Examination of Glacier's Theory of Organization*, by JOE KELLY. London, England: Faber and Faber Ltd., 1968. 332 pp. 63 s.

ROBERT J. HOUSE  
*Bernard M. Baruch College, CUNY*

This book addresses the question "Can man in his ingenuity and wisdom devise organizational solutions which give technological and economic effectiveness which in turn will give

the affluence he so urgently wants, without offending the human condition?" The method of assessing this question is an historical analysis of the evolution and functioning of the Glacier Metal Company of Great Britain which is the largest producer of plane bearings in Europe, employing approximately 4,500 people in six locations and consisting of a number of subsidiary companies in Great Britain, South Africa, and New Zealand. The Glacier Company was selected because (a) it has an international reputation in the fields of human and industrial relations, and (b) it has developed its own distinctive management philosophy, the result of a penetrating and radical analysis of the problems of organizational life made chiefly by Wilfred Brown, Managing Director and Chairman of the Board of Directors, and Elliot Jaques, consultant to the company since 1952. The history of the company is traced through three phases: "The Traditional Phase" (pre-1942), "The Human Relations Phase" (1942 to early 1950's), and "The Task Management—Pocket Bureaucracy Phase" (post 1950). For historical data Kelly relies on books and articles written by Brown and Jaques; business sections of newspapers and business journals; and company directives, policies and procedures. In addition, questionnaire, activity sampling, and interview data are used to analyze and evaluate Glacier industrial relations, the role of the Glacier section manager, and the structure and functioning of the Glacier supervisor and Glacier meetings.

"The fundamental contention of this research is that the Glacier system is more determined by structural factors than any theoretical prognostications produced by its own management theorists. The major determinants of Glacier policy and behavior are economic, marketing, technological, political, social and cultural factors" (p. 29). Kelly argues that the industrial culture of the 1940's was essentially one in which "human relations" was the prevailing philosophy of management and that the Glacier decision to establish an industrial democracy-type works council is an effect of this culture. "During the post 1950 period Glacier developed into a pocket bureaucracy, with fixed routes of communication and the use of cryptic argot with a military ring about it; efficiency was in; and happiness, if not out, received no mention" (pp. 26–27). This bureaucratization is attributed to the fact that in 1956, when the company faced a sharp fall in earnings, it brought in management consultants who applied their standard techniques, mainly work/study, programing, and budgetary control as a means of improving profitability.

Kelly judges the Glacier system of management in regard to consistency, validity, pre-

dictability, and utilitarian value; he concludes that it fails the second test and passes the remainder. If this book is judged in terms of the same four criteria, it fails all four. In regard to consistency, the author claims to respect social science data and methodology, yet consistently fails to support factual statements and generalizes well beyond his data. In regard to validity, there is little or no attempt to validate the data despite its suspect representativeness. In regard to predictive power, Kelly makes no attempt to relate his data to social science theory or to test specific hypotheses. Finally, in regard to utilitarian value, the book appears to be more a source of confusion than a contribution.

*Industrial Man: Selected Readings*, edited by TOM BURNS. Baltimore, Md.: Penguin Books, 1959. 414 pp. Paperbound. \$2.25.

GEORGE RITZER  
*University of Kansas*

Sociology is inundated with books of readings, and Burns has contributed to this flood. The only redeeming quality of most readers is that they make life easier for the sociologist by putting a number of important readings together in one convenient location. This book does not quite achieve this goal although it does include several little-known (in the U.S.) pieces by Europeans.

The book is divided into five parts. Part One deals with industrialization and the development of industrial societies. Included in this section are familiar pieces by Weber on the Protestant ethic, Marx on the centrality of technology in industrialization and social change, and Smelser on differentiation, integration, and the resulting social disturbances. These are all familiar to American sociologists. More unfamiliar are excerpts from works by MacPherson in which he develops yet another typology of primitive to modern societies and Landes' piece on industrialization in Europe. Part Two, industrialism, is composed of readings by Marx on alienation, Dahrendorf on the service class, and Wilemsky's article on leisure. Part Three includes familiar work on industrial organization by Stinchcombe, Woodward, and Crozier, as well as a previously published article by the editor. Part Four, which is supposed to deal with the social worlds of the manager and worker, has only two articles. Dalton's classic, but dated, article on line-staff conflict is offered as a description of the social world of managers. An excerpt by Popitz, *et al.*, on worker's image

of society is presented to inform the reader of the social world of workers. The last section deals with work relationships and the work situation with three pieces. One is the well-known study by Roy of the piecework machine shop, while the other two are European studies of the effect of technological change in coal mining and of Norwegian seamen.

As the above summary indicates, the basic problem with this book is that, in choosing to cover virtually all of industrial sociology with seventeen readings, Burns has had to cover a number of areas rather superficially. As an example, Dalton's article on line-staff conflict does not adequately cover the social world of the manager. Further, many of the pieces are dated, with only seven of them being published in the last ten years. Scant attention is paid to the mass of recent studies in industrial sociology. Excerpts from Marx do not do justice to the concept of alienation in the light of recent research and theorizing on the concept. Burns' general introduction and the introductions to each section are brief; and, while some are useful, others are inadequate. For example, his introduction to the section on work relationships and the work situation consists of a largely irrelevant discussion of the ideas of F. W. Taylor. This book, then, offers the industrial sociologist some readings which are generally not found in volumes such as this; but it is marred by a number of serious limitations.

*Termination: The Closing at Baker Plant*, by ALFRED SLOTE. Indianapolis, Ind.: The Bobbs-Merrill Co., 1969. 340 pp. \$7.50.

JANICE B. VANLANDINGHAM  
Auburn University

*Termination*, stemming from research conducted by Michigan's Institute for Social Research on the influence of environment upon employee health, describes the impact of job loss upon middle-aged employees. Baker, a manufacturer and supplier of paint for the Detroit automotive complex, was a plant where facilities were added as demand increased; this haphazard expansion process, combined with relatively inefficient, though humane, personnel policies, eventually culminated in Baker's termination and replacement by an automated plant in another state. Because of the peculiar nature of the market, the manager of Baker had to maintain the plant in operation until a new facility was completed—a two-year process. Employees from Baker were not permitted to transfer to the new plant.

The author has written a book that is difficult to assess in the typical manner since it reads like a novel with an unhappy ending. He has skillfully woven a dramatic account of the circumstances surrounding, events occurring, and personalities involved in "putting to bed" a plant that had, for so many years, been home to so many people. Slote reconstructs, through details of interviews with thirty employees from the plant manager to the janitor, actions and reactions of Baker employees from the time that the first announcement was made to the final closing date. The result of his effort is an emotionally enveloping account of people displaced in a society of rapidly-changing technology.

Middle-aged employees, faced with the prospect of job loss, often assume a sick role as a means of coping with the socially undesirable status of unemployment or underemployment. Many of the employees began exhibiting symptoms of nervous disorders immediately following the first formal announcement of Baker's closing. During the duration of the termination operation, three salesmen died and familial problems seemed to occur more frequently among the employees. Since many employees, because of their age, could not find what they deemed "suitable" employment, the sick role tended to prevail even after the plant was completely closed.

The theoretical value of the book has to be inferred since any statement of theoretical orientation is limited to the brief introduction and to occasional references throughout the text. The bulk of the material is in diary form and/or of the case study type. The author utilizes emotional interpretations of the respondents' reactions to his questions in order to illustrate the soundness of his theoretical assumptions. For example, in an interview with Ned Rockwell, Chairman of the Union Bargaining Committee and employee of nineteen years, Slote concludes: "failure is written all over this man, etched in his words." Interpretations of this type are characteristic of much of the book. Although Slote's theory, on the surface, seems valid enough, one must reserve judgment until a subsequent publication presenting statistical evidence is made available.

*Termination's* value stems from its depiction of management-union strategies and from its description of the human problems of displacement. It comes very close to indicting business, the community, and government for their failure to assume any responsibility for the plight of the middle-aged unemployed. It is a realistic account which can add relevance to courses in industrial sociology, psychology, and social change.



*Innovations in Social Psychiatry: A Social Psychological Perspective Through Dialogue*, by JOSHUA BIERER and RICHARD I. EVANS. London: The Avenue Publishing Co., 1969. 211 pp. Clothbound, \$7.00. Paperbound, \$4.00.

HAROLD A. MULFORD  
University of Iowa

The authors of this work accomplish their purpose: "The present volume seeks neither to present a review of the literature relevant to the emergence of the community psychiatry movement nor to evaluate the relative importance of the individual pioneers who have contributed to the growth of this movement. Rather, is this book concerned with the subjective impressions of one such pioneer" (p. 7). The impressions are those of Joshua Bierer—founder of the Marlborough Day Hospital and Director of the Institute of Social Psychiatry, London—as expressed to Richard Evans, professor of psychology, of the University of Houston.

The work opens with Evans briefly sketching his own social psychological views of human behavior and mental disorders. The key concept in his model is that of socialization and de-socialization. He views mental disorders, especially psychosis, as involving the breakdown of the individual's basic socialization skills in one or more of the following areas: (1) the family, (2) the occupational environment, or (3) some aspect or aspects of general interpersonal relationships. It is within this conceptual framework that Evans formulates questions for Bierer.

For answers Bierer draws on more than 20 years of clinical practice centering on the idea that treatment of the mentally ill should remain within the realm of society as much as possible, thereby achieving a maximum degree of overlap between the treatment situation and the behavioral expectations of society.

The book is divided into two sections plus appendices containing original writings by Bierer. In the first Bierer comments upon the history and evolution of social psychiatry and offers his views on the changing concepts of psychopathology as well as some of the fundamental principles of social psychiatry. In the second he discusses some of the limitations which decrease the effectiveness of traditional diagnostic techniques and presents his concept of "total therapeutic community service," as an alternative approach to helping the mentally ill. He then discusses the progress being made toward attainment of his ideal of the therapeutic community which involves therapeutic social clubs, day hospitals, and autonomous therapeutic community hostels. Section Two

closes with a chapter by Evans in which he critically evaluates the dialogue content and offers suggestions for possible evaluative research programs. Evans seems painfully aware of the dearth of systematic objective evaluation of Bierer's work—perhaps because major concepts are not clearly defined and are only loosely tied together.

Bierer's original writings describe in detail his many years of work developing day hospitals, therapeutic social clubs, and therapeutic communities, and give some of the rationale for his work. In the final article Bierer proposes a new educational system whereby the teaching for the whole nation would be placed in the hands of the best 20 to 50 educators in the country who would teach through the medium of television, records, radio, and films. Students would be organized in groups, choosing their own group leaders. Older students would be chosen as leaders for younger children in groups of ten. He argues that "By giving young people responsible positions and a feeling of importance . . . they might be prevented from seeking recognition on the negative side in the form of juvenile delinquency" (p. 209). This chapter was apparently added as an afterthought aimed at the prevention of mental disorders by preventing juvenile delinquency.

The reader unacquainted with the notion of shifting help for the mentally ill away from the institution and closer to the patient's own social setting will find this book of some interest. To the reader more acquainted with social psychiatry the innovations referred to in the title date from some years ago.

*Mental Health and Social Policy*, by DAVID MECHANIC. Englewood Cliffs, N. J.: Prentice-Hall, 1969. 171 pp. Clothbound, \$5.95. Paperbound, \$2.50.

*Mental Health and the Community: Problems, Programs, and Strategies*, edited by MILTON F. SHORE and FORTUNE V. MANNINO. New York: Behavioral Publications, 1969. 209 pp. Clothbound, \$7.95. Paperbound, \$4.65.

RAY R. CANNING  
University of Utah

It is somewhat amusing and pathetic that "mental health," like "crime and delinquency" and other major social and personal problems, is "in" and, being "in," becomes the focus of swirling forces of unfathomable dynamics and magnitude—money, public opinion, political pressures, intramural competition, and conflict. The decades of the fifties and sixties saw Washington not only *prime* the pump but almost

swamp it. And so the "third mental health movement" grew. David Mechanic considers some of the aspects of these events.

A coherent public policy concerning mental health is a necessity, he says, "not only to meet bureaucratic requirements but also, and more importantly, to use the limited resources available to achieve the best balance of short-term and long-range objectives." This, of course, requires hard decisions among competing and persuasive alternatives, each of which in turn depends upon and is relative to a particular set of assumptions and concepts. "Whatever the present state of scientific knowledge, it is essential that we at least recognize the issues relevant to public policy decisions, define the alternatives open to us, and consider the probable advantages and disadvantages of each form of action." He denies any intention of offering simple or sweeping recommendations and turns instead to the deep issues and problems which must be taken into account if adequate social policy is to result.

Mechanic introduces the mental health professions and their orientations but for brevity limits both discussions severely. I would have preferred a more extensive coverage of these two subjects in Chapter 1. In Chapters 2 and 3 he discusses various answers offered by these differing mental health professionals to the problems of defining mental health and illness and explaining their causes and possible control. Mechanic is not doctrinaire, nor is he cowed. Carefully he wends his way through the cross-fire, inspecting one position after another, and survives with no apparent wounds or hatreds.

Having paid tribute to the pioneers of the first mental health movement (Pinel, Dix, *et al.*), and the second (Freud and the analysts who followed him), Mechanic turns in Chapter 4 to post-World War II developments, especially so-called community psychiatry. Its responsibility becomes huge or *overwhelming*, depending upon which frames of reference and definitions of mental illness are adopted and therefore which criteria are used to identify disorders or cases and to plan their treatment. Needless to say, whatever combination prevails (and perhaps we are lucky that no single one holds sway) policy makers need to give careful consideration to issues and difficulties discussed in the last section of this book. They include choosing criteria for measuring the effects of alternative mental health programs (Chapter 6), concepts and strategies of prevention and community psychiatry (Chapter 7), and mental illness, the community, and the law (Chapter 8). In his final chapter, "Looking Toward The Future," Mechanic patiently but firmly calls us all together, the "team" that sometimes fights better

among ourselves than against the "enemy," and reminds us of our weaknesses and our potentials for intelligent joint action. To our claims and counterclaims he insists we "pay deference to the facts rather than to those who proclaim them." And he keeps us humble with a final reminder that "mental health is not the panacea for all our problems and difficulties."

Shore and Mannino, on the other hand, seem to fit most social problems under the umbrella of *mental health* problems. Unlike Mechanic, they assume the broad-spectrum definition and move on to their major concerns of problems, programs, and strategies. While Mechanic had the advantage of writing his own book, Shore and Mannino faced the three basic problems of editorship: (1) the difficulty of insuring consistency and high quality among the selections, (2) maintenance of a tight focus, and (3) responsibility for logical integration. Given their wide focus for "mental health" (ranging from support of urban renewal to consultation services to schools and development architects) they achieve an acceptable level of success. The papers are generally well written and by competent people. Logical integration is achieved by dividing the book into four parts which form a sequential consideration of first, programs with a social action orientation, second, programs with a mental health service orientation, third, programs with a problem-solving orientation, and, finally, new areas. Probably the greatest value to the reader interested in the various types of programs is the clear and candid reporting of failures and successes—of unexpected but crucial personal and social crises. Like case studies from applied anthropologists and administrators, these reports are personal documents honestly elucidating the pragmatics and foibles of planned change. As the editors say in their final words, "They are not intended to serve as a manual. If the authors have elicited discussion and/or controversy out of which new programs, new ideas, or new strategies will be generated, the volume will have served its purpose." Clearly, this is a worthwhile aim and one which I believe is generally achieved.

*Industrial Organizations and Health*, edited by FRANK BAKER, PETER J. M. MCEWAN, and ALAN SHELDON. Volume I: *Selected Readings*, New York: Barnes and Noble, 1969. 699 pp. \$14.50.

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This collection of 31 previously published articles will disappoint those who approach it

with the intention of learning how various types of industrial concerns and work settings are related to health. Most of the papers bear no resemblance to the kind of research carried out, for example, by Kornhauser in his *Mental Health of the Industrial Worker*.

The opening section includes papers by Inkeles and Levinson and French and Kahn which provide frameworks for the study of the relationship between organizations and personality by conceptually specifying relevant dimensions of each. A paper by H. Levinson views organizations as refuges "for recouping psychological losses in a rapidly changing society." Organizations, we are told, serve as objects of transference, provide defenses against unconscious anxiety, and relieve depression. It is also reassuring to learn that if the organization does not meet the individual's needs "he can leave it." This apologia for organizations is balanced somewhat by Argyris' study of aspects of industry which impede the fulfillment of human needs and aspirations.

Many of the articles in the second and fourth sections are redundant. The bulk of the papers relate factors such as absenteeism, accidents, and morale to organizational variables such as size and span of control. Especially worthwhile is Zander and Quinn's review of industrial research. Of the components of work situations found to be associated with variations in mental states the most prominent was the extent to which workers control their own fates. Positive psychological states are enhanced when workers participate in decision-making and have a high degree of job autonomy.

The articles in the third section are generally good but unrelated to each other. Included are research reviews on frustrations of industrial work, aspiration-achievement discrepancies and mental illness, and aging in industry. There is also a paper by Faunce on automation and a discussion of the techniques employed by organizations to reduce anxieties over demotion.

In the final section Pagès, Bennis, and Shepard and Blake discuss strategies for inducing change in organizations. The principal technique recommended is some form of group therapy such as laboratory training or sensitivity training, the purpose of which is to facilitate communication and cooperation by resolving irrational intra- and interpersonal conflicts, becoming for organizations what laxatives are to human organisms. In both cases appropriate measures are introduced to open the channels of a "clogged" system. Such measures are, therefore, more conducive to establishing equilibrium than to inducing genuine change.

The authors' faith in group therapy may account for their failure to delineate the types of

situations in which it may be employed effectively. For example, laboratory training probably works only when participants are drawn from the same level of the organizational hierarchy. As Katz points out in his paper, the technique only facilitates communication when the source of the problem is irrational. However, the most significant and persistent conflicts in industry have a rational basis as, for example, those between manual workers and management over wages and control of work.

It is impossible to recommend a reader that purportedly focusses on industry and health but which largely ignores crucial components of each and the relationships between them. Certainly, good studies are available which bear *directly* on the issue of health and industry. It is apparent that the health of members of industrial concerns is important to the editors primarily when it interferes with the efficient operation of the organization. Otherwise, how can one account for the numerous papers on absenteeism, motivated accidents, and morale? Furthermore, the editors obviously are more interested in organizations than with *industrial* organizations. This emphasis is evidenced by their failure to consider literature dealing with the special and often stress-laden conditions of work faced by manual workers as well as by the lower echelons of white collar employees.

The papers in this volume oriented to the efficiency and benevolence of organizations far outweigh those expressing a concern with human problems generated by industrial concerns. Consequently, its contents, like its price, may be more suitable to the tastes of managers than sociologists.

*Lectures in Plenary Sessions*, edited by MARK KELLER and TIMOTHY G. COFFEY. Volume II of the *Proceedings of the 28th International Congress in Alcohol and Alcoholism*. Written in French and English. Highland Park, N. J.: Hillhouse Press, 1969. 252 pp. \$7.50.

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The volume contains lectures and addresses dealing with various aspects of alcohol research, treatment, and control, along with statements regarding the purposes, functions, and background of the 28th International Congress on Alcohol and Alcoholism, held in Washington, D.C., in September, 1969. Each lecture is accompanied by an abstract printed in the alternate language, either French or English, to the one in which it was originally presented.

For the most part, the lectures are general summary statements directed at a limited num-

ber of issues. With few exceptions, the papers fit into the following categories: (a) current trends in physiological, medical-psychiatric, and socio-cultural research on alcoholism and drinking behavior; (b) discussions of attempts at government control of alcoholic beverages in Scandinavia, Australia, and the U.S.A.; (c) the "relationships and non-relationships" of alcohol and drug problems from several different perspectives, e.g., education, treatment; (d) the role of therapy in dealing with problem drinkers and alcoholics; and (e) other strategies and policies regarding alcohol use as a social problem.

Given the nature of the Conference and the diversity of its membership, the papers were not directed to specialists in any single discipline. Those who expect detailed discussions relevant to a specific interest will not want to read this book. On the other hand, for those looking for general, up-to-date information regarding the activities of those in other disciplines concerned with alcohol use, the book will be valuable. Since this is its strength, it is unfortunate that the editors failed on occasion to obtain and include references to articles cited in the papers and that they did not take time to present the papers in a consistent style.

Although the lectures are generally objective in their approach to alcohol use, leanings toward the morality issue are, as one might expect, inevitably in evidence. In fact, with the possible exception of a physiologist, one conclusion reached or implied in several of the papers and especially in those presented by a clergyman, a judge, and a road safety expert is "Never serve liquor to rats." Compared with the alcohol literature of the not-so-distant past, however, the general tenor of these papers suggests an openness of thought which will hopefully enter soon into our legal and social dealings with, and definitions of, the alcoholic.

*Poverty and Mental Retardation: A Causal Relationship*, by RODGER L. HURLEY. New York: Random House, 1969. 301 pp. Cloth-bound, \$6.95. Paperbound (Vintage), \$1.95.

SAMUEL A. KRAMER  
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This book professes to be "a new assessment" of mental retardation due to economic poverty, and describes the effects of cultural deprivation, inadequate and ineffective public education, lack of health care, and the continued dependence created by welfare programs. However, *mental*

retardation is almost wholly disregarded after the title and first chapter and is made synonymous with *social* retardation. The author claims that mental retardation and environmental deprivation are not "easy to separate in practice," so apparently he does not even try. Among others, Senator Kennedy was confused by this lack of definition since he refers in his foreword to "those among them we all too conveniently choose to call 'mentally retarded'."

As a descriptive commentary on poverty, this book is merely another of many concerned with the urban crisis, civil rights, the unemployed, the uneducated, and similar social problems. The claimed objective is missed: i.e., "to delineate the relationship between poverty and mental retardation and to place mental retardation in its true perspective as a social pathology which thrives especially in the ghetto." Selected quotations and references are used to "prove" a point. Sometimes quotations are used without citation for such proof, e.g. "Medical people seem wedded to the idea that 'if only they were not so lazy, they would enjoy the American standard of living as we do.'" There are easy transitions from selected writings to generalized claims. E.g., "estimates are that the worker's child in the USSR has *twice* as good a chance of going to college as his US counterpart . . . an ugly fact, but one we must permit to crawl out from under its rock." An estimate has become a fact; yet two pages later the author challenges the estimates of Goddard and other scientists because they have not overcome their "ignorance and prejudice." References to "poor prenatal care which can impair intellectual performance" are readily altered so that *can* becomes *do*, and *intellectual performance* becomes *mental retardation*.

Frequently, the author accepts anything which suits a desired position, no matter how contradictory. On pages 14, 15, and elsewhere he rejects the concept of IQ. "It will be shown later that currently used IQ tests are almost completely inadequate for evaluating the intellectual potential of the poor." Nevertheless, on pages 24, 29, and throughout the chapters on intellectual performance and education, IQ is used by him as an acceptable measure of deficiencies in achievement. "Seventy percent of 4,000 high school dropouts, the majority of whom were of low socio-economic status, possessed normal or above-average IQ's. Obviously, the system is not educating the very people whom educators believe have the potential to learn." Many other IQ results are cited approvingly when they meet the desired purpose.

In addition, very positive statements are made without proof. "Certainly one reason (for the predominance of Negroes among mentally

retarded) is that they are physically deprived to a greater degree than children from other ethnic groups." Certainly! Again, on page 92, two experiments in education "prove without question that poor children can be educated just as well as children from any other socioeconomic class." Prove! Statistics follow the same pattern of selectivity and imagination. "It is impossible to say with accuracy what the ratio is between those who suffer from organic damage and those who do not; but there are probably eight poor, deprived people—including Negroes—who are not organically damaged to every person who is." Probably! Curious readers may wish to continue this by comparing page 27 with 54 about lifelong dullness and pages 99, 103, and 106 about the pupil tracking system in school.

This book is not a study of mental retardation, and even the author seems to have forgotten his purpose. On page 132 is a list of 13 maladies afflicting the poor, but he does not

include mental retardation. On pages 236-237 is another list with the same omission. This may well be the only current book purporting to discuss mental retardation that does not even mention the National Association for Retarded Children. It also disregards the many-faceted Collaborative Perinatal Research Project of the National Institutes of Health which started in 1959 and will continue until 1974, studying 58,000 women and their children, most of whom are poor. It is the most extensive and expensive study ever undertaken but the author of this book did not recognize it.

Perhaps it would serve an interesting purpose to point out that a reference to New Jersey, the focal point of this study, is another error. New Jersey historians and St. Augustine city leaders may be surprised to learn on page 199 that Newark is "the third and oldest city in the nation." They will not be more surprised than I, as a medical sociologist, was in reading this book about mental retardation.

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# TRENDS IN VOLUNTARY ASSOCIATION MEMBERSHIPS OF AMERICAN ADULTS: REPLICATION BASED ON SECONDARY ANALYSIS OF NATIONAL SAMPLE SURVEYS \*

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American Sociological Review 1971, Vol. 36 (April):191-206

*A small but noteworthy increase in the percentage of American adults who belong to voluntary associations has occurred since the mid-1950's (the date of an earlier study of this topic by the authors), as documented through several replications of national sample surveys. These same replications also confirm a major generalization of the earlier study that such membership is less common than had been assumed; indeed, voluntary association membership is not characteristic of the majority of American adults. Data from the replications confirm the previously demonstrated relationship between membership and major socioeconomic characteristics; but subgroup trends suggest that during the more recent period there has been a sharper growth in associational membership among the lower status groups. Although the findings are not completely consistent, there also seems to have been a sharper growth in association memberships on the part of Negroes. All of these findings, like those in the earlier study which has been replicated, are based on secondary analysis of national sample surveys. These social trends should be systematically measured and documented in the future by additional replications and new primary surveys.*

A DOZEN years ago in this journal, by the secondary analysis of survey data, we presented the first evidence on the magnitude and correlates of voluntary association memberships of Americans, based on probability samples of the adult population of the nation (Wright and Hyman, 1958:284-294). The surveys on which we drew dated from 1953 and 1955. Those results, by virtue of their generality and

rarity, continued to be useful over the years to many social scientists interested in the topic. But with the passage of time, scholars dependent on the old evidence have been forced to make a giant inferential leap from the data of the early fifties to the facts about America as it approached and entered the seventies.<sup>1</sup>

\* We gratefully acknowledge grants-in-aid from the Public Affairs Center of Wesleyan University and, from the University of Pennsylvania's Computer Center. We also express our appreciation to the National Opinion Research Center, University of Chicago, and to Patrick Bova of that organization; and to the Interuniversity Consortium, University of Michigan, for making survey materials available to us; and to Professor Sidney Verba who provided unpublished data from his present research in progress; and to Judith Beinstein, Donna Ellis, and Barry Goldman, who assisted on the project.

<sup>1</sup> Other secondary analyses of national data in the literature since 1958 have served to enrich our knowledge of the phenomenon and its correlates, and of the range of measurement error, but have not served to up-date the estimates and document trends. Findings reported by Lane (1959:78) were based on a small national sample surveyed in 1952. Findings reported by Hausknecht (1962) were based on a 1954 survey and a reanalysis of the same 1955 NORC survey we had previously employed. Lazerwitz (1962:690-696) presents data for 1957 from a national study by the Survey Research Center. More recent evidence, referring to 1960, was presented by Almond and Verba (1963:300-322), but their published analysis examines cross-national differences, and the trend in America

One might make a good argument that the prevalence of membership in voluntary associations and its patterning in the social structure have remained stable. Some, harking back to Tocqueville, may see the phenomenon as rooted in the American national character or in the basic legal and political institutions which permit and encourage such involvements on the part of citizens. Others who stress social structural factors as the basic determinants of membership may also see the phenomenon as firmly rooted. If such conditions endure, why then should there have been any major changes in the findings?

Such arguments will hardly convince those who can advance plausible speculation for rival hypotheses. As the population has grown more affluent and educated, more individuals have arrived at the very circumstances found in repeated studies to increase voluntary association membership. In addition, as particular groups within the population have become mobilized and politicized by events—the poor, Negroes, young adults—their organizational involvement correspondingly may have grown. Certainly, the social and political changes we have all experienced could have increased the memberships of Americans in the aggregate and altered the former patterns and differentials found among subgroups.

But perhaps there are other currents moving the patterns in unexpected ways or reversing the normal trends. Some groups who had entered upon a phase of association may now have moved beyond the period of involvement and participation to a stage of disillusion and alienation. Perhaps they have rejected “conventional” associations. Instead of an increase in some quarters, there may have been a decline in voluntary association membership, as individuals retire prematurely into isolation and apathy or form into transient social movements which have not yet solidified into new forms of stable association.

is not treated. As will be described, we have used their American sample in our current analysis. To convey the stretch necessitated from old data to current description, we may note that several publications revised as recently as 1969 and one dated 1970, plus others published for the first time in 1969, reprint or cite our 1955 data as the only evidence.

Empirical evidence, not speculation, is needed. We ourselves have not yet the resources to undertake costly and systematic primary surveys on the problem or to finance a supplementary inquiry to be “piggy-backed” on appropriate national surveys. We hope that day will soon come for us and others. Even when it dawns, however, the course the phenomenon has taken in the years between 1955 and 1970 will remain a mystery, unless we find some way to illuminate that period. Other social scientists—with one possible exception—also seem not to have had the resources to carry out systematic *national* surveys on the phenomenon since 1955, nor to have published any substantial evidence on the national picture or trends.<sup>2</sup> But if secondary analysis served us well before, perhaps it can serve us again to provide evidence on the more recent patterns and the changes in voluntary association membership that have occurred since the mid-fifties in America. Such is our hope, and we shall try to replicate our earlier analysis and present whatever trends may be documented by available surveys.<sup>3</sup>

<sup>2</sup> The work of Almond and Verba constitutes the exception; Verba's data from a 1967 survey when published will provide elaborate evidence for that one point in time. To be sure many important *local* studies of voluntary association membership have been published over the years, attesting to the lively research interest in the topic. But none of these describes the large picture for the nation or lends itself to the measurement of national trends; all relate to a circumscribed universe such as a small community, a single metropolis, or even a neighborhood within a city. For obvious methodological reasons, such as variations in sampled populations, sampling procedures, instruments and operational definitions, to mention just a few, an analyst could not simply patch together these various local surveys to approximate a national study. Equally obvious, the findings about extent of affiliation in local studies need not be commensurate with our earlier or current national estimates of voluntary association memberships. For specific examples and further comment on these points, see our earlier paper (Wright and Hyman, 1958: 285, footnotes 3 and 4) and Babchuk and Edwards (1965:150).

<sup>3</sup> In a recent monograph, Duncan (1969) has made an eloquent plea for the systematic study of social trends by the replication of previous inquiries, and among the phenomena, which he singles out as important for such monitoring, he includes membership in voluntary associations. In noting that the 1955 NORC data are cited in a government publication as recently as 1969, he remarks, “It is

## METHOD AND DATA

It would be ideal if our search had revealed a series of completed surveys, comparable in every detail with the earlier 1955 survey and spaced out through time up to the present. Then any trends that might have been documented could be attributed to real factors and could not be discounted as artifacts produced by changes in procedures from survey to survey. If, in addition, near each point in time, there had been several surveys clustered closely together which were exact replications or which employed varied operations, it would have been possible to assess the reliability of the measurements and appraise the changes in results that flow from different operational definitions of the concept of membership, and different measurement approaches.

This ideal cannot be achieved, but parts of the grand design, smaller than desirable, but nevertheless attractive in nature, can be provided. As will be seen, we have sacrificed contemporaneity for comparability in selecting one major survey conducted in 1962, which came close to being an exact replication in the critical, technical respects essential to providing accurate evidence on the change over a seven-year period. From other surveys, we can piece together with this an approximate picture of changes over a longer period, and also obtain evidence on the degree to which the aggregate estimates and the subgroup patterns fluctuate because of the measurement procedures employed. To anticipate our discussion, we summarize in Chart I the component surveys used. Their exact function in the total design will be reviewed.

## CHART I

SURVEYS EMPLOYED FOR REPLICATION  
AND TREND ANALYSIS

## Replication I: 1955-1962

Comparable national surveys by NORC, sample sizes 2379 and 1775. Memberships of adult respondent in local associations, excluding unions.

## Replication II: 1953-1958

Comparable national surveys by NORC, sample sizes 2809 and 2691. Memberships of family unit in associations, including unions.

## Supplementary Sources:

1960 National survey of NORC, sample size 930. Memberships of adult respondent in associations, excluding unions. Instrument different from Replication I.

1967 National survey by NORC, sample size 3015. Memberships of adult respondents. Instrument different from Replication I or 1960 survey.

Considering matters in their worst light, scholars will no longer have to take such a giant leap forward into time, having as their empirical take-off point new evidence up to 1962. And their confidence before the take-off can be bolstered or qualified, since they now will know, at least, what stability or change in membership accompanies a seven-year interval, and since they can apply that knowledge to the inferences they are forced to make about the mysterious course of the phenomenon in the most recent interval.

We shall briefly describe the elements of comparability in the 1955 and 1962 surveys used to document aggregate trends and subgroup patterns over the seven-year interval. Both surveys were based on national probability samples of the noninstitutionalized population of the United States over 21 years of age. The 1955 sample contained 2379 respondents and the 1962 sample, 1775.<sup>4</sup> In both surveys, the question on voluntary association membership came at the end of a long personal interview on matters unrelated to voluntary associations, and there is no reason to think in either instance that the prior sequence of questions would make the matter of organizational membership salient or prestigious.<sup>5</sup> The relevant bat-

<sup>4</sup> For a detailed description of the sample design in the 1962 survey, the reader is referred to Johnstone and Rivera (1965). As the reader will find in that source, the sample design incorporated several supplementary samples including individuals under age 21, all of which could be combined by various weighting procedures. Our description applies only to the unweighted adult sample which could be easily segregated for our purposes. The completion rate for that sample is reported as 82%. A detailed description of the 1955 survey is presented in Feldman (1966), where the completion rate is indicated also as 82%.

<sup>5</sup> On the possible influence of the previous context of questions in changing the estimates of voluntary association membership, see Hausknecht's (1962: 127-129) discussion of the different esti-

curious that no more recent information is available" (Duncan, 1969:32).

teries of questions were virtually identical. In 1955: "Do you happen to belong to any groups or organizations in the community here? Which ones? Any others?" In 1962: "Do you belong to any groups or organizations here in the community? Which ones? Any others?"

Both surveys were conducted by the National Opinion Research Center, and therefore the general research procedures—the way the samples were designed and carried out, the way coding was handled, etc.—were comparable. But what should be stressed is that the style of interviewing and the character of the field staff was also constant over the surveys, which constancy is critical for the kind of data elicited by an open-ended question and supplementary probes. The results otherwise might vary simply because interviewing staffs differ in the degree to which they probe.<sup>6</sup> To be sure, one might speculate that a series of closed questions about different types of membership, or a card question that aided recall, might elicit more complete answers and more valid findings on the full extent of membership.<sup>7</sup> Whatever intrinsic limitations the open-ended approach may have, it nevertheless was operated in the same way over the two surveys. In the attempt to replicate the tests of the social correlates of membership, and in the examination of the refined trends for particular subgroups in the population, the constancy of the agency involved also increases our confidence that such variables as income and education, among others, have been enumerated in the same way in both surveys and that the same coding conventions have been applied to the raw data. Otherwise, any apparent changes observed might simply reflect capricious classification of the respondents, and variations in the way such

questions were put to them in the two surveys.

The concept of a "voluntary" association suffers from some ambiguity—what shall be included under that term? In the 1955 survey, the question, as noted in our earlier article, functioned so that union membership was not enumerated or counted in scoring the number of associations to which the respondent belonged. In the 1962 survey, union membership was also enumerated, but entered under a separate code category. Consequently, to maintain comparability, unions are given no weight in scoring number of memberships, and the estimates and the trends should be interpreted in light of this exclusion.

In summary, by choosing the 1962 survey, the trend analysis is based on general comparability in sample design and execution, question wording and conventions in scoring, question context, interviewing staff and procedure, and the general consistency that would characterize the work of a single, long-established survey organization. If such advantages could have been reaped with more recent surveys, it would have been even better; the inferential leap would have been shorter. But the sacrifice in timeliness forced upon us seemed a small price to pay for the improved design that resulted.<sup>8</sup>

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<sup>8</sup> Duncan makes the argument in the most emphatic way: "In a serious replication, one will repeat faithfully the 'errors' of the original study as well as adhere to its 'good ideas'" (1969: 27). To be sure, he also urges by his concepts—"splices" and "effect calibration"—that the errors due to procedures be estimated. The 1953–58 replication we shall present may be regarded, in juxtaposition with our 1955–62 replication, as providing such a test of the effects produced by particular instruments.

A search of the files of the Roper Center in 1968 indicated that the only national surveys since 1965 that contained an appropriate question were a series of Gallup Polls where the question was limited to union membership. A separate search of the NORC files netted six other surveys containing a relevant question conducted between 1955 and 1968. Two of these were conducted in 1958 and 1959 and were comparable in design to previous surveys, but do not extend the trend much beyond 1955. The 1958 survey, however, serves to provide one strategic trend measurement in our analysis. Surveys conducted in 1961 and 1964 contained a comparable question, but the sampling design, best described as a "modified probability sample," made them less comparable with the 1955 survey, and

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mates obtained in a Gallup 1954 survey and the 1955 NORC survey which had dramatically different topics prior to the particular question.

<sup>6</sup> For an illustration of different "house styles" of interviewing, and the variation between Roper and NORC interviewers in handling free-answers, see Riesman (1958: especially 344, 352).

<sup>7</sup> In our earlier paper some evidence from the comparisons of the use of a card question vs. an open-ended question suggested that the results were not that vulnerable to changes in instruments. Additional evidence will be presented below.

*Trends in the Aggregate and for Major Subgroups*

The data in Table 1 present the national trend over the seven-year period. In our judgment, the first conclusion to be drawn is that American adults, whether studied in 1955 or in 1962, most frequently are *not* members of voluntary associations. A majority report no membership whatsoever, and only a small percentage belong to many associations. In this sense, the findings of our earlier paper are reconfirmed. This is not to dispute the fact that the trend reveals a small increase in such membership, distributed into the various levels, a point to which we shall return later in the paper. But whatever increase has been registered, we do not suddenly find the emergence of any large proportion of Americans who have involvements in *many* associations.

The conclusion about 1955 *or* 1962 is, of course, dependent on the specific operational definition of voluntary association member-

ship implied by the wording of the question employed in both surveys. In our earlier paper we had also drawn on a 1953 NORC survey of a probability sample of adults, in which the measure of membership was much more inclusive. The question read: "Does anyone in the family belong to any sort of club, lodge, fraternal order, or union with ten or more members in it? What organization? Any other?" This question, of course, yielded a higher estimate of membership, since unions were counted, no restriction as to the local community was introduced, and the unit of description was the family, rather than the individual respondent. A totally uninvolved individual vicariously became a member through the activities of others in his family. For these reasons, the difference between the results obtained with the indicator used in 1953 (or in 1958) and the one employed in 1955 (or again in 1962) should not be attributed to unreliability of measurement. Different concepts are implied and the unit being described is different. The basic conclusion, however, seemed confirmed by the 1953 indicator—almost half of American families did not have any membership whatsoever in associations or unions.

The 1953 indicator can also be used to yield another test of the *trends* in the phenomenon during the same general period. In 1958, another NORC survey of an equivalent probability sample, using this same indicator, provided a trend measurement over the five-year interval, 1953–58. The instrument was almost identical.<sup>9</sup> The sequence of questioning was: "Does anyone in the family belong to a labor union? What other organizations—like clubs, fraternal orders, professional associations, or civic groups—with ten or more people in them, do adults in the family belong to?" The trend revealed over the two surveys is presented in Table 2. Despite a small increase in the proportion reporting membership, four out of every ten families in 1958 had no members who belonged to any voluntary association, including unions;

Table 1. Percent of Adults Reporting Levels of Membership (Excluding Unions).

Number of Memberships	1955	1962
None	64%	57%
One	20	22
Two	9	11
Three	4	6
Four or More	3	4
Unknown <sup>a</sup>	-	-
N	2379	1775

<sup>a</sup>Less than 0.5%.

perhaps less unbiased a source of data; they are not used here. Aside from the 1962 survey on which we place heavy reliance, we have made supplementary use of the two other NORC surveys: the 1960 survey for Almond and Verba is comparable in some respects and is exploited; the survey in 1967 for Verba and Nie is the most recent, and contains a detailed battery of questions. Although it is not comparable in sample design or instrumentation, it is drawn upon.

<sup>9</sup> Both the 1953 survey (No. 335) and the 1958 survey (No. 409) dealt with matters of health and medical care. The battery on voluntary associations came near the end in both cases, and the prior context was unrelated to the phenomenon and unlikely to make the issue of voluntary association membership salient or prestigious.

Table 2. Percent of Families Reporting Levels of Membership (Including Unions).

Number of Voluntary Associations	1953	1958 <sup>a</sup>
None	47%	38%
One	31	32
Two	12	16
Three	5	7
Four or More	4	8
Unknown	1	-
N	2809	2691

\* In 1958, some 8% of the respondents were coded as "not ascertainable" on number of associations, in contrast with the 1% in the 1953 survey. If all or most of these were really to fall into some particular level of membership, the trend findings would be changed. Through the cooperation of Patrick Bova, the librarian at NORC, the original questionnaires were located. Inspection reveals that almost without exception, none of these respondents gave ambiguous or vague answers as to the number or types of memberships. Almost all of them represent the complete omission of any recorded answer whatsoever. In light of the physical format employed in 1958 and a possible confusion arising from the "skip instructions" in the previous question, it seems highly likely that the interviewers simply omitted to ask the question, and such a clerical error probably distributed at random. It seems reasonable to assume that the distribution they would have is the same as those who had answered, and the percentages, eliminating them from the base, is the same as such an allocation. The alternative hypothesis that "no written entry" really means "none" does not seem borne out since such entries were recorded in over a thousand of the questionnaires.

and less than three out of every ten belonged to two or more associations. Whichever set of data is examined—in Table 1 or Table 2—it appears that the magnitude of the trend toward increasing voluntary association membership is about the same, about a 7% rise between 1953 and 1958, or between 1955 and 1962.

We return to the evidence presented in Table 1, these data being more recent and describing change over an interval of seven years. The increase in voluntary association memberships between 1955 and 1962, although small, should not be rejected on simple grounds of sampling error or discounted as trivial.<sup>10</sup> Although we regard the increase

as not large in magnitude—certainly not large enough to say that most Americans had become "joiners"—the trend revealed could have social significance. An increase in this form of social involvement by 7% of the adult population involves millions of individuals. (To be sure, mere membership does not insure active participation in the organization.) Furthermore, even a small increase in figures for the national aggregate may incorporate and reflect larger changes within various subgroups and strata of the population.

It seems worthwhile, therefore, to investigate these trends in voluntary association membership, exploring the changes over time for specified subgroups and various types of individuals. These refined analyses also provide an opportunity to replicate by more recent surveys the tests of correlates of membership previously conducted with the data from the fifties, as well as to introduce new variables into the analysis, which were measured for the first time in the later surveys. But first we shall examine the possibility of contribution to the change by the new cohort of young adults who were incorporated in the 1962 sample but who were children at the time of the 1955 survey, and therefore excluded. Perhaps it is their vitality that has been fed into the body politic which has invigorated associational life in the community.

The age group, 21–27 as of 1962, were too young in 1955 to be members of the population of adults who were then sampled. As Table 3 shows, it cannot be their life blood which has transfused the later survey findings. To the contrary, they depress the level of membership, since 75% of them do not belong to any voluntary associations.<sup>11</sup>

lished report for various N's and percentage estimates. Applying these figures to the estimate of voluntary association membership, it would be fair to conclude that the finding of 57% having no memberships has a standard error no larger than 1.4%. The sampling design of the 1955 study, with one minor modification, parallels that employed in the 1962 study. Careful review of the published account plus discussion with the personnel involved would suggest that the sampling error of the 1955 estimate is of the same magnitude (Johnstone and Rivera, 1965: 487; Feldman, 1965:12–15).

<sup>11</sup> From Hausknecht's (1962) analysis, we know that the pattern among young adults is not restricted

<sup>10</sup> The sampling errors, for the type of sampling design used in the 1962 study, were computed for various characteristics, and presented in the pub-

Table 3. Voluntary Association Membership by Age, 1962 and 1955.

Number of Memberships	21-27		28-34		Over 34	
	1962	1955	1962	1955	1962	1955
None	75%	77%	56%	62%	54%	63%
One	17	14	24	21	23	20
Two	4	6	10	11	12	9
Three	3	2	6	4	7	5
Four or More	<u>2</u>	<u>1</u>	<u>3</u>	<u>2</u>	<u>4</u>	<u>3</u>
N	248	302	263	434	1245	1643

There is, however, an ironic possibility to be entertained. Although the cohort who had entered into adult life since 1955 may be less involved than their elders are in the 1962 survey, it is conceivable that they are more involved than were their counterparts in the 1955 survey. Any increase in associational membership in the new generation could thus contribute to some of the aggregate rise as between the two surveys. The data in Table 3 on the pattern by age groups at the time of the 1955 survey again document that the youngest adults are the least likely to be members of associations in the community. By juxtaposing the various findings in Table 3, one can also trace a process of rising memberships as a cohort grows older, since those who were 21-27 in 1955 are the 28-34 years old in 1962. But by comparison of the same age groups in the two different surveys, one also documents the fact that the youngest adults, whatever their generation, are about equally uninformed.<sup>12</sup> It is not they who account for the

trend. Indeed, the *older* adults, as the several tables reveal, have been affected, for reasons yet unknown, in the recent period and have enlarged their memberships over the level of their counterparts in earlier times.

#### SOCIAL CORRELATES AND SUBGROUP PATTERNS

In examining the patterns of membership among different social groups, we can focus on the persistence of the relationships—in a sense, replicating the tests of the earlier survey. We can also regard these data as the disaggregating of the national trends, while we wait to see whether sharp changes in particular subgroups contribute disproportionately to the total change between 1955 and 1962.

#### *Social Stratification and Membership*

To document comprehensively and reliably the relationship between socioeconomic position and voluntary association membership, we present the findings in Table 4 using two major indices. Since the data are unwieldy, we have had to collapse the information in some degree. (Where some refinement of special interest has been obscured in the table, we shall comment on it.) Looking down the columns of the table, either in the

to membership focused around the *local* community. In the 1954 Gallup Poll he analyzed, the question was not restricted to associations within the local community, and there again the youngest age group examined, 21-29, reports a lower rate of membership than all other age groups; even those sixty or over show higher membership (1962:41).

<sup>12</sup> We have not presented refined age-breaks over the entire continuum of age, since our concern was essentially with the possibility that the youngest group and the most recent generation of youth account for some of the national trend. However, Hausknecht's detailed data for the 1955 NORC survey documented that the very youngest adults, aged 21-24, report the lowest membership rate, much

lower even than that documented for those who are over age 65 (1962:41). The finding is all the more dramatic as we realize that the youngest cohorts are much better educated than their elders, a variable that normally correlates positively with membership. For data on the interaction between age and other social characteristics, see Hausknecht's other analyses.



Table 4. Percent Belonging to Voluntary Associations by Family Income and Education.

	1955				1962			
	0	1	2+	N	0	1	2+	N
<b>Family Income:</b>								
Under \$2000	76½	17½	7½	385	69½	16½	14½	230
\$2000-2999	71	17	12	304	62	24	14	167
\$3000-3999	71	18	11	379	70	21	10	175
\$4000-4999	65	21	14	450	58	26	16	183
\$5000-7499*	57	22	21	524	56	25	20	592
\$7500 or More**	48	22	30	328	45	21	35	389
<b>Education:</b>								
0-8 Years	77	15	8	870	67	22	11	522
9-11 Years	67	20	13	495	64	22	14	352
12 Years	57	23	20	610	53	23	23	528
1-3 Yrs. Col.	46	24	30	232	48	24	27	194
4 Yrs. of Col. or More	39	25	36	170	37	20	44	164

\*In the 1962 survey this interval ends at 7,999.

\*\* In the 1962 survey this interval begins at 8,000.

left or right hand half, reveals the general relationship. Looking along any row of the table reveals the seven-year trend for any particular subgroup.

Inspection of the findings reveals that the substantial relationship between membership and higher status, documented in the earlier study, still holds.<sup>13</sup> If we examine the trends to locate the sources of the changes

<sup>13</sup> To round out the evidence, and parallel the data of the earlier report, we note that, as before, renters are less likely to be members of the associations than home owners, the 1962 figures for no memberships whatsoever being 71% and 50% respectively. Occupation, also used as an indicator of socioeconomic status in the previous report, was generally related to membership in voluntary associations in 1962 as in 1955. Precise comparability of the two sets of findings is not possible because the 1962 data refer only to the respondent, whereas in 1955 the reference was to the occupation of the head of the household. (This effectively eliminates about 40% of the 1962 sample, who are overwhelmingly women and predominantly housewives.) Nevertheless the general pattern holds; white-collar workers are more likely than blue-collar workers to belong to any voluntary association (unions ex-

cluded) and, when they do, they are more likely than manual workers to belong to several such associations. The proportions who report belonging to no organizations in 1962 were as follows: among professionals, 43%; among business proprietors and managers, 55%; among clerical and sales workers, 49%; among skilled laborers, 63%; among semi-skilled workers, 66%; among service workers, 69%; and among unskilled nonfarm laborers, 73%. A new indicator of status, the number of rooms in the dwelling unit, was also available in the 1962 survey. Among those who live in three rooms or fewer, 79% report no memberships; among those who live in four to six rooms, the corresponding figure is 59%; among those who occupy seven rooms or more, 40%. The same findings were documented by Hausknecht (1962) for the 1954 national Gallup survey, using a variety of indices of socioeconomic status (1962:Chap. II). Similar results were obtained by Lazerwitz (1962) from a 1957 national survey by the Survey Research Center, using a variety of indices. The NORC national survey in 1960, for Almond and Verba's (1963) study, also provides evidence of a substantial positive relation between voluntary association membership and socioeconomic status as measured by four indices: level of income, type of occupation, years of schooling, and interviewer's rating of respondent's socioeconomic class.

over this seven-year period, we observe that the growth of membership is not accounted for by a disproportionate increase in the prevalence of membership in the *higher* status groups. One model of the change might have been that the lower groups in the population continued to remain as uninvolved as ever but that the higher groups who have a greater inclination toward membership had intensified their previous pattern—thus widening the gap between the socioeconomic groups. The change has distributed itself all along the continuum, and, if anything, the sharpest growth appears to have occurred in the less advantaged.

This finding, apart from its salutary tone, assumes dramatic import, in light of a background fact. Those in 1962 who in *absolute* terms are equally poor and uneducated as the corresponding groups in 1955 are in fact worse off. In 1962, \$2000 bought less than earlier, and the ones in this income group, comprising (as the marginals will reveal) an even smaller segment of the population, were worse off in relative terms. They could well have been ground down and declined into total noninvolvement. However, by the same token, the changes among them cannot account for much of the national change. Because of their small numbers, an increase, for example, of 8% in memberships among those below the poverty line (\$3000) only produces a net shift in the aggregate of about 2%, because they constituted about 25% of the adult population in 1962.

Further reflection along these lines, however, suggests one of the paths leading to the national change. Although the sheer growth of membership among the poor may not contribute very much, the decline in their absolute *number* over the time period is also pertinent to this analysis. Since they continue to be the least likely to join the ranks of associations, any reduction in their numbers in the population as they move upward and increase the proportions of the non-poor is bound to produce some net rise in the membership of the national population. The several processes compounded their effects. The poor have become fewer, and those that remained in that unfortunate status were more prone to enter organizations.<sup>14</sup>

<sup>14</sup> Although the definition of poverty varies and the standard indices employed have been changed

Such a model, however, poses an intriguing problem. As poor people improve their status, can they quickly adopt a new pattern of membership? Perhaps they are rooted in the ways of their past, and it would take a considerably long time to become reoriented toward voluntary associations. We cannot speak to that specific question directly, but the 1962 survey fortunately provided unique and valuable information for the study of such processes of socialization into membership and for the understanding of the relation between stratification and voluntary associations.

The occupation of the father during the respondent's youth, and the formal education of both the father and the mother, were enumerated. If the experiences of being reared within a particular socioeconomic milieu governed the adult pattern of voluntary association membership, a difference should be revealed when respondents are contrasted in their social origins even when their own status is controlled. If the more contemporary situation governs, a difference should be revealed when respondents are contrasted by their own characteristics, even when their origins are controlled. Those who have been mobile, of course, have had some unknown but considerable amount of time to adjust to their status, but certainly their family of origin should leave its mark, if induction into membership in associations is truly a slow and gradual process. Table 5 presents the evidence.

The contrast between the influence of the *contemporary* status and the *historical* status of the family of origin is striking. Individuals from different socioeconomic milieus show the same level of voluntary association membership so long as they are themselves comparable in status. But those who had the same social origins nevertheless vary dramatically in their memberships if they themselves are at different levels in the social

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over time, one can infer from government sources that there was a considerable drop in the "incidence of poverty" in the population in 1955-1962. Between 1962 and 1969, the drop has been even more rapid, and by extension, one would anticipate an even greater trend toward membership, apart from new socio-political factors that might have intruded to alter the course. See *Economic Report of the President, January, 1969*, (154).

Table 5. % Who Are Members of Voluntary Associations, by Inter-generational Mobility, 1962.

	0	1	2+	N
Inter-gen. Occ. Mob. <sup>a</sup>				
FH-OH <sup>b</sup>	41%	23%	36%	151
FL-OH	53	20	27	186
FH-OL	69	24	7	72
FL-OL	66	21	13	267
Inter-gen. Ed. Pattern: F to O <sup>c</sup>				
FH-OH	48	21	31	335
FL-OH	49	23	28	446
FH-OL	66	23	11	108
FL-OL	66	22	12	495
Inter-gen. Ed. Pattern: M to O <sup>c</sup>				
MH-OH	48	22	30	407
ML-OH	49	23	28	388
MH-OL	64	23	13	128
ML-OL	66	23	11	517

<sup>a</sup> As previously noted, only the respondent's occupation was enumerated, and about 40% of the sample, overwhelmingly women, and predominantly housewives, are excluded from this tabulation, since they had no other occupation. In addition, respondents whose fathers were farm owners or who themselves were farm owners are excluded since the vexatious problem of classifying them as high or low on the basis of the data could be solved no better in this survey than in other surveys. "High" occupations include professionals, business managers or proprietors, clerical and sales workers. "Low" occupations include all other urban occupations and also farm laborers.

<sup>b</sup> Father = F, Mother = M, Offspring = O, High = H, Low = L.

<sup>c</sup> 377 respondents who reported that they did not know father's educational attainment are excluded from one table; 314 respondents who reported that they did not know mother's educational attainment are excluded from the other table. The cutting points on "high" and "low" education are different in *absolute* terms for the classification of parent vs. respondent. In both instances it is approximately a median cut. Grade school or less defines a low parent; high school or less defines a low respondent.

structure.<sup>15</sup> Admittedly, in one of the six tests of the influence of historical background when the respondent's own status is controlled, a difference is found. Coming from a background in which the father had a high occupation does add an increment to the memberships of the respondent, but only in the instance where the offspring himself also has a *high* occupational status. It is as if the parental contribution to membership can

only be free to enhance involvement when the respondent himself is released from the structural constraints that impinge on him as an adult. To rephrase the findings in the more conventional terms of social mobility, one might say that movers, whether their journey has been upward or downward, behave like the stayers in the status of their destination.

The tests give indirect support to the process implicit in our earlier formulation that changes in status can produce an increase in voluntary association membership, and that the individual is not held captive by his past. To be sure, we are not fortunate enough to have data within this survey on intracareer mobility and shorter-term changes.<sup>16</sup> We have other evidence, however, that works to buttress the general structure of findings. In addition to the social origins of the respondent, his ethnic origins were determined by questions as to the father's and mother's birthplaces. If this variable had made a difference in the association memberships of the offspring, it might simply have reflected other characteristics confounded with ethnicity. But instead we find that his origin, whether completely native American, completely foreign, or mixed (one parent native and one foreign), had a negligible effect on his membership in voluntary associations.

Thus it would seem that the social position the respondent has reached in adult life, his more recent condition rather than his distant origins, is consequential for his memberships.<sup>17</sup> It is reasonable, in this light, to

<sup>16</sup> A most valuable, and perhaps unique, set of findings by Babchuk and Booth (1969:31-45) is relevant: A four-year panel study of a sample of adults in Nebraska yielded evidence on changes in voluntary membership. Although the aggregate findings were stable, there was much turnover both in the direction of adding and dropping memberships—53% changing the number of memberships they had. The changes occurred mainly in the young individuals.

<sup>17</sup> Some ambiguity surrounds the findings presented in Table 6 because the milieu in which some three hundred respondents were reared could not be determined, since they did not report the education of one or the other parent. Perhaps they are influenced by their milieu, whatever its nature may have been. However, we can penetrate some of the obscurities surrounding the behavior of these individuals, despite their inarticulateness, by analysis of their other characteristics. As one might expect,

<sup>15</sup> Similar findings were obtained in a secondary analysis of a Detroit area survey (Curtis, 1959: 846-848).

believe that the general trend documented could have occurred partly through the improvement in the status of individuals. This is not the whole story, of course, since individuals of low status also seem to have a higher level of membership than previously.

There is a body of evidence that would appear difficult to reconcile with our findings. *Children* from families of higher status are considerably more likely to be members of youth associations than children from a lower class milieu.<sup>18</sup> However, these findings in juxtaposition with our own may simply document a most interesting *discontinuity*, and there is nothing logically incompatible about the two patterns. Before children have arrived at an independent status, and when they still are under the impress of the family,

they are predominantly older individuals. Very frequently they report that they do not know the education of both parents. They themselves tend to be less well educated than those respondents who do report the educational attainments of their parents. Consistent with this fact, they tend to have fewer memberships in voluntary associations, over 60% reporting no memberships whatsoever. If we assume that their family backgrounds are homogeneous, although unknown, and subdivide them in terms of their own educational attainments, we again find a positive relationship between their status and number of memberships. For them also, own status has a marked influence. To penetrate the obscurity further, we can also classify their parental milieu by using interchangeably whatever information might exist on the education of *one* of their parents. When we contrast those who have one parent of high education with those who have one parent of low education (for that smaller group who have made such reports), and control their own status, the difference in memberships is negligible.

<sup>18</sup> We draw upon a uniquely valuable body of findings from several studies of the Survey Research Center, including a survey of about 2,000 girls, 11-18 years of age, a sample representative of the national population enrolled in schools in 1956, and a parallel survey conducted in 1959 of about 1,400 boys, 11-13 years of age, representative of the population enrolled in grades 4-8. By a variety of indices, father's occupation, mother's education, possessions, etc., it was found that girls from higher status backgrounds were more likely to be members of associations. It is also relevant that the major reasons given for not joining were external obstacles and pressures, rather than personal feelings (Survey Research Center, *Adolescent Girls*, n.d. 158-159, 202-212; Survey Research Center, *A Study of Boys Becoming Adolescents*, 1960, 17-19, 24-25). One study in the literature also provides evidence on the problem for a specialized sample of adults. See Hodge and Treiman (1968:722-740).

their memberships reflect those influences and circumstances. But when they are removed from such conditions, and have arrived in a new social location, it then becomes governing and overrides the earlier influences. Certainly, it is unusual that early socialization, which has been found to be potent and persistent in its influence on many spheres of attitude and some realms of behavior, can be erased. Yet it may well be that joining and maintaining membership in associations is a very special type of behavior dependent less on internalization and inner directives and more on objective circumstances.<sup>19</sup>

The potency of the social position can be conveyed if we measure a *compound* of conditions in the respondent's current milieu. In the earlier table, respondents were classified each time by reference to only a *single aspect* of their status. Apart from reducing the reliability of the classification, this provided only a partial description of the totality of their circumstances. In Table 6, selected groups, clearly "low" and "high" in occupational status, are also classified by income, and their voluntary association memberships are examined. Those respondents for whom good fortune has been compounded have the highest level of membership.<sup>20</sup>

<sup>19</sup> We may try to gain further insight into these processes by introducing the age of the respondent in the analysis. Those who have been mobile and are still relatively young adults are closer in time and memory to the experiences in their families of origin than are the older individuals. The patterns we observed earlier in Table 5 should be differential if the process is slow in working its effects. The influence of parental and respondent status was examined separately for individuals under 35 years of age and over 35. This analysis revealed that even among the younger adults who are not that far removed in time from the milieu of their parents, social origins have relatively little influence on their memberships when their own status is controlled, whereas the respondent's status, when origins are controlled, has considerable effect on voluntary association membership, with a relatively greater effect among the older respondents.

<sup>20</sup> In our earlier paper, we explored the relation between various *situational* factors and voluntary association membership but were limited to data from a survey of Denver. Fortunately, the 1962 national survey contained data on a wider variety of situational variables, some measuring subtle features of daily existence which could illuminate one possible process that may underlie the relation

Table 6. Percent Belonging to Voluntary Associations by Occupation and Income, 1962.

Income Level	0	1	2+	N
Prof. & Bus.				
Above \$7000	47%	17%	36%	143
Below \$7000	53	25	22	111
Labor <sup>a</sup>				
Above \$7000	59	24	17	191
Below \$7000	72	19	9	342

<sup>a</sup>Skilled, semiskilled, and unskilled.

### *Differences and Trends among Racial Groups*

In our earlier paper, the analysis of both the 1953 and 1955 survey documented that whites were more likely to be members of voluntary associations. Certainly, it seems

between social status and membership; social status perhaps works its influence through altering the routine pressures upon the individual, and thereby facilitates or constrains his entry and activity in voluntary associations. Unfortunately, limitations of space prevent a full account of these findings here; they can be summarized as follows.

Situational factors that are in the nature of daily pressures, such as hours of work, available spare time, and commuting time, were found to have no systematic or major relationship to voluntary association membership; these findings corroborate those of the earlier study of Denverites. By contrast, variables that describe the respondent's situation within the *community* (e.g. his length of residence there or in the area, his expectations about moving) appear related to voluntary association memberships; this finding contradicts our earlier results from Denver. This contradiction between the local and national findings provides, belatedly, a cautionary note that replication and substantial data are the only safe bases for generalization. This caution led us to incorporate some additional tests from the 1960 Almond and Verba national survey as well. (Although it is unwise to use their survey as a 1960 point in our larger time series to measure aggregate or subgroup trends, for reasons given elsewhere, we can employ their data profitably as supplementary evidence for testing relationships and for comparisons of subgroups.) The findings on relations between community-type situational factors and memberships in the 1960 national data were generally consistent with those from the 1962 survey, although there were exceptions.

essential to replicate these comparisons to see whether the differences have persisted or whether there have been *differential* changes over time. The ideological currents that have run through the Negro population may have produced a sharp increase in memberships, and Negroes with time may have become released from the understandable fear of reprisals they anticipated, especially in some regions, for organizing, and thus increased their memberships.<sup>21</sup> The rise in their status may have worked, as demonstrated by our earlier analyses, to produce a corresponding increase in membership.

The analysis of the problem, however, must proceed very carefully, and the results we shall present should be regarded as tentative. Here, more than at other points in our analysis we regret the lack of current data, since our measurements may have been too early to catch the political tides that have begun to flow and the economic changes that have occurred. If it is reasonable to assume that the fears of Negroes may have inhibited their joining, it may also be reasonable to think that they may be inhibited in *reporting* their actual memberships, especially to white interviewers in the South, and this procedural variable and source of error must be controlled in the comparisons. Fortunately, in the 1955 survey, all Negro respondents in the South were interviewed by Negro interviewers; and in segments of the North which were clearly known to be segregated, Negro interviewers were also assigned. The same procedure was applied in the more recent NORC surveys on which we shall draw.

The way in which the logic of controls and the concept of spuriousness should be applied to such comparisons must also be considered very carefully. We have *not* controlled socioeconomic status. A configuration of handicaps is part of the distinctive social plight of American Negroes. To compare them with that unusual group of whites who are characterized by the same handicaps or disadvantages would hardly present a faith-

<sup>21</sup> A most interesting documentation of this pattern for one Southern city is presented by Ross and Wheeler (1967:583-586). For the atmosphere of threat surrounding membership in "political" organizations in Mississippi in 1960, see Matthews and Prothro (1966:204 ff.).

ful description of social reality, although it could provide clarification of processes that underlie or account for the different patterns of membership observed among Negroes and whites.<sup>22</sup>

One must also be concerned about the small size of the Negro samples in national surveys, and the lurking danger of biases creeping into the best designed samples as a result of difficulties in completing the requirements of the design in the Negro stratum.<sup>23</sup>

In trying to estimate the differences in membership by race, and whether the patterns previously observed have remained stable or changed, we take some comfort in the knowledge that (1) the survey procedures seem adequate to the specific task; that (2) comparability was maintained in (a) the way the samples operated and (b)

the way interviewers were assigned, and even in (c) other respects that one neglects at some risk, such as the definition and classification of the race of the respondent. And we have tried to buttress the evidence on trends by using both the 1953 to 1958 comparison and the 1955 to 1962 comparison. Nevertheless, we shall see that there are grounds for caution, and also contradictions in the supplementary sources of evidence.<sup>24</sup>

Examination of both pairs of surveys in Table 7 suggests that there has been a sharp increase in the memberships of Negroes. In the instance of the trend from 1955 to 1962, the differential in membership has, as a result, almost vanished, whereas the comparison of 1953 and 1958 still reveals a difference, albeit smaller, with whites more likely to report such memberships (including unions), and especially to show a pattern of multiple membership in associations. Whether the variations reflect the temporal unfolding of a process (the 1958 data catching the rise in Negro membership before it has accelerated to the point reached some four years later) is difficult to say. In part, the results may reflect the variation in the definition of the concept, the one set of data including union membership and nonlocal associations and a different unit of description.<sup>25</sup>

<sup>22</sup> As would be expected, Negroes in our sample have much lower incomes and lower educational attainment, factors that have been shown to decrease membership. In this light, some of our trend findings without controls may seem all the more dramatic. For analyses, controlling SES, see Orum (1966) or Olson (1970). Apropos our argument, Olson observes that the effects of component status factors are additive.

<sup>23</sup> Whatever gross tests can be made suggest that the 1962 sample functioned well. The obvious bias would be in the direction of underenumeration of young men in the Negro population, they being difficult to locate and interview. Such a bias, if it occurred, seems not to impair the comparisons in the 1962 survey. Among Negroes, 47% of the respondents were male and 19% of these were ages 21-27. Among whites, 44% of the respondents were males, and 13% were 21-27. Refined estimates based on the 1960 Census (corrected for errors in the enumeration of the Negro population) indicated that 49% of the *adult* Negro population were male, of whom 17% were in the age group 21-27 (Bogue *et al.*, 1964:343 ff. Tables 2A and B). In the context of this discussion, it may be worth noting that Hausknecht, on the basis of a Gallup 1954 national survey, and Lane, on the basis of a national 1952 Michigan survey, reported no difference in voluntary association memberships for Negroes and whites, whereas our 1953 and 1955 estimates indicated a substantially lower level of membership among Negroes. The Lane findings were based on a post-election subsample which comprised fewer than 550 respondents and the Negro group numbered 54 cases. The Gallup Survey involved a much larger sample which yielded 164 Negro respondents, but there is no indication as to the nature of the design, possible biases due to noncompletion or the types of interviewers assigned to Negro respondents (Hausknecht, 1962).

<sup>24</sup> We do not have in mind contradictions with studies of specialized universes in given communities or localities or urban areas, or the evidence from studies where it is impossible to determine what types of interviewers were employed, or where the results are a matter of definition of concepts, e.g., church membership being treated in some instances as membership in a voluntary association. The 1964 data reported by Marx are based on a very large sample of over 1000 Negroes interviewed by Negro interviewers in 1964, and are most valuable data, but are not comparable because the universe was restricted to metropolitan areas outside of the South, supplemented by samples of two Southern cities. And the index of membership "excludes church groups and civil rights organizations" (Marx, 1967: especially 70-71).

<sup>25</sup> The substantial number of cases in the 1958 survey whose memberships could not be ascertained creates some indeterminacy in the findings. As previously noted, these cases seem mainly to derive from clerical error and do not represent ambiguity as to the exact type or number of memberships. Thus one would expect them to be distributed at random and not to change the estimates. Yet it is hard to account for the greater proportion of such cases in the Negro sample. However, if we distrib-

Table 7. Trends in Voluntary Association Membership among Negroes and Whites.

Race	% of Respondents Who Belong to Community Associations								% of Families Who Belong to Associations Including Unions							
	1955				1962				1953				1958			
	0	1	2+	N	0	1	2+	N	0	1	2+	N	0	1	2+	N
Negro	73%	18%	9%	229	60%	22%	18%	202	60%	29%	11%	279	45%	39%	16%	190 <sup>a</sup>
White	63	20	17	2139	57	22	21	1529	46	31	23	2472	37	31	32	2489 <sup>a</sup>

<sup>a</sup>Excludes 29 Negro and 220 white respondents, respectively, for whom memberships in voluntary associations were not ascertained.

Because of the small size of the Negro samples and the many methodological difficulties that might affect these comparisons, the membership rates of Negroes and whites were also compared on the basis of the 1960 survey data of Almond and Verba.<sup>26</sup> It should be added that the sex distribution for Negro and white respondents was almost identical, almost 50% of each group being male. Negro interviewers were allocated in the way previously described. The data are presented in Table 8.

The 1960 evidence still reveals a difference, Negroes being less likely to have membership in any association (excluding unions) and whites more likely to show multiple memberships.<sup>27</sup> That there was a differential

ute these cases on the basis of other assumptions, the comparisons of Negro and white memberships and trends from 1953 to 1958 reveal the same basic findings.

<sup>26</sup> The 1960 survey for Almond and Verba was conducted by NORC and the sample design paralleled the 1962 survey. The completion rate was 83%. The question used to determine memberships was also an open-ended question which came at the end of the interview, and we can assume that the interviewers handled it in the same general way as in the earlier surveys. In our computation of memberships, we have excluded individuals in their sample who were under 21 years of age and we have not counted union membership, so as to maintain those aspects of equivalence to the 1962 analysis. Despite all these points of comparability, however, important differences remain. The question about memberships, in its exact phrasing and mention of many examples, might have heightened recall. And perhaps even more important, the content of the long series of prior questions was such as to make membership salient and prestigious; thus the figures would be inflated over those of 1955 and 1962.

<sup>27</sup> In interpreting these findings, it may be noted that as in the other surveys, the Negro sample in

trend and a sharper increase in Negro memberships seems to us beyond doubt on the basis of the double test presented in Table 8. Whether the difference between Negroes and whites has almost vanished remains uncertain, although one might argue that the social pattern in 1960 had not yet unfolded to the point it reached by 1962. Obviously, what is in order is further pursuit of this process in the years to come, which would serve not merely to reduce this particular uncertainty but to measure aggregate and other subgroup trends and possible future changes in the other social determinants of membership.<sup>28</sup>

Table 8. Percent of Respondents Belonging to Voluntary Associations by Race, 1960.<sup>a</sup>

Race	0	1	2+	N
Negro	59%	19%	22%	96
White	48	22	30	834

<sup>a</sup>Excluding unions.

1960 was less well educated; of lower income and in lower occupations than the white sample. For these findings, see two other secondary analyses of the Almond and Verba survey (Marvick, 1965: especially 116; Hyman and Reed, 1969: especially 349). In Indianapolis as late as 1968, Olson found that whites were more likely to be members (including unions) when SES was *not* controlled.

<sup>28</sup> A similar case can be made for special attention to other subgroups in future surveys. Religion serves as an example. We are unable to trace trends in voluntary association memberships among various religious subgroups because insufficient cases are captured in one national probability sample. For Catholics and Protestants as a whole, 60% of the former and 56% of the latter had no voluntary

## THE MISSING YEARS: 1962-1970

The course of the process of voluntary association membership from 1962 to 1970 may remain a mystery, unless there are series of fugitive but comparable national surveys which remain to be unearthed and analyzed. The general consistency of the findings from our several trend analyses does give us some confidence in extrapolating and conjecturing that membership gradually increased in the sixties, and that those groups who formerly had not forged their strength into collective forms have increasingly begun to do so. That huge numbers would have remained outside the boundaries of voluntary associations even until recently seems to us also true, in light of one national set of data which provide compelling evidence. Unpublished data kindly made available to us by Verba and Nie are based on a large national survey conducted in 1967 by the National Opinion Research Center. The sample design is not strictly comparable to the other surveys we have presented and might have affected the estimates in some unknown but minor way. And the instruments employed to measure membership were markedly different, a card containing 15 different types of associations being presented to the respondent, with the interviewer asking specifically about each, and then asking a supplementary open-ended probe. Certainly this may have worked to increase the estimates of membership over the level obtained by the techniques of the earlier surveys, and the prior context of questioning might also have elevated the salience and prestige of associations. Our 1962 findings showed that 57% of adults in the nation had no memberships and that only 4% reported memberships in four or more associations, excluding unions. The corresponding figures in the 1967 survey were 46% with no memberships and 6% with four or more memberships. The increase in that recent five-year interval seems real but modest, when one takes into account the respective measuring instruments.<sup>29</sup>

association memberships as measured in the 1962 survey.

<sup>29</sup> A tabulation by race reveals the following: 44% of whites report no memberships, whereas 61% of Negroes report no memberships, which adds further obscurity to the question of these subgroup comparisons and their trend.

## CONCLUSION

We have traced trends in the voluntary association memberships of American adults between the mid-1950's (the time of our previous study) and the early 1960's, by means of secondary analysis of national survey data from studies that are methodologically comparable. Data from other, somewhat less comparable, national surveys have been used to supplement these findings and to provide indications of developments up to 1967. In addition, analyses were made of trends among certain subgroups and of selected social correlates of membership.<sup>30</sup> Our major conclusions, subject to the qualifications specified above, can be summarized as follows: (1) Voluntary association membership is not characteristic of the majority of Americans (a finding originally from data in the 1950's, now confirmed by data from the 1960's). (2) A relatively small percentage of Americans belong to two or more voluntary associations (another finding from the earlier study, confirmed by the new data). (3) There was a small but noteworthy increase in voluntary association memberships between the mid-1950's and the early 1960's. (4) The trend toward more membership in associations was not caused by the cohort who came of age during the period from 1955-1962, the two points in the study. (5) Membership is directly related to current socioeconomic position, as measured by a variety of indicators, (a relationship established in the earlier study, confirmed by data from the 1960's). (6) The trend toward increase in associational memberships is not confined to the more well-to-do strata of the population, but occurs all along the line and especially among those of poorer economic means. (7) Current economic situation appears to have more effect upon membership than does one's station of origin. (8) The trend toward increased membership applies to both Negro and White adults but is somewhat more evident among the former, thereby tending to reduce previous subgroup dif-

<sup>30</sup> An analysis of the functions of voluntary association membership, such as was presented in the earlier study for 1955, will be the subject of a separate treatment at a later date. The need to give attention to the nature and correlates of multiple membership, which we term the "multiple joining syndrome," will also be met in that paper.



ferences in membership. However, these findings are most tentative because of the small number of Negro respondents found in each sample.

We present the above findings as bearing on a social phenomenon of interest and importance in its own right. But we also hope that this secondary analysis, in its strengths and limitations, will suggest the urgent need to establish more frequent, systematic, and comparable national surveys to provide varied social indicators for the study of trends and social change. Lacking such data, there will remain large gaps in our knowledge of social change and our ability to project future trends.

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# THE SOCIOECONOMIC ACHIEVEMENT OF WHITE RELIGIO-ETHNIC SUBGROUPS: SOCIAL AND PSYCHOLOGICAL EXPLANATIONS \*

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*American Sociological Review* 1971, Vol. 36 (April):207-222

*In longitudinal data for the decade 1957-67, the socioeconomic achievement of white metropolitan native males from five religio-ethnic backgrounds is examined. Jews, regardless of ethnic ancestry, attain higher levels of education, occupation, and income than all other subgroups, while Roman Catholics of Italian and Mexican heritage achieve the lowest levels. Controlling statistically for social origins reduces the gross differentials by about one-third; no net effect of religio-ethnic affiliation remains during the ten-year period after both social origins and prior achieved statuses are controlled. Thus, there is no evidence of occupational and income discrimination on purely religious or ethnic grounds. Contrary to current emphasis in the social psychology of religio-ethnic achievement, achievement-related work values and motivations of adults are neither key intervening variables nor do they influence the process of stratification to a substantial degree. The most important variable in explaining the differential socioeconomic achievement of the religio-ethnic subgroups is education, after the variation owing to the handicaps and benefits of social origins has been removed statistically.*

**D**IFFERENTIALS exist in the attainment of occupational and economic statuses in the United States by white religious and ethnic subgroups; data suggest that Jewish and Protestant religious subgroups achieve higher socioeconomic levels than do Roman Catholics. Lenski (1963) reports a series of data collected in the metropolitan Detroit area between 1952 and 1958 for white males in which the father-son vertical occupational mobility of Protestants exceeded that for the Catholics. While the data for Jews are not reported in detail, Lenski affirms "their rapid rise in the economic system," an experience closer to that for white Protestants than for Catholics. Citing Weller's (1960) analysis of the Detroit data, Lenski concludes that the Protestant-Catholic differences in occupational mobility cannot be attributed to national origins or paternal social status. Moreover, the time-series

data for Detroit reveal no decline in the religious differentials.

Goldstein (1969), Gockel (1969), and Warren (1970b) have refined the understanding of religious subgroup achievement. Warren's (1970a) review of recent studies documents that while Roman Catholics achieve at a socioeconomic level below an undifferentiated classification of Protestants, in a detailed classification of major faiths and Protestant denominations, Roman Catholics attain higher social statuses than do some denominations.

The Duncan and Duncan (1968)<sup>1</sup> study of national origin differences in educational and occupational achievement demonstrates substantial gross differences among ethnic categories. These differences, however, are reduced by about one-third when the national origin groups' social origins (paternal occupational status, education, and size of family of orientation) are rendered equivalent through statistical controls. For white native sons of foreign fathers with equivalent social origins, highest educational achievement is associated with Russian paternal origins, while the lowest educations are attained by the sons of Latin Americans. As

\* The research for this paper was supported by grant number 91-24-69-13 from the Manpower Administration of the U.S. Department of Labor pursuant to the provisions of the Manpower Development and Training Act of 1962. The author thanks Robert M. Hauser, James R. Sorenson, and Robert P. Althaus for their comments on earlier drafts, and Otis Dudley Duncan, Joseph Veroff, Paul Siegel and Howard Schuman for their advice during the year in which this research was conducted.

<sup>1</sup> Estimates are derived from a sample (Blau and Duncan, 1967) of the native nonfarm population, ages 25-64 in 1962.

for occupational achievement, the range of national origin groups is bounded at the lower end by sons of Latin Americans and at the upper end by sons with paternal ethnic ties to northwestern Europe (with the exception of Ireland and Germany).

In attempting to account for the observed ethnic differentials in achievement, the Duncan and Duncan research employs a multiple regression model and a social structural argument. Nearly one-third of the gross variation in the dependent variables is associated with the socioeconomic background and the number of siblings in the family of origin. The remaining net effects of ethnic subgroup membership on level of occupational status achieved are partitioned into two components. The first component represents the indirect effects of national origin background on occupational status through differential access to and completion of years of formal schooling. The remaining set of net effects measures the direct influence of ethnic background on occupational status (after social origins and educational achievement had been controlled statistically). These latter direct effects are interpreted as a measure of occupational discrimination (positive and negative) between persons of varying ethnic ancestries, but with similar social origin statuses and educational qualifications.

The mode of research applied to the national sample of males by the Duncans and their interpretation of the data differ from earlier social psychological work by Rosen (1959) with school-age youngsters in the New England region. Rosen explores the relationship between the "achievement syndrome" and the vertical mobility of ethnic minorities, whose mobility differentials are assumed rather than demonstrated.<sup>2</sup> Rosen proposes to explain his assumed socioeconomic achievement differentials by showing that Jewish boys score higher than boys of Southern Italian extraction on the com-

ponents of the achievement syndrome: TAT protocols scored for nAch (Atkinson, 1958), achievement-related values, and achievement aspirations. Upon finding a parallel between the ranking of boys on the assumed rates of vertical mobility of their ethnic subgroups and the ranking of the achievement syndrome, Rosen concludes somewhat tentatively that motivational orientations might account for the differential socioeconomic achievement of religio-ethnic subgroups. Coupled with Crockett's (1962) data, linking upward occupational mobility with above average scores on achievement motivation for a national sample of men, Rosen's work has encouraged many students of social stratification to accept this social psychological interpretation for the emergence of religio-ethnic differentials in achievement.<sup>3</sup>

Another social psychological explanation for differential minority socioeconomic attainments were advanced by Lenski (1963), whose data for religious affiliation groups demonstrate differential rankings of a set of work-related values. Rosen's and Lenski's researches—along with most other social psychological studies that inquire into the relationship between religious and ethnic subgroup background, motivations, values, and achievement behavior such as occupational mobility—have been conducted as a series of univariate analyses. Researchers investigate subgroup variation in actual achievements and on a set of personality instruments. Where the ranking of the subgroups is parallel on both dependent variables, the investigator is usually satisfied that the psychological traits or dispositions account for the observed differences in actual achievement.

The present analysis avoids the pitfalls of this approach by constructing a multivariate model of the process of religio-ethnic socioeconomic achievement in which the personality dispositions are included as intervening variables. The inclusion of motivational variables into the analysis will permit an assessment of the following propositions

<sup>2</sup> See Duncan and Duncan (1968:356-357) for a discussion of the probable inaccuracy of Rosen's assumed order of ethnic subgroups by achievement. That Duncan and Duncan employ origin and destination statuses in a regression analysis of achievement, rather than examining social mobility per se, does not vitiate the comparison of the formers' work with Rosen's. For a methodological treatment of the status variable-mobility variable issue, see Blau and Duncan (1967:194-199).

<sup>3</sup> Although Rosen and others (in Crockett, 1966) are frequently cited in support of a social psychological mechanism for social mobility, his caveat about overgeneralizing his "tentative" findings, as footnoted in Rosen *et al.* (1969:151), is regularly omitted.

which are suggested in the work of Lenski and Rosen: (1) religio-ethnic variation in achievement orientations toward work accounts for the gross socioeconomic differentials; (2) achievement-related motivations account for the net religio-ethnic differences in occupation and income, among men of similar social origins and education; and (3) for men of equivalent religio-ethnic background, achievement-related motivations function as intervening variables between social origins and socioeconomic achievements. One objective of the research reported below is to examine religio-ethnic differences in occupational and economic achievement. A second purpose is to assess among adult males, the tenability of the social psychological explanations of subgroup attainments which emphasize religio-ethnic patterns of achievement-related motivations and values. The latter interpretations of variations in attainment compete with social structural explanations which are couched in terms of religio-ethnic discrimination in the educational system and the labor market.

### *Data and Methodology*

This investigation is based on a secondary analysis of longitudinal data from the Princeton Fertility Study (PFS) (Westoff *et al.*, 1961). Data pertain to native white, two-child families residing in seven of the largest SMAs in 1957 (New York, Philadelphia, Pittsburgh, Detroit, Chicago, Los Angeles, and San Francisco). Only a subset of this stratified random sample is selected for analysis; viz., those males in the data for the duration of the restudy period (1957-67) and who had returned an attitudinal questionnaire in supplement to the original interview of the wife in 1957. This subset includes 715 males and represents 88% of the total remaining in the sample in the follow-up period. These men were approximately thirty years of age at the initial interview. Analysis reported elsewhere (Featherman, 1969) indicates that the subset provides unbiased regression estimates of relationships for the total sample ( $n = 1165$ ), at least at Panel I. Follow-up interviews (Panels II and III) occurred in 1960 and in the interval 1963-67.

From information on nationality back-

ground and 1957 religious affiliation, the following religio-ethnic categories are created (see Featherman (1969) for details of construction and of the derivation of category labels):

1. Jewish, all ethnic (N=88)
2. Anglo-Saxon Protestant (N=121)
3. Protestant, Other (N=142)
4. Roman Catholic, except Italian and Mexican (N=216)
5. Italian and Mexican Roman Catholics (N=100)
6. None or Other Religion and Nonresponse (N=48)

Since the cross-tabulation of cases by nativity and religio-ethnicity yielded some small cell frequencies, the reporting of separate analyses by parentage category seems inadvisable; the parentage variable did not appear to differentiate the religio-ethnic subgroup findings in preliminary work (Featherman, 1969). Furthermore, lacking any indicator of the degree of religio-ethnic identification for these native males, one does not know the extent to which any subcultural influences (say, of child rearing) are associated with the religio-ethnic labels. The effect of these missing data on the following analysis is unclear.<sup>4</sup>

The religio-ethnic categories permit one to examine the hypothesis that major religious groups can be differentiated by nationality background in their occupational and economic attainments. From previous theory and research, one could postulate that white Jewish males, regardless of ethnic background, attain the highest socioeconomic levels. Anglo-Saxon Protestants might be second, possessing an advantage of ethnic background which Other Protestants fail to en-

<sup>4</sup>One might argue that in the native population, of which a small proportion were born of foreign parentage, the special analysis of second-generation subpopulations is less representative of the experience of the larger religio-ethnic subgroups. At the same time, it may be only those individuals who identify with their subcultural heritages for whom religio-ethnic background differentiates achievement-related behavior. Strong religio-ethnic identification might be expected more frequently among those with foreign parentage, or with a direct tie to the subculture. Again, with fewer in the population having such parentage, the lack of these data may not severely impair the analysis. However plausible, this is an unsubstantiated assumption.

joy. Thus, Other Protestants could comprise the third category in the ordinal level of socioeconomic achievement. Roman Catholics, too, could be differentiated by nationality background, with Italian and Mexican Catholics presumably providing the lower boundary to the distribution of religio-ethnic achievement. Roman Catholics other than those of Mexican or Italian stock, one might postulate, will achieve above the level of Italian and Mexican Catholics, either equal to or slightly below the attainments of the Other Protestants.

The basis for differentiating between the two Protestant subgroups is rooted in the observation of Duncan and Duncan (1968) that men whose origins can be traced to northwestern Europe (except Ireland and Germany) appear to enjoy preferential treatment in the job market which advances their relative position beyond that which could be expected from their social origins and educational achievements. Italian and Mexican Catholics are distinguished from other Roman Catholics, although in the Duncan and Duncan research men with Italian and Mexican origins experience slightly different patterns of education, but both subgroups fall below the sample mean for occupation. Mexican stock (a surrogate herein for Latin American) was associated with severe handicaps in the occupational market which were only in part a function of educational underachievements, while men from Italian backgrounds were not as restricted by their (better) educational attainments. Still, one can expect these Italian and Mexican Catholics to underachieve vis-a-vis the other Roman Catholics and to comprise the lowest achieving religio-ethnic minority; this is an assumption consistent with those in the Rosen (1959) study, where Southern Italians were presumed to experience the lowest vertical mobility of the white minority groups he considered. Also consistent with Rosen's assumptions is the hypothesis that Jews of all nationality backgrounds will achieve the greatest socioeconomic success. If the Russian national origin subgroup in the Duncan research is equated with the Jewish minority status, then one would predict considerable achievement for native men from such backgrounds.

In sum, the religio-ethnic categories will permit an examination of the assumptions and conclusions cited in previous research about the relative socioeconomic success of these subgroups in American society. Each category will enter the analysis below as a dummy variable. A given man's membership in his religio-ethnic subgroup will be indicated by a score of unity; his lack of membership in the other categories will be indexed by a score of zero. These dummy variables for religio-ethnic background then will be included in a multiple regression analysis for the following dependent variables: education, occupation and income at Panel I, and occupation and income at Panel III. Aside from the categories of the qualitative variable, the regressions will include three other independent quantitative variables which are indexes of social background statuses: father's occupation, size of family of orientation, and extent of farm or rural residence of the husband.

All occupational variables are scaled in eleven 5-unit categories of the 1947 NORC prestige metric (NORC, 1947), with each respondent's score corresponding to the midpoint prestige value for the interval into which he was coded. Income refers to total dollars in salaries and wages of the man in the year preceding that of the interview. Income at the third interview (Panel III) was adjusted to the 1967 dollar units according to a procedure detailed elsewhere (Featherman, 1969), owing to the extension of the final interviews over a five-year period.<sup>5</sup> Education is years of formal schooling completed at the time of initial interview (1957) while size of the family of orientation is the number of siblings in the man's family. Extent of farm or rural residential influence denotes the degree of exposure to urban or to rural and farm environments during respondent's rearing, and to the influences of his parents who themselves were of urban, farm or rural origin.

<sup>5</sup> The Consumer Price Index was used to standardize Panel III dollar units. This variable number of years between Panels II and III has not proven to be identifiably critical for the determination of occupational statuses at the terminal interview, and the effect on the variation of adjusted income in Panel III is slight (Featherman, 1969:48-54).

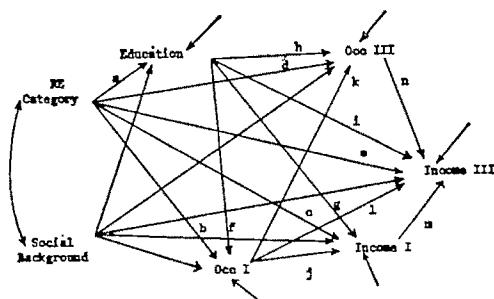


FIGURE 1. DIAGRAM OF DIRECT AND INDIRECT EFFECTS OF RELIGIO-ETHNIC (RE) SUBGROUP MEMBERSHIP ON SOCIOECONOMIC ACHIEVEMENT

### *Gross and adjusted differentials in educational, occupational, and economic achievement*

This and subsequent analyses are based on a causal model similar to Figure 1. Social background variables and the religio-ethnic variables are assumed to be predetermined;<sup>6</sup> the remaining achievement variables are arranged in a recursive system of equations, with achievement variables ordered temporally.

Table 1 records gross and net (adjusted) deviations of the religio-ethnic categories' means from the grand means for education and the socio-economic achievement variables throughout the follow-up period. Columns of net effects for religio-ethnic background, social background, and prior achieved statuses on the dependent variables are equivalent to regression equations with

<sup>6</sup> Current religious preference (Panel I) is used as an indicator of religious background, since in these and other data (Warren, 1970b) about 90% of the men retained their "background" religious preference at the time of or recently after marriage; the author erred in not obtaining the variable, religion in the home of rearing, from the original investigators. Given the high stability of religious preference and the similar pattern of effects of the original and current preference categories on the achievement variables (Warren, 1970b), the causal model adduced in Figure 1 remains plausible. Only those direct paths from causally antecedent to consequent variables are included in Figure 1 which previous analysis (Featherman, 1969) has demonstrated to be significant statistically (using a criterion of twice the absolute size of the standard error of the regression coefficient). However, all paths from social and religio-ethnic origins to consequent variables have been retained for this analysis.

their partial (metric) coefficients. In Table 1 our primary concern is the extent to which the gross deviations are reduced when adjustments are introduced by controlling statistically for social background differentials and then for both social origins and previously achieved statuses. The direct and indirect effects of the social background and prior achieved statuses on socioeconomic achievements between 1957-67 are subjects for separate treatment in earlier sources (Featherman, 1969; 1971).

One notices in the columns of gross effects for education and the occupation and income variables that the assumed order of relative achievements of the religio-ethnic subgroups is confirmed. Jews, regardless of their ancestry, attain more years of schooling, higher levels of occupational prestige, and greater salaries and wages than all other subgroups. The Jews and Protestants of Anglo-Saxon descent lie above the grand means of the respective achievement variables, and with the exception of the income for the Other Protestants at the initial panel, all other subgroup effects deviate negatively from the grand means. In terms of gross differentials in achievement, then, the Jews surpass all other religious groups, followed by the Protestants and the Roman Catholics, in that order. However, Protestants of Anglo-Saxon heritage obtain more education and socio-economic status than all other Protestants, and the Italian- and Mexican-American Roman Catholics achieve lower levels in the metropolitan stratification systems than their Catholic counterparts from other ethnic backgrounds. In short, national origin background does appear to differentiate the gross socioeconomic attainments of major religious groups in these metropolitan data.

When one equates the religio-ethnic subgroups on the three social background variables, the gross deviations in educational achievement decrease by about one-third, the remaining differences representing 0.6 standard deviation. Gross differentials in occupational and economic achievement remain about the same over the restudy period; approximately 0.8 standard deviation separates the highest from the lowest occupational prestige means, and about 0.6 to 0.7 standard deviation separates the income

Table 1. Gross<sup>a</sup> and Net<sup>b</sup> Religio-ethnic, Social Background, and Prior Achieved Status Effects on Successive Socioeconomic Achievements for Princeton Subset.

Independent Variables	Socioeconomic Achievements													
	Education		Occ-NORC I		Income I		Occ-NORC III		Income III					
	Gross (1)	Net (2)	Gross (3)	Net (4)	Net (5)	Gross (6)	Net (7)	Net (8)	Gross (9)	Net (10)	Net (11)	Gross (12)	Net (13)	Net (14)
Religio-ethnic Background:														
Jewish, all ethnic	1.60	1.15	1.05	.79	.28	1580.	1414.	713.	.85	.57	-.14	2986.	2292.	325.
Anglo-Saxon Prot.	1.08	.72	.84	.57	.25	617.	359.	-149.	.72	.44	-.08	1827.	1185.	-31.
Prot., Other	-.21	-.26	-.20	-.25	-.14	95.	35.	258.	-.21	-.26	-.03	-495.	-592.	-97.
R.C., exc. Ital. & Mex.	-.52	-.30	-.37	-.20	-.07	-583.	-425.	-247.	-.39	-.21	-.03	-603.	-195.	350.
Ital. & Mex. R.C.	-.89	-.50	-.64	-.36	-.15	-789.	-552.	-227.	-.42	-.13	.21	-1956.	-1272.	-448.
None/Other Rel., NR ethnic	-.71	-.69	-.40	-.41	-.11	-442.	-483.	-113.	-.15	-.16	.22	-1588.	-1608.	-710.
Social Background:														
Father's Occ-NORC	--	.31	--	.23	.10	--	199.	-5.	--	.24	.03	--	567.	38.
No Siblings	--	-.25	--	-.15	-.04	--	-116.	19.	--	-.16	-.02	--	-373.	-25.
Farm/Rural Background	--	-.05	--	.01	.03	--	55.	52.	--	.00	-.00	--	-37.	-62.
Prior Achieved Statuses:														
Education	--	--	--	--	.44	--	--	15.	--	--	.01	--	--	34.
Occ-NORC I	--	--	--	--	--	--	--	870.	--	--	.90	--	--	383.
Income I	--	--	--	--	--	--	--	--	--	--	--	--	--	0.
Occ-NORC III	--	--	--	--	--	--	--	--	--	--	--	--	--	1583.

<sup>a</sup> Gross deviations about the grand mean.<sup>b</sup> Net or adjusted deviations; partial regression (metric) coefficients.

means. Standardizing the religio-ethnic subgroups for social origin reduces the range of differences at Panel I by 40% for occupation and 20% for income, and at Panel III by 50% for occupation and 30% for income.

While the variance in religio-ethnic educational achievement is reduced, not all is accounted for by social background differentials; testing the model in Column 2 of Table 1 against a model (not shown) in which only the three social background variables are used as independent variables produces a statistically significant F-ratio ( $F_{8,708} = 6.406$ , which is significant at  $\alpha = .01$ ). This finding is consistent with Warren's (1970b) data on religious group and denominational achievement.

Adjusting educational and socioeconomic differentials for social background leaves the ordinal positions of the subgroups essentially unaltered, despite the reduction in the variances in the achievement variables. In part this arises from the higher status origins of both the Jews and the Protestants, relative to the Roman Catholics, and the smaller sibships of the former two subgroups than of the latter; higher paternal occupational status and smaller numbers of siblings are positively associated with educational, occupational, and economic achievements (Blau and Duncan, 1967; Duncan *et al.*, 1968). In addition, the zero-order relationships between the residential background variable and the achievement variables are small, and in these metropolitan data all the effects of farm or rural residential origins on achievements are expressed indirectly through the handicaps incurred from lower socioeconomic origins and larger numbers of siblings (Featherman, 1971).

If one adjusts the gross differentials in subgroup achievement for both social background and prior socioeconomic attainments (where education is antecedent to occupational achievement, which in turn precedes economic achievement) and tests the sums of squares in the occupation and income variables at Panel I which are attributable to the religio-ethnic classification, one obtains non-significant F-ratios in both instances (Table 1, Columns 5 and 8). A similar finding applies to achievements at Panel III. Thus, there remain no statistically significant net

religio-ethnic differentials in occupational or economic achievement during the restudy period. The gross differentials are explicable in part by the handicaps and benefits of social origins, by the various amounts of formal schooling obtained, and by early career attainments. Since the net effects of religio-ethnic background on socioeconomic achievement at Panels I and III are not significant, white religio-ethnic minorities in metropolitan stratification systems apparently do not suffer discrimination on purely ethnic or religious grounds in the competition for jobs or wages; this state of affairs differs from reported ethnic discrimination against black Americans (Duncan, 1967).

#### *Direct and indirect effects of religio-ethnic background on achievement*

Gross differentials in educational achievement for the religio-ethnic subgroups can be understood as the products of the relative handicaps and benefits of social origins, but for men of equivalent social backgrounds there remain subgroup-specific experiences (socialization and/or discrimination) which affect the length of formal schooling. Adjusting socio-economic career achievements for both social origins and education essentially accounts for all of the gross occupational and economic differentials during the follow-up period. This is to say that there are no statistically significant net or direct effects of subgroup background on the occupational and income variables (paths *b*, *c*, *d*, and *e* in Figure 1), while, net of social origins, there is a direct effect of religio-ethnic background on educational achievement (path *a* in Figure 1).

The implication seems that the effects of religio-ethnic background are mostly indirect, except for educational achievement, where they bear directly on attainment as well as being distributed indirectly through their correlation with social origin variables. The precise nature of the indirect effects on the occupational and economic variables are illustrated in Table 2, which is constructed from the model in Figure 1 and the coefficients in Table 1. In order to separate the indirect effects of religio-ethnic background on achievement from those owing to the correlation of the two sets of background vari-



ables, the analysis holds constant father's occupation, number of siblings, and extent of farm or rural influence.<sup>7</sup>

That the indirect effects (net of social origins) are larger than the direct effects in Table 2 is expected from the foregoing analysis. In addition, a decomposition of the indirect effects demonstrates that the educational component is the most important source for the expression of net religio-ethnic differences for all subgroups except for the Other Protestants (not Anglo-Saxon). This, too, results from the statistical nonsignificance of the paths *b*, *c*, *d*, and *e* in Figure 1, and the significance of path *a*.

In summary of Tables 1 and 2, the gross differences in occupation and income for religio-ethnic subgroups is in part a matter of the handicaps and benefits of socioeconomic background. The remaining variation is primarily explained by religio-ethnic, category-specific educational achievement and then by early career, socioeconomic attainments. Aside from these indirect expressions of religio-ethnic background (e.g., the differential access to education, net of social origins), the data indicate no statistically significant direct effect of the categorical variables, and, therefore, no discernible socioeconomic discrimination on purely religio-ethnic grounds. There are net religio-ethnic differentials in educational achievement after

adjusting for social origin effects, indicating the possibility of religio-ethnic discrimination in the distribution of education (Duncan and Duncan, 1968) and/or the operation of differential orientations toward educational completion (Rhodes and Nam, 1970), which in some sense are independent of socioeconomic context of rearing.

#### *Religio-ethnic background and achievement motivations: correlations*

Having considered several social structural models of the process of status attainment for religio-ethnic subgroups, one now asks how the introduction of personality variables (i.e., motivations) enlarges the understanding of religio-ethnic achievement.

In another source (Featherman, 1969) it has been shown that the PFS religio-ethnic subgroups could be differentiated on a series of work-related values bearing close similarity to the items employed by Lenski (1963) to elicit the spirit of the Protestant Ethic. That analysis suggested some exceptions to the conclusions drawn by Lenski about the relative values and achievements of Protestants, and it spoke to the importance of differentiating major religious groups (such as white Protestants and Roman Catholics) by ethnic ancestry.

Of importance herein are three indexes of motivational orientations toward work, derived from a psychological supplement to the 1957 interview. Details of index construction appear elsewhere (Featherman, 1969: 142-159); the actual questionnaire items in each are found in the appendix below. Suffice it so say that two action tendencies or orientations toward the occupational or work setting were deduced by this author from the conceptual and empirical writing in the area of achievement-related motivation, in which a distinction has been raised between intrinsic and extrinsic motivation in an achievement situation (Douvan, 1956; Lenski, 1963:89; Gurin *et al.*, 1960:149-150; Veroff *et al.*, 1962; Burnstein, 1963; Burnstein *et al.*, 1963; Kohn and Schooler, 1969).

The first action tendency, "Primary Work Orientation" (WO), attributes a positive value to the work or job context. This positive value is noneconomic in quality, but whether work is regarded as intrinsically

<sup>7</sup> To illustrate the calculation routine for the estimation of the values in Table 2, the religio-ethnic effects on income at Panel I are selected. The direct effect of (say) Jewish subgroup membership on income in 1957, net of the variation attributable to social origins, is the value for path *c* in Figure 1; this value is obtained from Table 1, Column 8, where for Jews "\$713" is recorded. The indirect effect of Jewishness is expressed in Figure 1 via two paths—through education and occupation at Panel I. Values for the educational route are computed from  $[a(g + fj)]$ , and the effects through occupation are calculated from  $[bj]$ . The coefficient *a* is taken from Table 1, Column 2 (1.15), the estimates for *g* and *j* come from Table 1, Column 8 (*g*=\$15.; *j*=\$870.), and *f* and *b* are taken from Table 1, Column 5 (*f*=.44; *b*=.28). Multiplying the estimates according to the expressions above determines that, for men of equal social origins, Jewish males earn \$713 above the average due to the direct effects of their subgroup membership. Indirectly through their educational achievements, they earn an additional \$460, and from their relative occupational status, \$241, which total to \$701. These values for Jews appear in Table 2, Column 2.

Table 2. Direct and Indirect Effects of Religio-ethnic Categories on Socio-economic Achievements at Panels I and III.

Religio-ethnic Subgroups	Dependent Achieved Statuses			
	Occ-NORC I	Income I	Occ-NORC III	Income III
Jewish, All Ethnic:				
Direct	.28	713.	-.14	325.
Indirect Total	<u>.51</u>	<u>701.</u>	<u>.72</u>	<u>1967.</u>
Education	.51	460.	.47	1176.
Occ-NORC I	--	241.	.25	613.
Income I	--	--	--	233.
Occ-NORC III	--	--	--	-55.
Anglo-Saxon Protestant:				
Direct	.25	-149.	-.08	-31.
Indirect Total	<u>.32</u>	<u>509.</u>	<u>.52</u>	<u>1217.</u>
Education	.32	288.	.30	737.
Occ-NORC I	--	220.	.23	560.
Income I	--	--	--	-49.
Occ-NORC III	--	--	--	-31.
Other Protestant:				
Direct	-.14	258.	-.03	-97.
Indirect Total	<u>-.11</u>	<u>-223.</u>	<u>-.23</u>	<u>-495.</u>
Education	-.11	-103.	-.11	-262.
Occ-NORC I	--	-120.	-.12	-305.
Income I	--	--	--	84.
Occ-NORC III	--	--	--	-12.
Roman Catholic, Exc. Italian & Mexican:				
Direct	-.07	-247.	-.03	350.
Indirect Total	<u>-.13</u>	<u>-178.</u>	<u>-.18</u>	<u>-565.</u>
Education	-.13	-118.	-.12	-302.
Occ-NORC I	--	-60.	-.06	-153.
Income I	--	--	--	-81.
Occ-NORC III	--	--	--	-12.
Italian & Mexican Roman Catholic:				
Direct	-.15	-227.	.21	-448.
Indirect Total	<u>-.22</u>	<u>-325.</u>	<u>-.33</u>	<u>-824.</u>
Education	-.22	-199.	-.20	-508.
Occ-NORC I	--	-126.	-.13	-321.
Income I	--	--	--	-74.
Occ-NORC III	--	--	--	79.
Others:				
Direct	-.11	-113.	.22	-710.
Indirect Total	<u>-.30</u>	<u>-371.</u>	<u>-.38</u>	<u>-899.</u>
Education	-.30	-274.	-.28	-699.
Occ-NORC I	--	-97.	-.10	-248.
Income I	--	--	--	-37.
Occ-NORC III	--	--	--	85.

satisfying as an activity or whether it is so regarded for its noneconomic instrumental possibilities (the expression of achievement, affiliative, and power tendencies) is not known. Agreement with the questionnaire items defining this construct indicates a preference for work rather than relaxation, a choice of the occupational role over recreation. [Both the spending of time at an activity and the expressing of choice (preference) it, vis-à-vis some competing activity, are behavioral indicators of motivational tendencies.]

The second action tendency toward one's work is called "Materialistic Orientation" (MO). Whereas the first tendency refers to the noneconomic incentives of one's occupational role, the present index emphasizes the material goals of the good life, goals achieved through work as instrumental activity. The items defining the second motivational construct suggest some expressed anxiety over the prospects of not attaining the desired level of material prosperity. They also incorporate a social comparative or reference group focus—material achievement of consumption equalling or surpassing that of neighbors and friends.

Empirical support for the clustering of questionnaire items in conformity with these conceptual distinctions is gained from a factor analysis of the item matrix (Featherman, 1969:142-159). The estimated reliability of index WO is .841, and of index MO, .717.

A third index of achievement-related motivation toward work is constructed from items seemingly eliciting a man's satisfaction with his socioeconomic achievements to

the time of assessment (Panel I). The index, "Subjective Achievement Evaluation" (SA), serves as an indicator of the conscious psychological effects of past and present socioeconomic achievements which (presumably) have motivational properties for an individual's socioeconomic achievements over the duration of the follow-up period. One might think of SA as indicating subjectively perceived relative deprivation. The reliability coefficient for index SA is .797.

Table 3 records the correlation coefficients between the three motivational indexes and between each index and the occupational and economic achievements over the restudy period. WO and SA are positively correlated (.44) and both are positively correlated with the achievement variables over the seven-to-ten-year duration of the study. MO and SA are negatively correlated (-.24), while MO and WO are uncorrelated (-.01) and are virtually orthogonal orientations toward socioeconomic achievement. Zero-order correlations between MO and the occupation and income variables are negative. Multivariate analysis in which these three indexes appear along with social background and education variables shows that each motivational index bears a separate, positive net relationship to socioeconomic achievements at Panels II and III (Featherman, 1969). Thus, adult achievement-related motivational indexes such as WO, MO, and SA (measured at Panel I) have predictive validity for occupational and economic achievements over a seven-to-ten-year period, apart from their correlations with socioeconomic background and educational achievement.

Table 3. Coefficients of Correlation between Three Motivational Indexes and Socioeconomic Achievements over the Restudy Period.

Variables	MO	SA	Occ I	Inc I	Occ II	Inc II	Occ III	Inc III
Primary Work Orientation (WO)	-.01	.44	.41	.24	.40	.28	.36	.31
Materialistic Orientation (MO)		-.24	-.12	-.06	-.09	-.05	-.11	-.01
Subjective Achievement Evaluation (SA)			.36	.33	.33	.40	.31	.34

Using the three achievement-related orientations, Table 4 reveals religio-ethnic differences in motivations toward work. One notes that Jews have higher mean scores for both WO and MO and are second only to the Anglo-Saxon Protestants on the SA index. Somewhat like the Jews, both Roman Catholic subgroups score higher on the MO index, but lower than the other religio-ethnic categories on WO and SA. Although below the mean score of the Jews, the Anglo-Saxon Protestants also are high on WO, and their average score on SA is the highest. Apparently Anglo-Saxon Protestants are not materialistically oriented toward work, for their MO scores are below all other religio-ethnic subgroups. Other Protestants score comparatively intermediate on all three indexes.

Differences between the mean scores on each of the three indexes for the religio-ethnic subgroups are tested for statistical significance in an analysis of variance. Although the subgroup variation on WO proves non-significant ( $F_{4,668} = 1.551$ ), the religio-ethnic differences on MO and SA are significant at  $\alpha = .01$  (F-ratios of 3.531 and 4.503, respectively).

Unlike Lenski's findings for Detroit, Jews in these data report positive motivations toward work *both* as an instrumental activity (extrinsic value) and as a terminal activity

(intrinsic value). Anglo-Saxon Protestants respond more strongly to the intrinsically rewarding aspects of work, in the fashion one might presume if WO were a generally valid indicator of the Weberian essence of the Protestant Ethic, as Lenski interpreted that Ethic. These same Protestants, however, are less responsive to work for materialistic reasons (MO), and thus are less motivated by the elements of the more modern interpretation of the Protestant Ethic. Roman Catholics, like the Jews, share in a materialistic approach toward work, although possibly for differing sets of reasons. While the Jews have a positive subjective sense of socioeconomic achievement and may respond to work as a source of future economic benefits, the Roman Catholic's SA scores are low. Their positive response to work as a source of future economic goods quite likely is born of relative economic deprivation (Veroff *et al.*, 1962).

Thus, Jews possess the unique set of work values and motivations which predict socioeconomic success over the course of the follow-up period. They and the Anglo-Saxon Protestants share in what by some has been labeled the Protestant Ethic, an orientation toward work which emphasizes the intrinsically satisfying and interesting aspects of work activities. The Jews also (but not the Protestants) respond to the instrumental qualities of work, as evidenced by their MO scores which predict actual higher income levels for this subgroup. The Jews, then, are doubly endowed with the Protestant Ethic, as it were; they respond psychologically to extrinsic and intrinsic work rewards.

#### *The social psychology of religio-ethnic stratification*

One wishes to move beyond these correlations of values, motivations, achievements, and subgroup background to the multivariate case. Specifically, one hopes to ascertain if the motivational orientations toward work which are specific to each religio-ethnic subgroup can account for the differential socioeconomic achievements reported above. The three orientations toward work are thus conceived as intervening personality variables. As such, one might argue that WO, MO and SA are indicators of relatively enduring char-

Table 4. Mean Scores for Five Religio-ethnic Subgroups.

Religio-ethnic Subgroups	WO <sup>a</sup>	MO <sup>b</sup>	SA <sup>c</sup>
Jewish, All Ethnic	3.50	4.17	5.19
Anglo-Saxon Protestant	3.38	3.26	5.81
Other Protestant	2.98	3.50	5.06
Rom. Cath., Exc. Ital. & Mex.	2.84	3.73	4.98
Ital. & Mex. Rom. Cath.	2.53	3.70	4.28
Total	2.97	3.62	5.03

<sup>a</sup>Work Orientation

<sup>b</sup>Materialistic Orientation

<sup>c</sup>Subjective Achievement Evaluation

acteristics, but influenced also by situational experience in the educational process and early adult career experiences.

How should such personality variables be included in the causal model (Figure 1)? To be most accurate, one should include the three indexes as causally prior to Panel III achievements but coincident with those of Panel I, when they were measured. One could argue (as do most social psychologists limited to cross-sectional data) that WO, MO, and SA as measured at Panel I are essentially stable indicators, at least in the short run; therefore, one could assume them to be causally antecedent to the specific attainment of Panel I statuses but perhaps causally dependent upon the level of schooling. Alternatively, one could represent the dispositional variables as causally dependent upon education and Panel I achievements, as correlated with them, or in some set of reciprocal causal relationships with the 1957 statuses. For the following analysis, it is assumed that educational completion is antecedent to the formation of the dispositions, which in turn are causally prior to Panel I achievements.<sup>8</sup>

Having outlined the causal assumptions in the social psychological model, one can test the several propositions about the role of motivations in the status attainment of religio-ethnic subgroups; occupational and economic achievements at Panel I are the dependent variables.<sup>9</sup> The first proposition to be tested is the following: Religio-ethnic differences in achievement-related motivations (WO, MO, and SA) account for the

gross variation in occupational and economic achievements by the subgroups. This asserts that differences in achievement orientations account wholly for religio-ethnic achievement differentials, where the motivational indexes are treated as intervening variables between religio-ethnic background and socioeconomic achievement. The partial regression coefficients in Table 5 for the dependent variables occupation (left panel) and income (right panel) enable us to calculate mean squares for each column or "model," and it is the comparisons of these models or columns within each panel of Table 5 which provide the tests for the above and subsequent propositions with respect to both dependent variables. When one tests the mean squares of the difference between the explained sums of squares in Columns 4 and 3 in the left and right panels of Table 5 against the mean square of the residual in Column 4, the *F*-ratios for occupation ( $F_{8,708} = 8.177$ ) and for income ( $F_{8,708} = 3.669$ ) are both statistically significant at  $\alpha = .01$ . This indicates that there remains a statistically significant amount of variation in the religio-ethnic socioeconomic achievements after scores on the three psychological indexes have been controlled, and this test gives no support to the proposed contention that religio-ethnic differentials in Panel I socioeconomic achievements are *primarily* a function of the variation in religio-ethnic achievement-related motivations. At the same time, the results of this test do not gainsay the assertion that motivations play some role in the achievement of occupational and economic statuses for religio-ethnic subgroups. They do establish that this role is not a major one for achievement-related work motivations measured in the early adulthood of metropolitan males.

A second proposition takes a more limited view of the role of achievement orientations as intervening variables. It asserts that motivations explain religio-ethnic achievements within social background and education categories. Achievement-related motivations account for the religio-ethnic differences in occupation and income, for men of statistically equivalent social origins and education.

This proposition clearly is not supported, since the analysis above demonstrates that the religio-ethnic effects on Panel I achieve-

<sup>8</sup> This causal assumption, its alternatives, and their effects on models, which include motivations as intervening variables in the status attainment process, are topics treated in other sources (Featherman, 1969) and in continuing analyses.

<sup>9</sup> Only the analysis for socioeconomic achievements at Panel I is reported since gross religio-ethnic differences are substantial at this stage in the careers and because the motivational variables are believed to be most potent for achievements concurrent with the psychological assessment. Also, a separate analysis using Panel III achievements supports the same set of conclusions about the role of motivations, but at the same time it is a more severe test of the hypotheses than the one reported here. This being true, the causal assumptions about the ordering of the dispositional variables vis-a-vis Panel I achievements seem to do little harm to this analysis.

Table 5. Partial Regression Coefficients for Effects of Socioeconomic and Religio-ethnic Background, Work Motivations, and Prior Achieved Statuses on Occupation and Income at Panel I.

Independent Variables	Occupation I					Income I				
	1-6	7-11	9-11	1-6 9-11	1-8 1-11	1-6	7-12	9-11	1-6 9-11	1-8 1-12
1. Jewish, all ethnic	1.048	---	---	.908	.276	.315	---	---	1420.36	793.44
2. Anglo-Saxon Protestant	.843	---	---	.547	.285	.222	---	---	203.61	18.57
3. Other Protestant	-.204	---	---	-.224	-.123	-.144	---	---	59.53	216.06
4. Roman Catholic, exc. Italian & Mexican	-.370	---	---	-.344	-.087	-.123	---	---	-596.66	-302.55
5. Italian & Mexican Roman Catholic	-.643	---	---	-.389	-.167	-.112	---	---	-446.15	-308.32
6. Others <sup>a</sup>	-.397	---	---	-.055	-.112	.048	---	---	196.12	-142.39
7. Father's Occ-NORC	---	.097	---	---	.095	.075	---	---	---	39.22
8. Education	---	.397	---	---	.446	.376	---	---	---	224.92
9. Primary Work Orientation	---	.157	.331	.307	---	.165	---	---	186.26	---
10. Materialistic Orientation	---	-.026	-.078	-.081	---	-.027	---	---	---	---
11. Subjective Achievement Evaluation	---	.076	.154	.149	---	.082	---	---	378.18	---
12. Occ-NORC I	---	---	---	---	---	---	---	---	---	384.46

<sup>a</sup>Includes those reporting "none" or "other" for religious preference and/or NR on ethnic background.

ments are zero, after the variation from social origins and education is removed statistically. At least for the post-education period, the adult motivational variables do not function as intervening variables between religio-ethnic background and socioeconomic career achievements.

Perhaps the achievement orientations function as intervening variables, but in a different manner. A third proposition looks within each religio-ethnic category and asserts that work motivations function as important intervening variables between social origins and socioeconomic achievements.

The data which test this proposition document more clearly the inadequacy of the three psychological indicators as intervening variables; Column 6 in each panel of Table 5 is tested against Column 4. For occupation ( $F_{2,704} = 119.699$ ) and for income ( $F_{3,703} = 12.225$ ), the additional variance explained by father's occupation and the son's own education over the explained variance due to the religio-ethnic classifications and the psychological scores is unquestionably significant. One concludes from this test that the motivational variables are unable to account for the full effects of social background and education on socioeconomic achievement, net of the variation in the dependent variables being due to religio-ethnic background.

While operating as weak *intervening* variables between backgrounds (social and religio-ethnic) and the achieved socioeconomic statuses, the three psychological indexes do affect occupation and income *directly* at Panel I. Comparing Column 6 in each panel of Table 5 with Column 5, one calculates statistically significant F-ratios (occupation:  $F_{3,704} = 18.727$ ; income:  $F_{3,703} = 13.118$ ), indicating that within levels of education, father's occupation, and the religio-ethnic classification, the three indexes contribute directly to the explanation of socioeconomic achievement.

These data and tests suggest that although there are differences among the religio-ethnic subgroups in work-related motivations and in the actual distribution of occupational prestige and income, the adult motivations are not a sufficient explanation of the religio-ethnic, socioeconomic achievements. Taken in conjunction with controls for social origins and education, motivations as here measured

have some small, separate role in reducing the differences in occupational and economic levels between the usually overachieving Jews and the underachieving Roman Catholics of Italian or Mexican ancestry. However, net of the religio-ethnic classification, work motivations do not explain how the effects of social origins and education get translated into socioeconomic attainments. Whatever role motivations play (if any) in the relative achievements of religio-ethnic subgroups, it is not a direct one in the post-education period among men of similar social origins. Thus, these data do not support the assertion that adult motivations are the major intervening variables between social and religio-ethnic background factors and occupational and economic achievements which account for the effects of the former on the latter.

### *Discussion and conclusions*

Data on the religio-ethnic differences in the education and socioeconomic achievement for white metropolitan males demonstrate roughly the same amount of gross subgroup variation near the beginning and in the middle of the work career. By the middle of the career (Panel III) virtually all religio-ethnic variance in occupational and economic achievement can be explained by differentials in social origins, education, and early career attainments. The largest achievement differential was observed for education, where about one standard deviation in years of schooling separated the subgroups. These levels of education represent the effects of social origins, but, net of social background, one's religio-ethnic origins have a direct impact on the completion of years of schooling. Whether this net religio-ethnic effect represented discrimination, achievement-related motivation, or both, could not be determined. However, the latter was the only direct effect of religio-ethnic background on the socioeconomic career.

In an analysis of personality correlates of subgroup achievement, one observed that religio-ethnic categories could be differentiated on achievement-related motivations and by actual socioeconomic achievements. However, these adult motivational variables proved insufficient to explain away the gross occupa-

tional and economic variation for the subgroups. Net of the religio-ethnic classification, the same motivational variables could not account for the effects of social origins or education on early career attainments. Neither were the motivations involved in any effects of religio-ethnic background on achievement, net of education and father's occupation. Thus, the motivational arguments of social psychologists to the effect that achievement-related motivations are major sources of religio-ethnic differences in the occupational and economic achievements of adults, and that these motivations are key intervening variables in the process of socioeconomic achievement in the general population, need to be re-examined and modified.

In light of the foregoing conclusion about the key role of education in the process of religio-ethnic stratification, and for the general population as well, one can suggest that if motivations and other personality dimensions do function as "key" intervening variables in the processes of achievement in American society, then perhaps they constitute important determinants of the number of years of schooling obtained by men, generally, and by certain religio-ethnic subgroups, specifically. This possibility is taken up in continuing research, although a discussion of this issue elsewhere (Featherman, 1969) discourages unqualified optimism about the prospects of such an investigation.

What seems clear from this analysis is that the achievement-related motivations of adult males play a modest role in the post-education socioeconomic career. However, the same motivations are relatively unimportant in the distribution of socioeconomic statuses to the various religio-ethnic subgroups in metropolitan areas. When speaking of motivations and mobility in the future, social psychologists should define clearly the portion of the life cycle (vis-a-vis educational completion) in which the relationship is purported to exist, especially for research among religio-ethnic subpopulations.

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## APPENDIX

ACHIEVEMENT-RELATED ORIENTATIONS  
TOWARD WORK: QUESTIONNAIRE  
ITEMS

## Primary Work Orientation (WO)

(N=715)

My work is more satisfying to me than the time I spend around the house. (Yes=29.1%)

If I inherited so much money that I didn't have to work, I'd still work at the same thing I am doing now. (Yes=49.5%)

Some of my main interests and pleasures in life are connected with my work. (Yes=57.8%)

The work I do is one of the most satisfying parts of my life. (Yes=53.1%)

I enjoy my spare-time activities much more than my work. (No=45.6%)

To me, my work is just a way of making money. (No=63.6%)

## Materialistic Orientation (MO)

It is extremely important to me to have a higher income. (Yes=60.7%)

I spend a lot of time thinking about how to improve my chances for getting ahead. (Yes=70.6%)

Getting money and material things out of life is very important to me. (Yes=51.5%)

It is important to me to own material things, such as a home, car, or clothing, which are at least as good as those of my neighbors and friends. (Yes=47.0%)

I am very anxious to get much further ahead. (Yes=76.7%)

Getting ahead is one of the most important things in life to me. (Yes=56.6%)

## Subjective Achievement Evaluation (SA)

I feel that my wife and I have had very good financial breaks since we have been married. (Yes=65.0%)

I would be satisfied if a son of mine, when he reaches my age, is in the same kind of work I am now in. (Yes=45.7%)

I feel that my present financial situation is very good. (Yes=39.5%)

I would be satisfied if my children received the same amount of education as I have. (Yes=35.2%)

I am pretty well satisfied with the chances for getting ahead in my present work. (Yes=64.6%)

I have sometimes regretted going into the kind of work I am now in. (No=56.0%)

I would be satisfied if my children, when they reach my age, have the same income and live the same way as I. (Yes=45.4%)

On the whole, my financial future looks very good. (Yes=74.1%)

There are many times when I have to deny myself and my family things we would like because of our income. (No=30.9%)

In my present financial situation, I have to worry about bills or debts. (No=48.3%)

# UPWARD SOCIAL MOBILITY AND POLITICAL ORIENTATION: A RE-EVALUATION OF THE EVIDENCE \*

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American Sociological Review 1971, Vol. 36 (April):223-235

*That upward social mobility has a different effect on the political orientations of Europeans than of Americans is commonly accepted. Several scholars have concluded that upwardly mobile Europeans are less conservative than middle-class stabiles. The contrasting conclusion that in the United States upwardly mobile people become even more conservative than middle-class stabiles is based on a single study completed some years ago.*

*Here, this relationship is re-examined by analyzing data from five nationally-representative American samples over 14 years. In these samples, the upward mobiles are consistently less likely to be conservative than the middle-class stabiles and more likely to be conservative than the working-class stabiles. However, analysis of the joint effects of sex and social mobility indicates that American upwardly mobile males are more likely to approximate the politics of the class to which they have risen than are upwardly mobile females.*

IN recent years the conclusion that upward social mobility has a different effect on the political orientations of Europeans than Americans has become accepted by many as well-established. For example, a current introductory text on social stratification (Tumin, 1967:94) states without qualification, "In America, persons who move up into the middle class are more conservative than those born into it, whereas in European countries studied, the latter are more conservative than the former."

The evidence for this conclusion was originally presented by Lipset and Zetterberg in a pioneering article, "A Theory of Social Mobility," prepared for the Third World Congress of Sociology in 1956. Utilizing survey data, Lipset and Zetterberg contrasted the proportions of left-wing party supporters among middle-class men from divergent class backgrounds in Finland, Germany, and the United States. In the American sample pre-

sented, those middle-class men with fathers who were manual workers, i.e., the upwardly mobile, were 4% less "left-wing" (Democratic) in their 1948 party choice and 8% less so in 1952 than the middle-class respondents who had middle-class fathers. In contrast, the 1949 Finnish data indicated the upwardly mobile were 17% more left-wing than the middle-class stabiles, and the 1953 German sample showed a similar ordering with the upward mobiles 12% more leftist than those who were both raised in the middle class and presently located in it by occupational definition.

No explanations for this cross-Atlantic difference were offered in "A Theory of Social Mobility," but, when the same data were presented three years later in Bendix and Lipset's *Social Mobility in Industrial Society* (1959) with the addition of consistent information from Swedish and Norwegian samples, it was set in the theoretical context of status striving and rejection. Here Lipset and Zetterberg suggest that the relatively greater tendency to take a left-wing orientation among the upward mobiles in Europe in contrast with the United States can be accounted for by factors forcing the upwardly mobile Europeans to retain links to the class of origin to a greater extent than upwardly mobile Americans. The authors hypothesize that greater class differences in life style and concern with an individual's

\* The data utilized in this study were made available by the Interuniversity Consortium for Political Research, which bears no responsibility for the analyses or interpretations presented here. Financial support in aid of this research was provided by the University of Southern California Research and Publication Fund and by the Ford Foundation Faculty Research Fellowship Program. The helpful advice of S. M. Lipset is gratefully acknowledged. An earlier version of this paper was read at the 1969 Annual Meetings of the American Sociological Association in San Francisco.

class background in European society lead the socially mobile individual who has risen from a working-class background to experience status rejection and frustration. As a consequence, the upwardly mobile European is viewed as less likely to embrace the conservative political norms of the class to which he has risen than his American counterpart. Lipset and Zetterberg speculate that, since upwardly mobile Americans are less likely to suffer status rejection after rising to the middle class, there is relatively little pressure for the mobile individual to retain the left-wing political orientation of the working class from which he came. No suggestion is made by the authors, however, to account for the political "over-conformity" of upwardly mobile Americans to the political norms of their class of destination.

Other scholars, basing their research on the pioneering work of Lipset and Zetterberg, without further confirmation of their American results, have developed alternative interpretations to explain the apparently different political consequences of upward social mobility for Europeans versus Americans. For example, Anderson's (1963) sample of Swedish men and women showed, as did the earlier Swedish sample cited by Lipset and Zetterberg, that the upwardly mobile Swedish respondents did not adopt the political orientation of the class to which they had risen, but were politically intermediate in party choice between the more extreme positions of the class stables.

Rather than attributing differing political consequences of upward mobility to differential amounts of status rejection in Sweden and America, Anderson views political socialization as the key variable. Specifically, he argues that a greater degree of political socialization among Swedish workers than among American workers is the most likely reason upwardly mobile Swedes maintain the working-class party preference to a greater extent than their upwardly mobile counterparts in the United States. He points out that a complex organizational structure links the labor unions to the Social Democratic Party in Sweden, and grass-roots participation by workers and their families in politically-relevant educational, recreational, and economic activities is encouraged. Anderson contrasts the politically involved

Swedish workers with what he considers the more politically apathetic blue-collar Americans, dominated by local political machines that discourage any involvement other than periodic voting for machine candidates.

Lopreato (1967) examines the differences between the political effects of upward social mobility on Europeans and Americans from a perspective which tests more directly the explanation offered by Lipset and Zetterberg. After presenting data from a sample of Italian male family heads showing that, in Italy as in the other European nations sampled, the upward mobiles were less likely to be conservative than the middle-class stables, Lopreato examines the influence of discrepancies in consumption styles and experienced status rejection on the political orientation of the upwardly mobile as suggested by Lipset and Zetterberg. His evidence indicates that the former variable is not a significant factor accounting for the difference in the political choice of the Italian middle-class stables and upward mobiles, for with level of consumption controlled, the latter remain proportionally more left-wing in political orientation than the former. However, status rejection is found to be a key variable in Lopreato's analysis, for those upwardly mobile Italians in his sample who perceived restrictions in social relations between the classes were significantly more likely to be left-wing in political orientation than those who did not perceive class relations as restricted. Furthermore, the latter group of upward mobiles does not differ politically from the middle-class stables.

Although Lopreato's analysis suggests that status rejection is the key variable distinguishing the experiences of the upwardly mobile in the United States and Europe, he points out that an absence of status rejection among upwardly mobile Americans in contrast to upwardly mobile Europeans can not account for the finding that the upwardly mobile are *more* likely to be conservative than the middle-class stables in this country. To account for this phenomenon, Lopreato suggests another variable, the "excessive" emphasis on success and achievement in American society. In response to this ethos, Lopreato speculates that the successful upward mobiles express their gratitude for a social order enabling them to rise by "over-

conformity" to the political norms of the middle class.

These examples indicate that the original findings reported by Lipset and Zetterberg have produced both rich and imaginative speculation attempting to account for the conclusion that the political behavior of the upward mobiles in the United States is different from that of their counterparts in Europe. Yet while a number of studies have presented evidence tending to verify the original finding that European upward mobiles are less likely to be conservative than those stable in the middle class, the single finding indicating that the upward mobiles are more likely to be conservative than the middle-class stables in the United States has not received similar confirmation.

Thus the primary purpose of this paper is to examine rather closely the evidence on the political orientation of the upward mobiles and middle-class stables in this country in an effort to determine the validity and reliability of Lipset and Zetterberg's original conclusion that American upward mobiles are more often Republican than the middle-class stables. This re-examination of the evidence will involve a review of some of the relevant literature as well as a presentation of more recent survey data relating social mobility to political orientation.

### *Contradictions in the Literature*

Certain studies purporting to have examined the relationship between social mobility and political orientation in the United States are often cited. Taken as a set, they reveal the need for greater systematic empirical examination of the topic. Not only are serious problems of limited sampling and inadequate operationalization evident, but the contradictory findings further emphasize that the issue of the specific relationship between upward mobility and partisanship in the United States has not yet been settled.

One of the earliest attempts to survey the relationship between upward social mobility and political party choice is presented by Patricia West (1953). Working with what she states is a nationwide representative sample of nearly 10,000 college graduates, West examines the effect of social mobility on political orientation by contrasting the party

identifications of "self-made men" and "privileged men." She defines the "self-made man" as a person who "had to earn the bulk of his way through college and has gone on to reach the top economic brackets." The "privileged man" has reached the same level of economic success, but did not have to earn any of his college expenses. To test for possible changes in the relationship as individuals grow older, West divides her sample into men over the age of 40 and under 40.

West concludes that the upwardly mobile "self-made men" do in fact differ in their political party choice from the "privileged men." She finds the upwardly mobile in her sample *less* likely to identify with the Republican party than the middle-class stables and *more* likely to withhold from party identification by choosing the "Independent" affiliation. However, finding that these differences are less in the older age group than in the younger, West reasons that as upwardly mobile men grow older they do tend to increasingly "forget" the social patterns and political habits that are widespread among those in their old economic stratum.<sup>1</sup>

The next study, explicitly comparing the politics of the upwardly mobile with that of the middle-class stables, is reported by Maccoby *et al.* (1954). Their sample was limited to young people: 339 respondents in Cambridge, Massachusetts, between the ages

<sup>1</sup> West's findings are cited by Blau (1956:291) to the effect that "... the upwardly mobile are more likely to vote Republican than people who have remained workers and less likely to do so than those who have originated in the middle class." Blau argues that these findings support his contention that political orientation follows the "acculturation" pattern whereby the behavior of both the upwardly and downwardly mobile is located intermediately between that of the two nonmobile categories, the stable middle class and the working-class stables. Blau also cites *Voting*, the landmark study of political behavior by Berelson *et al.* (1954), as furnishing evidence similar to that provided by West. However, a careful examination of the *Voting* material reveals that the analysis of social mobility and political orientation conducted therein does not permit such a definitive conclusion. Berelson *et al.* limited their analysis to a comparison of two groups: (1) those upwardly mobile *relative* to their fathers and (2) those whose occupations are the *same or lower* than those of their fathers. Not only are the downwardly mobile unsegregated from the class stables, but the relative measure of mobility employed fails to identify the present social position of the respondents in any fixed way.

of 21 and 24. The authors find that the upward mobiles in their sample are basically similar in political orientation to the middle-class stables and conclude that (1954:39) "young people who are socially upwardly-mobile tend to adopt the political behavior of the group into which they have moved." However, when the degree of willingness to make a definite party choice is examined, a difference is found between the two groups. The authors summarize that (1954:34) "... the mobile young people seldom consider themselves Independents: they seem to make a definite party choice more often than non-mobile people."

Greenstein and Wolfinger (1958) consider social mobility to be among the factors which may possibly influence change in party loyalty. They hypothesize that upwardly mobile individuals conform to the political norms of the class they have joined in order to gain security and recognition of their new social position. However, they state that their data did not provide support for this thesis, for they found that "suburbanites who were objectively upwardly mobile . . . were more Democratic than were 'stable' suburbanites who were at the same occupational levels, suggesting that these individuals tend to adhere to familial party loyalties" (1958:479). From these data, the authors conclude that "we found no association between 'objective' upward mobility and Republican allegiance . . ." (1958:481).

The contradictory nature of the findings

in the literature on upward social mobility and political orientation in the United States is illustrated in Chart 1. The summarized conclusions there show that in addition to the view that upward mobiles are more likely to be conservative than middle-class stables in the United States, other findings exist to argue that the two groups are politically indistinguishable or that the upward mobiles are even less likely to be conservative than the middle-class stables.

Thus this brief review of the literature cautions against a ready acceptance of the widely disseminated view that in the United States there are a greater proportion of conservatives among those who have made an upward change in class than among those who have remained stable in the middle class. In fact, the only conclusion that seems warranted by the evidence developed until now is that *no* consensus has been reached by scholars who have touched on the subject of upward social mobility and political orientation in the United States. An examination of more recent national samples seems imperative in this circumstance.

### *The Evidence from National Samples*

The data for the following analyses are taken from nationally representative samples of the American electorate drawn by Michigan's Survey Research Center. To form the social mobility categories employed here, respondents were first placed in manual and

Chart 1. Conclusions Concerning the Relative Conservatism of Upward Mobiles and Middle-Class Stables in the United States.

Study	Sample	Upward Mobiles Relative to the Middle Class Stables Are--
West, 1953	Male college graduates, early 1950's	<u>Less</u> likely to be conservative while young <u>Similar</u> when older <u>More</u> likely to be independent
Maccoby et al., 1954	Cambridge youths between 21 and 24, 1952	<u>Similar</u> in conservatism (while young) <u>Less</u> likely to be independent
Lipset and Zetterberg, 1956	National, males, 1952	<u>More</u> likely to be conservative
Greenstein and Wolfinger, 1958	Suburbanites, 1952	<u>Less</u> likely to be conservative

nonmanual occupational categories according to the nature of their usual job or, if the respondents were housewives, according to that of the head of the household. Unemployed and retired respondents were classified according to their usual occupation or that job held prior to retirement. Farmers, students, and those failing to provide occupational information were eliminated from this study.

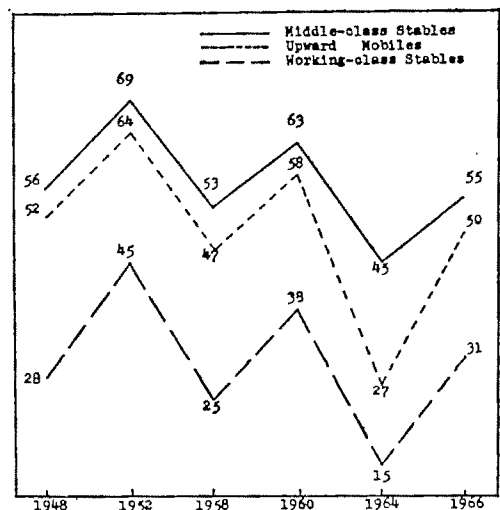
After being classified on the basis of their present occupationally defined class, respondents were also categorized on the criterion of whether the father's occupation was manual or nonmanual when the respondent was growing up.<sup>2</sup> Those in the sample who failed to give information on their fathers' occupations or who had farming fathers were excluded from this analysis. Thus the category of "middle-class stables" in this study consists of those respondents who both currently hold middle-class jobs and were raised as children in middle-class homes. The "upward mobiles" are those who now hold white-collar positions, but were raised in homes where the father was a blue-collar worker. Finally, the "working-class stables" are those in the samples who are both blue-collar in origin and also presently hold blue-collar jobs.

Unfortunately, information on the occupation of the respondent's father has often been omitted from questionnaires in the past. The 1952 study conducted by the Survey Research Center at Michigan appears to be the first nationally representative American sample which taps all the variables necessary for the formation of occupationally defined categories of intergenerational social mobility. However, respondents in the 1952 samples were not only questioned on their party

preferences in regard to the then upcoming presidential election between Stevenson and Eisenhower, but were also queried about the party they had supported four years earlier during the Truman-Dewey contest for the presidency. Although such long-term recollections are of dubious validity as indicators of past behavior, the remembered party preference for the 1948 election is included in this analysis to extend the time span covered to as early a date as possible.

Figure 1 presents in graphic form the proportions in each social mobility category indicating a Republican party preference for six elections extending from 1948 to 1966. Thus it indicates both an absolute level of political conservatism within the mobility categories over time and also permits a comparison of the proportion of conservatives among the upward mobiles relative to that among the middle-class stables and the working-class stables. The most striking finding to be drawn from these data is that, despite the different candidates and issues involved in the various contests, in none of the six elections do the upward mobiles ex-

FIGURE 1: PER CENT REPUBLICAN OF TWO-PARTY CHOICE, BY SOCIAL MOBILITY CATEGORY, 1948-1966



The number of voters on which these proportions are based are, for the middle-class stables, upward mobiles, and working-class stables, respectively: 1948: 180, 128, and 213; 1952: 211, 145, and 249; 1958: 187, 161, and 222; 1960: 264, 189, and 315; 1964: 193, 187, and 241; 1966: 110, 103, and 126.

<sup>2</sup> Since father's occupation can vary over time, it is important to note the specific timepoint for measurement selected in a given study. For the purposes of this study, father's occupation "when the respondent was growing up" is a pertinent reference point, since it pinpoints the most likely time of social-class socialization influences on political behavior. However, other studies may ask for father's "main" or "last" occupation. The use of such different reference points could lead to variation in the assignment of certain cases to mobility categories, and this type of variation may be a source of some of the disparate findings in mobility studies.

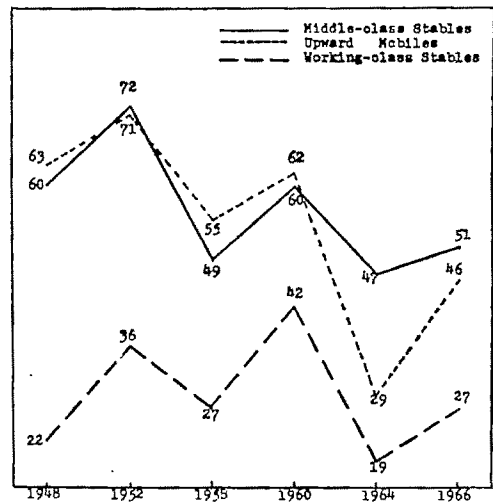
hibit a greater proportion of conservatives than do the middle-class stables.

Although the evidence for five of the six elections indicates a somewhat minor difference between the upward mobiles and the middle-class stables in the proportion of two-party support going to the Republicans, the tendency for a greater proportion of conservatives to be found among the middle-class stables than among the upward mobiles is a consistent one. In the 1964 sample, however, the upward mobiles actually exhibit a pattern of party support closer to that of the working-class stables than to that of the middle-class stables.<sup>8</sup>

Viewed in terms of a *rank-ordering* of the three social mobility categories on the dimension of political conservatism, no variation is found over the six election years for which evidence is presented. In each case examined here, the middle-class stables have the greatest proportion of conservatives; the working-class stables, the smallest proportion of conservatives, and the upward mobiles hold an intermediate rank-order position between the two other categories, even though they generally are closer in political tendency to the stable middle class. Thus these data indicate that the upward mobiles in the United States do not differ as markedly in political orientation from their counterparts in Europe as has been assumed.

How then are these data to be reconciled with those presented by Lipset and Zetterberg upon which the "European-American" difference in the effect of social mobility on political orientation was based? It will be noted that the major difference between this analysis and the one conducted by Lipset and Zetterberg is that in this analysis both men and women have been included in the sample, whereas Lipset and Zetterberg utilized only male respondents. Thus an examination of the relationship between social mobility and

FIGURE 2: PER CENT REPUBLICAN OF TWO-PARTY CHOICE OF MALE VOTERS, BY SOCIAL MOBILITY CATEGORY, 1948-1966



The number of voters on which these proportions are based are, for the middle-class stables, upward mobiles, and working-class stables, respectively: 1948: 73, 70, and 109; 1952: 92, 77, and 138; 1958: 100, 77, and 143; 1960: 108, 87, and 181; 1964: 91, 79, and 132; 1966: 53, 50, and 59.

party choice with a further breakdown by sex of respondent should prove to be useful.

When an examination of the political orientations of voters by social mobility categories is conducted for male respondents alone, a far more complex picture emerges than that seen in Figure 1 utilizing all respondents. In contrast to the invariably smaller proportion of conservatives among the upward mobiles relative to the middle-class stables seen there, Figure 2 shows that in three of the six elections the upwardly mobile males were slightly *more* likely to be Republican than the nonmobile members of the middle class. Yet in the 1952, 1964, and 1966 elections the upward mobiles show a lower level of support for the Republican party than do the middle-class stables, although only in the 1964 election is the level of conservatism of those who had moved up into the middle class significantly lower than that of the middle-class voters born and raised in that class.

Since Figure 2 shows fluctuation from election to election in the relative proportions of Republicans among middle-class stable and upwardly mobile men, it would seem useful

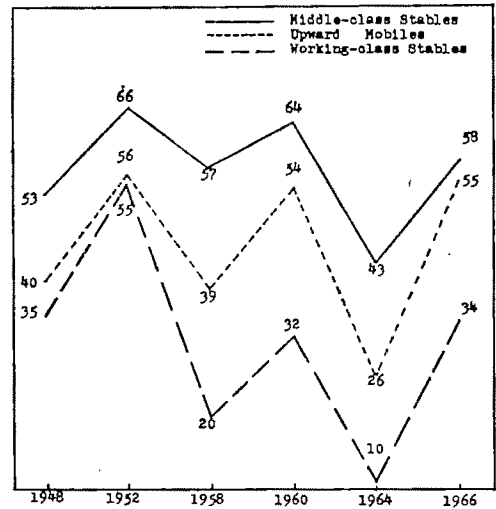
<sup>8</sup> Any attempt to account for the exceptional political behavior of the upward mobiles in 1964 has as its necessary point of departure the fact that the 1964 election had an unusually one-sided outcome. Perhaps in a year when Republicans were to go down to an overwhelming defeat, the middle-class individuals whose class backgrounds did not predispose them to vote Republican, i.e., the upward mobiles, were particularly sensitive to the unpopularity of the Republican cause.

to examine the political difference between the two mobility categories when the data for the six elections are aggregated. Among male voters, the difference in proportions supporting the Republican party over the six elections between upward mobiles and middle-class stables has been only an insignificant 2%, with the upwardly mobile men exhibiting only a slightly smaller proportion of conservatives than the stable men in the class to which the mobiles have risen.<sup>4</sup> Thus our samples seem to demonstrate that, among American men, upward mobiles tend to be politically indistinguishable from middle-class stables and much more Republican as a group than are the working-class stable men.

Figure 3 shows that the political orientations manifested by the upwardly mobile females in the samples under study differ rather markedly from those of upwardly mobile males relative to the class stables. Contrary to the findings in Figure 2, in none of the six elections do upwardly mobile women indicate a proportion of support for the Republicans greater than that of the women who were both raised, and remain, in the middle class.

However, considerable fluctuation over the six-election period is manifested in the degree to which the upwardly mobile women approach the pattern of party choice of the class-stable females. At one extreme, in the 1952 sample the upwardly mobile women were virtually indistinguishable in their distribution of party preference from women stable in the working class—differing by only one percentage point—yet during the 1966 congressional elections, only three per cent less of the upwardly mobile women chose the conservative party than did the middle-class stable women. But when the evidence from all six elections is considered, it is clear that upwardly mobile women tended toward a pattern of partisanship in-

FIGURE 3: PER CENT REPUBLICAN OF TWO-PARTY CHOICE OF FEMALE VOTERS, BY SOCIAL MOBILITY CATEGORY, 1948-1966



The number of voters on which these proportions are based are, for the middle-class stables, upward mobiles, and working-class stables, respectively: 1948: 107, 58, and 104; 1952: 119, 68, and 111; 1958: 87, 84, and 79; 1960: 156, 102, and 134; 1964: 102, 108, and 109; 1966: 57, 53, and 67.

intermediate between that typically exhibited by middle-class and working-class women who have remained in the classes in which they were raised. During the time examined here, the upwardly mobile women were, in the aggregate, less Republican (differing by 14 percentage points) than the women who were stable in the middle class and more conservative (by 13 percentage points) than working-class stable women voters.

Thus, in examining the relationship between social mobility and political partisanship, sex of respondent emerges as a highly influential variable. Among the class stables, whether they be nonmobile in the middle class or the working class, men and women do not differ markedly in their political orientations when the results from the six elections studied are aggregated. Among the working-class stables, women are slightly more Republican than men, 31% to 30%, while the same relationship holds among the voters stable in the middle class, 58% of the women being Republican compared to 57% of the men. But among the upward mobiles, the females are less Republican

<sup>4</sup> Since the 1964 election survey results differ from the others markedly, calculations were also conducted with data from this survey eliminated. However, little difference was found in the overall results, for upwardly-mobile and middle-class stable men diverged even less politically (by only one percentage point) in the five-election aggregate.



than men over the six elections, 44% in contrast to 55%.

The differing effects of the sex of respondent on political orientation for the various social mobility categories can also be viewed in absolute terms. A majority of both men and women middle-class stables support the conservative party over the six elections, while a majority of both male and female working-class stables support the Democratic party when all six surveys are considered. However, the sexes display a divergent degree of conservatism among the upward mobiles. Over the six elections for which data are available, a majority of the male upward mobiles indicate a conservative political choice, but a majority of the female upward mobiles demonstrate a liberal political orientation in party choice.

The inclusion of sex as a third variable in the analysis of the relationship between social mobility and political orientation does to some extent explain the discrepancy between the findings concerning this relationship in the United States presented here and those offered by Lipset and Zetterberg in 1956. Whether the sample of upward mobiles is confined to males alone or contains respondents of both sexes has an important bearing on the inferences which the researcher is likely to draw about the relationship between social mobility and political orientation. At the same time, the fact that in political orientation upwardly mobile males tend to resemble the male voters in the middle class to which they have risen, while upwardly mobile females tend much more to occupy a political orientation intermediate between those of class-stable females, should not obscure the conclusion reached in this paper that the upward mobiles in the United States exhibit no tendency toward being the social mobility grouping having the highest proportion of support for the more politically conservative party, contrary to what has hitherto been assumed for the American case.

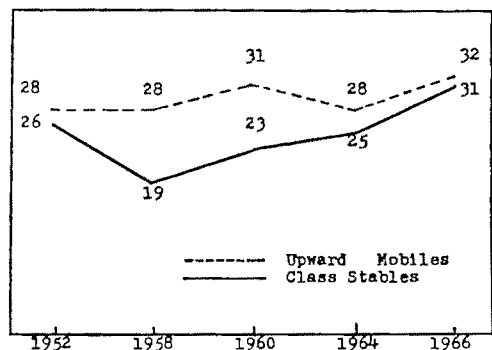
Another problem suggested by the literature on the political behavior of the upward mobiles remains for closer examination. Previously reported findings were contradictory regarding the question as to whether the upward mobiles are more or less "Independent" in their political orientation than the

class stables. This question is interesting from a theoretical viewpoint, for cross-pressure theory suggests that the upward mobiles are more likely to be subject to political cross-pressures than the class stables and, hence, should be expected to respond to these cross-pressures by failure to make a party choice to a greater extent than class stables. Thus, cross-pressure theory leads to the hypothesis that a larger proportion of the upward mobiles will indicate an "Independent" political orientation than the class stables.

To test this hypothesis, we are forced to select a dependent variable, as an indicator of political orientation, different from the one utilized up to this point in the analysis. In the United States, the voter is essentially offered a dichotomous choice at the polling place, with very little means of expressing an "Independent" political orientation in the vote. But if the respondent's subjective partisan identification is utilized as the indicator of political orientation, rather than his vote intention or recollection, then we have a measure of the extent to which the mobiles versus the class stables think of themselves as "Independents," rather than as "Democrats" or "Republicans."

Figure 4 presents this information in graphic form. It shows that over the period

FIGURE 4: PROPORTION OF RESPONDENTS CHOOSING "INDEPENDENT" AMONG THOSE MAKING CHOICE OF PARTISAN IDENTIFICATION, BY SOCIAL MOBILITY CATEGORY, 1952-1966



The number of respondents on which these proportions are based are, for the upward mobiles and class stables; respectively: 1952: 174 and 594; 1958: 264 and 657; 1960: 245 and 740; 1964: 236 and 572; 1966: 164 and 458.

for which data are available, the hypothesis that upwardly mobile citizens are more "Independent" in political orientation than class stables is generally supported. Even though the difference between mobiles and class stables in proportion of "Independents" is statistically significant in the 1958 and 1960 samples only, the rank ordering of the two groups of respondents is invariant on this dimension in the five samples drawn over a 14-year period. In those five samples the proportions of respondents identifying themselves as "Independents," rather than as partisans of either of the two major parties, average five percentage points greater among the upward mobiles than among the class stables.

These data, then, support both cross-pressure theory and West's (1953) finding that those who move upward are more likely to be "Independent" than the class stables. Correspondingly, they contradict the disparate finding by Maccoby *et al.* (1954) that upwardly mobile youths are less likely to be Independent.

#### *Summary of Findings and Implications*

This study set out with the primary purpose of re-examining the proposition that upward mobiles in the United States are even more likely to be conservative in political orientation than middle-class stables. The material presented here appears to contradict this contention. In the first place, findings in the literature on social mobility and party choice are sharply divergent, thus lending no consistent support to the proposition. In the second place, the evidence developed from the six elections from 1948 to 1966 indicates a slight tendency for American upward mobiles to exhibit a lower level of support for the more conservative party than the middle-class stables. While this finding was seen to be primarily attributable to the females making up the national sample, analysis of the male subsample failed to disclose any strong or constant support for the view that male upward mobiles are more likely than male middle-class stables to be conservative in political orientation.

A number of implications would seem to follow from the new findings presented here. First, the attempt to link differences between

certain aspects of European and American society to differential political effects of upward social mobility is obviously premature if the gross differences between the politics of upwardly mobile Americans and Europeans that had previously been assumed do not exist. Of course, prior to Lopreato's effort, theoretical explanations for the presumed cross-Atlantic difference in the effect of upward mobility on political behavior focused on reasons for the greater liberality of European upward mobiles, rather than the greater conservatism of American upward mobiles. However, Lopreato's application of the concepts of "excessive" emphasis on achievement and a "cult of gratitude" to the American scene completed the theoretical work begun by Lipset and Zetterberg, who had emphasized the political potency of status discrepancy and rejection of the upwardly mobile in the European setting. Thus Lopreato's work even more pointedly built on the assumption that upward mobiles in the United States are more likely to be conservative than the middle-class stables of this country, an assumption that now appears in doubt.

But even if the rank ordering of the conservative support—proportionally given by upward mobiles, middle-class stables, and working-class stables—is invariant in all the nations where such data exist, whether they be European or American, more subtle differences may well be found in the politics of the mobiles in various countries. It follows, then, that the cross-national analysis of social mobility and political orientation should be characterized by more refined measures of the political variable than has been the case until now. For example, the degree to which the intermediate rank-order position of the upward mobiles departs from the rank-order positions of the class stables could be expressed on a standardized scale and reported in future research on the topic. Possibly, European upward mobiles would be found to rank closer to the working-class stables than American upward mobiles on such a scale.

In addition to developing more discriminating measures of the political orientations of mobiles relative to the class stables, this analysis suggests the usefulness of utilizing a number of samples as the basis for general-

ization. To this time, most speculations on mobility and politics have been based upon one or, at best, two samples from a given nation. Yet, as Figure 1 demonstrates, the party choice of the mobiles relative to the class stables shows considerable variation from year to year within the United States. It is not unlikely that this range of variation would be found in other nations as well. Thus theories developed on the basis of data from only one sample are likely to be misleading.

Another conclusion which would seem to follow from this analysis is that the political orientation of the upward mobiles should be compared not only with that of the class to which they have risen, but also with the party choice of the class which they have left behind. Indeed, it may well be that the tendency of those doing research on social mobility and politics to concentrate their attention on a comparison only of the politics of the upward mobiles and the middle-class stables has led to confusion in the attempt to sort out the causal factors that influence political change.

Causal complexity is introduced by the fact that these two groups diverge on more than one dimension. Not only have the upward mobiles experienced social-class change while the middle-class stables have not, but also the two groups are distinguished by their disparate class origins. Thus any differences between the two groups cannot without further analysis be attributed exclusively either to factors associated with class change versus class stability on the one hand, or to influences stemming from divergent class backgrounds on the other hand. In each case, there is a failure to control for the potential influence of the other type of variable.

The difficulties which can arise from focusing attention exclusively on one of these two sets of factors when comparing upward mobiles with middle-class stables is illustrated by conclusions drawn by various sets of researchers whose reports have been reviewed previously. For example, Greenstein and Wolfinger found that the upward mobiles were less likely to be conservative than the middle-class stables in their sample. Concentrating only on class background differences for an explanation of this finding, they concluded that early political socialization of

the mobiles exercised an over-riding influence on their adult political orientation and that, therefore, upward mobility had no effect on partisan allegiance.

Yet other scholars, when faced with the problem of explaining differences in the politics of the upward mobiles and middle-class stables, have concentrated exclusively on explanatory factors associated with the individual's experiences *after* undergoing class change. Lipset and Zetterberg's status rejection explanations, Anderson's speculations about the continuing political influence of labor unions, and Lopreato's investigation of perceived restrictions on inter-class social relations have all presumed that the crucial variables influencing the politics of the mobiles are factors operating while the mobile respondent is an adult. They have ignored the potential influence of class background factors and the possibility that the political differences found between mobiles and stables were determined *before* class change ever took place.

Thus the analysis of causal factors underlying the social mobility and political orientation problem would be greatly advanced if the political orientations of the upward mobiles were not only compared to those of the middle-class stables, but were also compared to those of the working-class stables. In addition, a further aid to causal analysis would be the examination of longitudinal data on the partisan choice of mobiles and stables. While the independent variable, class change, in the relationship is treated in a manner which allows for analysis of change over time, the dependent variable, political orientation, commonly is not. But since we do not know at what point in the individual's life political changes have taken place, we cannot estimate with any degree of confidence the relative influence of pre-adult versus post-childhood factors which might be at work influencing the political orientations of the socially mobile persons.

This lack of longitudinal data plagues even the most ingenious efforts to unravel the causal factors involved in the class change and party choice research problem that have been made so far. For example, even Lopreato's skillful and imaginative testing of components of Lipset's "status rejection" theory suffers from a lack of longi-

tudinal data on the political variable. Although he finds that perception of class restrictions is associated with political orientation among upward mobiles in his Italian sample, in the absence of information on political change, his inference from that finding remains problematical.

Instead of concluding that those upwardly mobile Italians who did not perceive class restrictions tended to change to a conservative political orientation, while those who subjectively experienced such restrictions tended to retain a left-wing partisanship, it is equally possible that his independent and dependent variables can be reversed. That is, the relationship may be interpreted by inferring that those upwardly mobile Italians who are of Marxist or left-wing orientation are more likely to perceive class relations as closed because of their political orientation than are the upward mobiles who are conservative to begin with. In this interpretation, political change need not have taken place among the upwardly mobile Italians to produce the finding Lopreato presents.

#### *Interpretation of Findings*

In the absence of adequate longitudinal data on the political orientations of American mobiles and class stables, the explanations for the findings presented here on the political differences between male and female upward mobiles can only be suggestive. However, plausible alternative frameworks for interpretation can be developed.

Turning first to the possibility that factors coming into play after the experience of upward social mobility produce political effects, it is evident that men and women differ markedly in the likelihood that such factors will be politically potent for them. First of all, a large component of the women in the samples analyzed here are housewives and derive their statuses from the manual or nonmanual nature of their husbands' occupations. Being upwardly mobile by marrying a man who is in a social class higher than that of her father may well produce quite different political attitudes in a woman than climbing up to a nonmanual position in the occupational stratification system does in a man.

These differences might be due to the fact

that women who "marry up" are able to avoid some of the influences pressing toward a conservative political orientation that middle-class men typically experience. Such factors as the conservative political influence of some forms of professional training, the middle-class male's concern with family financial management, and the greater salience of politics as a discussion topic among men's work and recreational groups leading to a heightened awareness of middle-class political norms are all influences more likely to produce political conservatism among upwardly mobile men than women.

Even a sense of appreciation, or "cult of gratitude," toward the social order for providing the setting in which the mobile individual was able to achieve is more likely to result in political conservatism among men than among women who have been upwardly mobile. The woman who marries up is more likely to explain her upward mobility in terms of chance operating in mate selection or in terms of her personal charms, while the upwardly mobile male seems far more likely to translate his experiences into conservative politico-economic maxims.

But instead of attempting to account for the differing degrees of conservatism among male and female upward mobiles, in terms of differential forces pushing them toward the political right, the phenomenon might be explained in terms of unequal forces holding the mobiles to the presumably more leftist political orientation of their youth. Status rejection, the experience of being rejected from full membership in a higher status because of class of origin, is such a force. Status rejection theory assumes that the desire to emulate the behavior of the higher status groups is so strong that it is, in effect, a constant in all advanced societies. Thus variation in the degree to which mobiles adopt the political orientation of the middle-class stables is to be accounted for by factors such as status rejection negating the effect of this pervasive desire.

If this explanation for the political behavior of upward mobiles is valid, we are led to the hypothesis that, among the upward mobiles, women are less likely than men to adopt the conservative political orientation of the middle class because they suffer status rejection to an appreciably

greater extent than do males. There appear to be no obvious grounds for accepting or rejecting this notion. But even if women are more sensitive to status considerations than men, and that in consequence upwardly mobile females experience greater status rejection than upwardly mobile males, it is implausible that the consequences of this experience would take a political form. It seems unlikely that politics is of such salience to American women that experienced status rejection would lead upwardly mobile females to reject the conservatism of the middle class in favor of the more leftist partisanship of their class of origin.

Finally, none of the foregoing may be important in influencing the politics of male and female upward mobiles. It may well be that the differences in political orientations of the upwardly mobile men and women are to be accounted for by factors operating before the experience of upward social mobility rather than after.

For instance, it is likely that those offspring of working-class families who are to become upwardly mobile differ significantly *before* experiencing class change from those working-class children who will remain in their class of origin. Selective recruitment may result in an "over-selection" of conservatively-oriented individuals among the men who become upwardly mobile, with no significant degree of political change taking place among them subsequent to the mobility experience. A cluster of values may be held by some working-class boys such as an emphasis on individual responsibility, achievement, etc., associated both with heightened chances for upward social mobility and a greater tendency toward a conservative political orientation.

The same factors involved in the process of selection for upward mobility may not, however, produce a group of upwardly mobile women who are conservative to the same degree that upwardly mobile men are. This might be due to the fact that the combination of conservative political and social values is not as influential in determining the chances for upward social mobility through marriage as it is for mobility through the occupational sphere. If this is the case, the differences in the political orientations of male and female upward mobiles would

not be due to changes in their politics as a consequence of social mobility, but would be due to preselection of a certain "mix" of political types for subsequent mobility.

The great number of plausible explanations for the differing degree of conservative party support found among upwardly mobile men and women in this discussion is an indication of how little work has been done so far in sorting out the causal factors that influence the politics of the socially mobile persons. Thus, perhaps the major conclusion of this paper is that the variables involved in a comprehensive analysis of the topic are likely to be both more numerous and more complex in their interactions than had been previously assumed. If this is so, then only by utilizing measures of partisanship which can identify *when* in an individual's life political change takes place, and by adopting more sophisticated multivariate analytical techniques to study the problem, can major advances be made in understanding the causal relationships involved in the connection between social mobility and political orientation.

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## FUNCTIONS OF CHILDREN'S PERCEPTIONS OF THE STRATIFICATION SYSTEM

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*American Sociological Review* 1971, Vol. 36 (April):235-249

*The purpose of this study has been to examine children's perceptions of the stratification system and to consider the possible consequences of these perceptions for the larger social order. There has been an attempt to extend one aspect of Davis and Moore's classical stratification theory by exploring the status attitudes requisite for an adequate number of children to strive toward prestigious, socially-important occupations. A sample of 1,917 black and white children from grades 3-12 from Baltimore City were interviewed. Findings indicate that young children have developed a type of status consciousness that should facilitate such occupational striving: that is, a clear awareness of occupational prestige differences as early as third grade (based on a comparison with 1963 adult North-Hall ratings), and a continuing optimism about their own opportunities to achieve desirous occupations. In addition, several mechanisms are noted that appear to be dampening anger against the class system among the disadvantaged school children.*

ACCORDING to the functionalists' classic theory of stratification, occupational prestige and income differentials are vital in motivating persons to sacrifice the time and energy required to train for complex jobs functionally indispensable to the society (Davis and Moore, 1945). Stratification rewards are necessary to motivate persons to assume key occupational positions. Although Davis and Moore have not been explicit in this regard, their theory is particularly applicable to the motivation of school-age children. For obviously it is during the school years that the individual makes the decisions and formulates the aspirations and plans that propel him toward an ultimate occupational choice. It is the age group that has not yet made an occupational commitment to which this aspect of Davis and Moore's theory appears most relevant.

The mere *existence* of a system of unequal rewards and privileges is not, however, sufficient in itself to induce children to acquire the training and utilize the talents necessary for ultimate occupational functioning. Certain status and class perceptions and attitudes are also required in order to bring about the necessary motivation. The present analysis seeks to extend Davis and Moore's theory (1) by considering some status perceptions and class attitudes requisite for children to be motivated toward socially functional occupations, and (2) by describing the extent to which such attitudes are present in a representative sample of urban school children of various ages and social backgrounds. Other consequences of these children's attitudes for the maintenance of the class system will also be considered.

Systems which are important to society may very well require that individuals be socialized for them at an early age. Hyman's work (1959) indicates that political attitudes developed early are likely to persist; and similarly, one might predict that deep-rooted attitudes toward the stratification order formed in the early school years would be particularly likely to have later effects. For

\*The work of the first author is currently supported by a Research Development Award from the National Institute of Mental Health, #5-K1-MH-41, 688-02. The work was also partly supported by USPHS, Grants 1-F3-MH-41,688-01 and MH-197541-01. The authors are deeply indebted to Leonard I. Pearlin and Melvin L. Kohn for their constructive criticisms and suggestions.

this reason, the perceptions of children as young as third grade will be examined in this study. Although sociologists have been interested for decades in learning how adults perceive their stratification system (Warner, 1941; Centers, 1949; North and Hatt, 1953), and although adolescents have been included in the samples studied, relatively little attention has been paid to young children's class perceptions (see Stendler, 1949; Weinstein, 1958; Lehman and Witty, 1931; Gunn, 1964); and there has been virtually no consideration of the functions of these perceptions for the persistence of the stratification order.

#### METHOD

The data for this analysis were collected as part of a larger cross-sectional study of the development of self-image among children from Grades 3 to 12. Baltimore City was chosen as a typical large metropolis for the site of the study. A random sample of 2625 pupils distributed among 25 schools was drawn from the population of third to twelfth grade pupils in Baltimore City.

The sampling procedure is modelled after the U.S. Census Bureau's usual cluster sampling method.<sup>1</sup> In the present study, each school in Baltimore City was initially stratified by two variables: (1) the proportion of non-white students, and (2) the median income of its census tract. Twenty-five schools falling into the appropriate intervals were randomly selected. From each school, 105 children were selected by random procedures from the central records.

Some children had withdrawn from school subsequent to the compilation of the central records and were no longer available. However, we were able to interview 1917 children, that is: 79.2% of the sample children still registered in the school, or 73% of all the children originally drawn from the central records. Reflecting the population, the present sample is heavily Negro and somewhat skewed toward the working class. Comparisons across ages must also take account of the fact that school dropouts are absent from our population. Unless other-

wise indicated all of the findings presented in this analysis hold if race is controlled. Race and class are the only major status variables related to grade-level (i.e. there are more blacks in the lower grades). None of the findings presented here, however, were found to be spurious when controlled for race or class.<sup>2</sup>

Each subject was interviewed in his school for three hours directly after school. The interviews were divided into three sessions for the younger children and two sessions for the older.<sup>3</sup> For the elementary school children, objective background information was collected from the parents, since pretesting indicated that young children are unaware of the many facts basic to the sociologist. Parents were reached either by a 5-10-minute telephone interview, or in cases where there was no telephone, by home interview. Almost all parents were extremely cooperative and in only 60 cases were we unable to locate the parent or conduct the interview.

#### FINDINGS

##### *Awareness of Occupational Prestige*

In order for occupational prestige differentials to serve as an inducement to children to select and begin training for socially-functional occupations, the child must first be aware that there is an occupational prestige hierarchy and, furthermore, know what it is. The unequal rewards of prestige and desirability attached to various occupations will not function to assure that needed occupations are filled unless persons selecting jobs are sufficiently cognizant of these rewards to expend the necessary effort. The consensus among adults from varied social backgrounds, and from different technologically

<sup>2</sup> Any examination of the effect of other conditional variables on status consciousness (peer-group leadership, sex, race in more detail than is presented here) will be dealt with in future publications. None of these variables explain away the relationships reported here.

<sup>3</sup> The first hour of interviewing was identical for all subjects. However, the order of the two sections to be used in the final two hours was randomly alternated since we did not expect all subjects to finish all sections. Therefore, 82 subjects did not begin the section relevant to this analysis and 82 more did not complete the section.

<sup>1</sup> Appreciation is expressed to Joseph Waxberg of the U.S. Bureau of the Census for his consultation.

advanced nations, regarding the occupational prestige hierarchy is little short of remarkable (Inkeles and Rossi, 1956). But because the bases for such judgments are quite complex and intricate, one wonders to what extent this awareness penetrates the mind of the elementary school child.<sup>4</sup> In order to determine the level of this awareness, children in the present study were asked to rate 15 of the occupations used in the North-Hatt scale in 1963 (see Hodge *et al.*, 1964):

"This is a ladder with five steps. (GIVE CHILD CARD "C")

The TOP step means that *most people* think a job is the best kind of job, an excellent one. (POINT)

The SECOND step means that most people think a job is a good job. (POINT)

The THIRD step means that most people think a job is in the middle, that is, a fair job. (POINT)

The FOURTH step means that most people think a job is not so good a job. (POINT)

The BOTTOM step means that most people think a job is the worst kind of job, a poor job. (POINT)

Now I'm going to read you a list of jobs. I want you to tell me which of the 5 steps MOST PEOPLE think the job belongs on. Do you understand? All right, let's begin."<sup>5</sup>

The scoring procedure in the present research was exactly the same as that used in the NORC studies. One of the most striking findings of this study is how very early children come to learn the relative prestige ranking of occupations in the society. Table

<sup>4</sup> The researches of Weinstein (1958) and Lehman and Witty (1931) indicate that young children have some awareness of differences in occupational prestige. However, these studies do not indicate the level of this awareness or present comparisons between children and adults. See Gunn (1964) for somewhat more comparative data.

<sup>5</sup> The question wording differed slightly from the original North-Hatt wording to allow comprehension on the part of the younger children. Instead of the "general standing" of a job, we used the simpler but longer "which step most people think the job belongs on." Instead of "average," we used "in the middle, fair"; instead of "below average," we used "not so good a job." Similarly, as shown in Table 1, the occupational titles were sometimes simplified for the children. It is of course possible that the slight differences in wording might have affected the responses. However, the actual findings suggest that such effects are small. For a discussion of the methodological problems of the North-Hatt scale, see Reiss (1961, Ch. 2).

1 indicates that *as early as the elementary school stage, children rate occupations in an order almost identical to that of high school pupils and, indeed, of adult samples.* Using a Spearman  $\rho$  rank-order correlation, the rank orders of elementary school children and adults correlate .93, well beyond the .01 level of significance; and a Pearson product moment correlation of the absolute scores shows a correlation of .96. In fact, if we just consider the very youngest group of children, the third graders, we find a remarkable understanding of the American prestige structure. Their Spearman rank correlation with the adult data is .93 and the Pearson correlation is .94. Even at this early age, children recognize that some jobs are better or worse, more or less desirable and prestigious; and even though the glamor of the policeman or fireman or railroad engineer still may linger in consciousness, their occupational perceptions are those of adults.

These high correlations hold not only for middle-class, white children but for blacks and working children of every age. The fact that these relationships are just as strong at the junior and senior high levels, and hold equally within broad social class groupings, is demonstrated in Table 1. There are, to be sure, age differences in the rank accorded to individual occupations. But the two highest occupations—Supreme Court Justice and Doctor—remain on top over all ages; and the three lowest occupations—truck driver, garbage collector, and shoe shiner—preempt the last places, regardless of age. It is true that 59% of the elementary school children do not know what a Supreme Court Justice is, but those who do know have already learned that it is the job with the highest prestige on the list.<sup>6</sup>

While the *relative* occupational rankings of young children and their elders are strikingly similar, there are noteworthy differences in their *absolute* ratings. As a general

<sup>6</sup> The scoring procedure excludes subjects who do not know what the occupation is. Sixteen percent of junior high students and 6% of senior high students do not know what a Supreme Court Justice is; 18% of elementary school and 3% of secondary school pupils do not understand the word "minister." For the other occupations rated, there were no more than 1% "don't knows" for either the elementary school or secondary school children.



Table 1. Comparison of 1963 U.S. Adult Occupational Prestige Data to Children's Occupational Rating. Absolute Scores by Grade, Class, and Grade.

Occupation	Hodge 1963 Adults	Total			Total			Middle Class**			Working Class**		
		Total Elem.	Jr. High	Sr. High	Total Elem.	Jr. High	Sr. High	Total Midl. Class**	Elem.	Jr. High	Elem.	Jr. High	Sr. High
U.S. Supreme Court Justice	94	97	95	97	97	95	97	95	97	93	95	97	96
Physician-"doctor"	93	93	94	96	94	93	96	93	92	94	97	92	94
Minister-"minister of a church"	87	88	84	83	83	86	85	86	85	80	82	88	85
Airline pilot	86	83	83	84	82	84	84	82	84	79	82	83	85
Banker-"owner of a bank"	85	92	93	93	93	92	93	92	93	93	94	92	92
Public School Teacher	81	87	80	77	80	84	84	80	85	76	73	87	81
Owner of factory that employs about 100 people-"owner of a factory"	80	87	88	86	88	87	86	87	88	89	87	86	88
Policeman	72	85	74	67	75	79	76	79	85	70	62	85	76
Railroad Conductor	66	69	63	60	62	67	64	67	67	61	53	69	64
Mail Carrier-"mailman"	66	74	65	64	65	71	64	71	71	62	58	74	66
Machine operator in factory-"worker who runs a machine in a factory"	63	73	66	62	64	70	62	70	69	61	56	73	67
Truck driver	59	60	56	55	54	59	55	59	58	52	49	61	57
Clerk in a store-"clerk or sales person in a store"	56	72	66	64	67	69	64	69	70	67	61	72	65
Garbage collector	39	44	35	35	36	40	35	40	42	32	31	44	35
Shoe shiner-"person who shines shoes"	34	42	27	26	31	35	26	35	38	25	24	42	27
Sample size (N)	651	1030	517	331	416	1407	331	1407	192	122	102	751	410
Pearson product-moment correlations*** of 1963 scores and other column scores		.96	.97	.98	.97	.98	.98	.98	.96	.96	.96	.96	.98
Spearman rank-order correlation of 1963 scores and other column scores		.93	.92	.92	.92	.94	.92	.94	.88	.91	.91	.93	.93

\*The second item indicates the exact wording for the children in the present study. The first item indicates the NORC wording.

\*\*Middle class corresponds to Hollingshead SES scores of 1-3; working class to scores of 4 and 5.

\*\*\* All correlations significant beyond the .001 level.

pattern, young children show a more favorable disposition in rating jobs. They are less likely to say a job is a "poor" or "not very good" one. Almost all jobs look fairly good to them—some are just better than others. As they grow older, children give almost all occupations lower absolute scores, with the exception of some of the highest jobs. In fact, in all but two cases (doctor and pilot), i.e., in 87% of the cases, the elementary school children give the job a higher score than do the adults; and in 80% of the cases (12/15) they give higher ratings than the older children. Conversely, only 54% of the third grade children give any occupation a "poor" rating, as compared to 78% of the senior high students. (Chi square = 77.2876,  $df = 3$ ,  $p < .001$ .)<sup>7</sup>

Thus, at an early, impressionable age the adult's view of occupational prestige has been communicated to the elementary school child. While still quite young, the child is quite clearly aware of which occupations are better than others; but only as he grows closer to the time of actually entering an occupation does he realize how much better. If social attitudes acquired early tend to persist in later years, then it becomes more likely that those occupations considered prestigious today will continue to be desirable and to draw recruits in the future.

#### *View of Opportunity Structure*

A consciousness of the occupational prestige structure is not sufficient in itself to motivate adequate numbers of children to strive for prestigious occupations. The children must also believe that they have a *reasonable chance* to succeed in such aspirations; otherwise there is simply no sense in making the sacrifice to acquire the training and develop the talents which can never be

used. In a caste system, recognition that an occupation holds high prestige clearly will not be enough to induce striving on the part of a lower caste member. It would appear that a belief in equal opportunity for all, a belief that the class system is open, would be functional in inducing effort on the part of all talented children in our society.

#### *General Opportunity*

American ideology is oriented in this direction. In contrast to other attitudes concerning the stratification system which seem to be learned in the absence of explicit instruction, the doctrine of the "American Dream" is a matter of formal learning. In school, children are taught that all men are created equal, that everyone has an equal chance to get ahead, that opportunity is open to all with the talent and ability to grasp it.

In order to examine the power of this norm among our youthful respondents, we asked:

Do all kids in America have the same chance to grow up and get the good things in life, or Do some kids *not* have as good a chance as others, or Don't you know.

In our study, the children, both younger and older, do *not* appear to accept the doctrine of equality of opportunity. Although 32% of the elementary school children are unable to answer this question, those who do have an opinion almost always believe equal opportunity to be blocked to some types of children. Seventy percent of those elementary school children who understand the question believe that some kids do *not* have as good a chance as others. The major effect of age is to decrease the percentage of children who cannot answer the question, so that 70% of all high school children believe in barriers to equal opportunity. Of those children who indicate what type of children do not have an equal chance, 68% attribute the problem explicitly to socioeconomic, racial, or ethnic disadvantages.

For such a high percentage of young children to reject the American Dream—the American Myth that the patterns of opportunity are equally open to all—is quite dramatic. Many young children seem to be as

<sup>7</sup> Although in general the younger children use less of the five-point scale in rating occupations than do older children, it would not be accurate to say that a few children who do use the entire range are responsible for the high correlations found between elementary school students and adults. For a few children to be responsible, the majority would have to rate all occupations the same. However, 58% of elementary school children do use the entire range; 24% more use four of the five possible scale-points, 12% use three points, and only 5% use less than this.

Table 2. Opinions on Equal Opportunity<sup>a</sup> by Grade.

	Elem. School	Jr. High	Sr. High
Yes	20%	21%	20%
No	48	63	70
Don't Know	32	15	10
	(1033)	(518)	(335)

$$\chi^2=103.2, df=4, p<.001.$$

<sup>a</sup>"Do all kids in America have the same chance to grow up and get the good things in life?"

perceptive about the American opportunity structure as they are about the prestige hierarchy.

### *Personal Opportunity*

This attitude would appear to be a deterrent to motivation, particularly among the less advantaged children in our sample. If opportunity is restricted, there is apparently little point in striving toward prestigious occupations. If, however, we distinguish between the child's view of the opportunity structure in general and his view of his own personal opportunity, quite different conclusions will be reached. To measure the respondent's perception of his own life opportunities, we asked the following questions:

"Would you say you have a poorer chance, just as good a chance, or a better chance to rise in the world than most other people?"

"People like me don't have much of a chance to be successful in life. Do you feel like this?"<sup>8</sup>

"Do you think you could get any kind of job you would want to get?"

"Do you agree or disagree with this statement: 'I would like to get ahead in life, but I don't think I will ever get ahead as far as I would like'."

Although the majority of children do not believe in America as a land of equal opportunity for everyone and although the less

privileged are less likely to expect high success, the data would seem to show that the majority at every combination of age, race and socioeconomic level are fairly optimistic about their own personal prospects (e.g., see Table 3). It is of course, possible that the children are reacting defensively to these questions, and that their optimism is less than indicated by the data. But while comparatively few say their chances to get ahead are better than those of other persons, almost no one says they are worse. Ninety-seven percent say that they themselves have "a better chance" or "just as good a chance" to rise in the world as most other people; 86% seem to believe that people like themselves can be successful in life; 63% assert they will get ahead as far as they would like; and finally 52% claim they can obtain any job they want. The older children, closer to actually testing the job market, appear as optimistic as the younger.

When children who perceive general opportunity barriers are compared to others, no significant differences are found in their views of their own personal prospects. Although the child is persuaded that his society is characterized by a blocked opportunity structure, he does not appear to believe that he is one of those being blocked. The incentive for striving seems to remain.

### *Occupational Aspirations*

Of course, neither knowledge of inequality nor optimism about their own chances will assure striving unless there is legitimacy granted to the system of unequal rewards. Unfortunately we have no measure of the legitimacy accorded to the system. But if the system is perceived as legitimate, the combination of a high awareness of the occupational prestige structure and an apparently optimistic view of their own chances for occupational success should indicate high occupational aspirations on the part of these children. And, in fact, Table 4 shows that 64% of working class elementary school children and 69% of working class senior high students desire middle-class occupations above the clerical and sales level.<sup>9</sup> These

<sup>8</sup> For third and fourth graders, the words "to do well" were substituted for "to be successful"; this modification was required to make the item comprehensible to them.

<sup>9</sup> The findings persist whether we examine the occupations the respondents first say they desire; or whether we include only those occupational

Table 3. Percentage Holding Views of Personal Opportunity by Social Class.

Personal Prospects	1-2†	3	4	5	Total	N
Believe they have "as good" or "better" chance to rise in world than most	97	97	97	97	97	(707)†
*Disagree that "people like me don't have much chance to be successful"	95	91	87	80	86	(1573)
Believe they can "get any kind of job you want"	56	55	52	51	52	(1714)
Disagree "I don't think I will get ahead as far as I would like"	74	61	63	60	63	(800)†

\*Significant at .05 level.

†Not asked of elementary school children.

‡Defines upper end of scale.

working class respondents seem desirous of exceeding their fathers, by a rather wide margin; that is, they have significant intergenerational mobility aims. Further data show that by senior high school, 63% expect to be richer than their father, and 85% of working class boys desire an occupation of higher prestige (according to a comparison of the Hollingshead scores for father's occupation and their own desired occupations). Middle-class students are even more likely to aspire to high-ranking occupations, although not as desirous of upward mobility. (See Table 4—last three entries.)<sup>10</sup>

It should be noted that aspirations for intergenerational mobility rise with age. Table 4 shows that 18% of working-class elementary school children expect to be richer than their fathers in contrast to 63% of working-class high school children. Seventy-six percent of working-class boys in elementary school aspire to occupations with a higher Hollingshead prestige score than their fathers', in comparison to 85% in senior high. This finding, however, does not appear to be caused by a rise in the level

choices the subjects later claim they "really want," excluding the job choices considered "just a nice idea."

<sup>10</sup> A similar relationship between class and aspiration level is found in Rosenberg (1957). See Coleman (1966) and Lott's (1963:74ff) review of the literature for a discussion of the extent to which disadvantaged children hold unrealistically high aspirations. Our concern at this point is simply with aspirations and not with plans to actualize these desires.

of occupation selected for oneself as much as by a more realistic perception of one's father's success. As the child gets older, particularly the working class child, he sees his father as less successful. He assigns his father's occupation a lower prestige score, and is much less likely to believe his parents have done "very well" in life. As the maturing youngster realizes that his father's place in the economic and social structure is lower than he had thought, he consequently finds himself wishing to go beyond his family in income and prestige.

As predicted by this analysis, these mobility desires are associated with both a consciousness of the prestige structure and an expectation of personal opportunities for success. Table 5 shows a greater desire for upward mobility on the part of students who are more conscious of occupational prestige differences, that is, on the part of those who realize that some occupations have "poor" prestige. Among working-class secondary school students, for example, 53% of those who show this greater awareness expect to be richer than their parents, in contrast to 41% of the less aware. Similarly, 57% of these working-class teenagers who are more optimistic about their occupational prospects expect to be upwardly mobile in comparison to 47% of the less optimistic.

The type of status-consciousness developed by these children thus appears likely to contribute to the availability of an adequate pool of talent to fill currently prestigious, socially important jobs.

Table 4. Prestige Scores and Opinions by Grade and Social Class.

Topic	Elem. School		Jr. High School		Sr. High School	
	Midl. Class	Work. Class	Midl. Class	Work. Class	Midl. Class	Work. Class
Prestige Rating of Own Desired Job	87 (184)*	90 (713)	86 (110)	84 (372)	84 (89)	83 (231)
Prestige Rating of Father's Job	86 (184)	82 (694)	78 (112)	72 (365)	78 (88)	69 (223)
% Believing Parents Have Done "very well" in Life	67 (185)	67 (719)	48 (115)	39 (333)	44 (93)	33** (236)
% Expecting to be Richer Than Parents When Grown	19 (185)	18 (724)	35 (118)	43 (352)	52 (92)	63 (230)
% Desiring Jobs with Higher Hollingshead Score Than Father's Job	43† (192)	76‡ (751)	46‡ (122)	83‡ (410)	42‡ (102)	85‡ (246)
% Desiring Jobs Above Clerical & Sales Level (Hollingshead Score 1-3)	72† (173)	64† (675)	79 (106)	74 (372)	87‡ (90)	69‡ (226)

\*Excluding No Answers (NA's) and "Don't Knows" (DK's).

\*\*Difference between middle and working class significant at 10% level by  $\chi^2$  test.

† $p < .05$ , by  $\chi^2$  test.

‡ $p < .01$ , by  $\chi^2$  test.

Table 5. Percent Expecting to be Richer than Their Parents by Recognition of Occupational Prestige, and by Belief in Personal Opportunity

School and Class	Recognize that some occupations have "poor" prestige.		Belief** that personal occupational opportunities are:	
	No	Yes	Low (0-1)	High (2-4)
Elementary School:				
Middle Class	9 (69)	24* (111)	--	--
Working Class	13 (261)	20* (434)	--	--
Secondary School:				
Middle Class	36 (33)	45 (159)	35 (106)	51* (100)
Working Class	41 (128)	53* (443)	47 (350)	57* (254)

\* $p < .05$  for comparison within one class and grade level.

\*\*Based on a scale constructed from the four personal opportunity items detailed above. Scores vary from "0" (do not indicate belief in high personal opportunity on any of the four items) to "4." On the first item, "Would you say you have a poorer chance, just as good a chance, or a better chance to rise in the world than most people," children who said they had a "better chance" were opposed to the rest.

*Dysfunctions: Class Conflict*

An adequate functional analysis of any social element must also take account of its potential dysfunctions. This is conspicuously the case with the stratification system itself. Stratification is a system of rewards and privileges; and it is functional for the society that these differential rewards become known to societal members. But potentially this class-awareness can produce resentment among those accorded fewer privileges; and in the extreme can encourage efforts to overturn the stratification system itself.

A class system doesn't have the ideological option of a caste system to dampen conflict in the face of high class-consciousness—i.e., the ideology that one's inherited position is the morally just one. The ideology that would appear to be the best functional alternative is the belief in equality of opportunity, the belief that stratification rewards are justly distributed, that unequal rewards are dependent on unequal levels of talent and effort. But our data have already indicated that a high percentage of children reject this ideology.

Although these ideologies are not functioning to mitigate potential class conflict, a closer look at our data indicates several other mechanisms at work in this direction at least during the school years. First of all, it has been noted that a high percentage of working class youngsters are optimistically oriented toward upward mobility. As long as children expect future success themselves within the system, they are probably unlikely to rebel.

Secondly, it would be a mistake to discard the possibility that false consciousness is still operating to reduce class tensions, on the basis of the child's clear awareness of occupational prestige differences. There are many dimensions to class and status consciousness. If we confine our attention simply to the respondent's views of the occupational prestige structure and the opportunity structure, we can easily exaggerate the extent of the child's class awareness and the consequent problem for the system. As suggested by the discrepancy between the child's view of general opportunities and his perception of his own personal opportunities, the young person frequently does not seem

to apply his knowledge of the general structure to his perception of himself, his family, or his peers. As the following discussion indicates, false consciousness and low levels of certain types of status awareness appear in all age groups but are especially characteristic of the younger children.

*Self-Identification: Social Class*

As indicated earlier, although the elementary school child is clearly conscious of occupational prestige differences, he tends to inflate the prestige level of his father's job and the degree of his parents' success in life, more than do older students. (Table 4) These young children appear to be demonstrating in exaggerated form, the same ego-involvement shown by the adults in the original NORC study, who also rank their own occupations higher than does the population as a whole.

To further investigate this "false" consciousness, we explored the child's awareness of the concept of social class and his class identification. In describing the historical process of the development of class consciousness, Marx distinguished between *eine Klasse an sich* and *eine Klasse für sich*. A class *an sich*, the logically prior stage, was characterized by an awareness of a stratification system divided into broad gradations of power, with an incipient cognizance of their hostile interest. While presumably such consciousness grew out of direct experience in the economic marketplace, it could conceivably occur earlier through social learning. Seeking to ascertain the extent to which this has occurred, we asked our respondents:

"Have you ever heard about 'social class,' or haven't you ever heard this term?"

"What word would you use to describe the social class you belong to? \_\_\_\_\_"

"If you were asked to use one of these four names for your social class, what would you say you belong in? Would you belong in the

Upper class  
Middle class  
Working class, or  
Lower class"

As might be expected, Table 6 shows that whatever the younger children may understand about the *concept*, they certainly have little familiarity with the *term*. Although

Table 6. Percent Saying "Yes" to "Do you know what the word 'social class' means?"

Elementary School	15%	(1007)*
Junior High School	39	(511)*
Senior High School	75	(335)*

$\chi^2=428$ ,  $df=2$ ,  $p<.001$

\*Excluding NA's.

75% of the senior high school children have heard of "social classes," this is true of only 15% of those in elementary school.

Even among those children aware of the term, there is little sense of being submerged

or oppressed. Like adults, scarcely any children of any age or objective SES-level locate themselves in the "lower class." Even those children who are aware that their fathers have histories of unemployment do not choose this identification—only one out of 47 such children perceive themselves as lower class.

Moreover, even within the remaining three categories, the children elevate their statuses. When we compare the children's ratings to those of a recent nationwide sample of male adults (Kohn, 1969), we find that the children tend to overselect the middle class as their social locale (Table 7). And the elementary school children, in contrast to the older children, often select "upper class." Given the actual life circumstances

Table 7. Social Class Identification by Grade and Objective Social Class Background.

Class Identification	Elementary School			Junior High School		
	Middle	Work.	Total	Middle	Work.	Total
Upper Class	14%	30%	26%	5%	1%	2%
Middle Class	71	44	50	81	69	74
Working Class	14	23	22	14	29	23
Lower Class	-- (21)*	3 (97)	2 (136)**	-- (62)	2 (132)	1 (201)**
Dif. in %'s identifying with work. or lower class	-12 = 14-26			-17 = 14-31		

Class Identification	Senior High School			Adults from Kohn's Sample (1969)		
	Middle	Work.	Total	Middle	Work.	Total
Upper Class	8%	1%	3%	2%	3%	2%
Middle Class	80	65	70	81	43	60
Working Class	12	32	26	17	52	36
Lower Class	-- (83)	2 (161)	1 (246)**	-- (1358)	2 (1679)	1 (3074)
Dif. in %'s identifying with work. or lower class	-22 = 12-34			-37 = 17-54		

\*Not including DK's and NA's.

\*\*Includes cases in which we cannot classify children according to their objective social class background.

of most of these children, this designation, from the adult perspective, appears bizarre or whimsical.

As children grow older, they are more realistic about their placement. At the elementary school level, more working-class than middle-class children (26% vs. 14%) identify with the working or lower classes; at the junior high level, this difference is 17 percentage points (31% vs. 14%); in senior high, 22 percentage points (34% vs. 12%); and in Kohn and Schooler's adult sample, 37 percentage points (54% vs. 17%). Increasing age brings increasingly accurate perception of one's location in the stratification system. However, in comparison to working class adults, the teenage working class children still place themselves high. Similar findings were reported in a study by Centers (1950) comparing high school students to adults. This failure to realize or admit the full extent of one's deprivation would be expected to help counteract pressures toward class conflict.

#### *Peer Comparisons*

The examination of still other dimensions of the young child's status awareness also illustrates his difficulty in applying the knowledge he has to his own environment. His sensitivity to occupational prestige differences does not indicate an equal sensitivity to distinctions of wealth, status and privilege among his age-mates.

We asked our respondents:

"Do you know any kids whose families are richer than your family?"

"Do you know any kids whose families are poorer than your family?"

Table 8 shows that the younger children, unlike the teenage respondents, have little ability to perceive economic differences among their peers. Fewer than 35% of elementary school children are able to identify a youngster who is richer or poorer than they in contrast to over 70% of senior high school students. The growth in consciousness seems to be a regular one, with each grade level more able to perceive such differences than the grade before.<sup>11</sup>

<sup>11</sup> In this study there was no way to measure the accuracy of their perceptions. Stendler (1949)

Table 8. Percent Perceiving Economic Differences among Peers by Grade.

Grade	Do you know any kids whose families are:	
	Richer	Poorer
Elem.	25	34
Jr. High	46	59
Sr. High.	71	73
	(1896)*	(1886)*
	$\chi^2=236$	$\chi^2=184$
	df= 2	df= 2
	p<.001	p<.001

\*Excluding NA's.

Part of the growth in status awareness is undoubtedly due to simple maturation, to an enhanced ability to make fine distinctions. However, there appears to be another mechanism operating to reduce status consciousness among the younger children, namely the social homogeneity of their environment. Elementary schools are characteristically much smaller than secondary schools and draw from a narrower geographical area. As a consequence the school is likely to be characterized by a narrower socioeconomic and racial dispersion. Using the 5-point Hollingshead SES score for heads of households, we find that in the elementary schools the standard deviation is 0.81; in the secondary schools, 0.89. Nine percent of elementary school children in the Baltimore school population attend racially-mixed schools (schools which are 30%-70% white) in contrast to 21% of secondary school children. Hence younger children have less opportunity to actually encounter children who are richer or poorer than they or who are socially different.

The relevance of the changing school experience is reflected in the respondent's answers to the question of where they know

shows that older children are more accurate in estimating the economic status of their peers, with first graders inaccurate two-thirds of the time and eighth graders accurate over 75% of the time.



these richer or poorer children from. The younger respondents who can identify such peers are more likely than the older to say they know them from their *neighborhoods*, the older more likely than the younger to say they know them from *schools*.

### *Differential Class Consciousness*

Thus, although a knowledge of occupational prestige differences has been well-rooted in the consciousness of elementary school children, certain other types of status and class awareness are lacking in these early years. Some false consciousness persists into the high school period also. But along most dimensions, the teen-age student appears to grow much more cognizant of the actual stratification order and his family's place within it. At this point in secondary school, when the youth is less able to inflate his picture of his socioeconomic status, disillusionment and even anger against the system might be expected on the part of the disadvantaged. Further analysis indicates that the full potential for class militancy among the disadvantaged is not being realized even at the secondary school level.

The degree of class or status consciousness of advantaged and disadvantaged children was compared along the following dimensions: (1) recognition that occupations differ widely in social prestige (indicated by the fact that the child sees at least one job as having "poor" prestige); (2) comprehension of the term "social classes"; (3) personal awareness of children who are poorer or richer; and (4) recognition of the existence of barriers to equal opportunity.

At an early age there is often not much difference in the level of class consciousness of black and white children, or of middle and working class children. As the children grow older, however, substantial differences in degree of class consciousness appear. It is important to note that it is the *advantaged* children, not the *disadvantaged*, whose class consciousness appears to increase most. Class consciousness increases with age for both advantaged and disadvantaged children, but the rate of increase is sharper among whites and middle-class children than among blacks and working or lower-class children.

In Table 9, race and class are considered

jointly as they affect the development of class consciousness over age. For all the dependent variables shown here, the most advantaged child—the white middle-class youngster—is more conscious of the class and status system by secondary school than the least advantaged child—the black working-class student.<sup>12</sup> Usually the difference is considerable.<sup>13</sup> For example, 74% of middle-class teen-age whites understand the term "social classes" in contrast to 48% of working-class blacks; 80% of middle-class whites can identify a richer child in contrast to 49% of working-class blacks; 75% of the most advantaged know a poorer peer as opposed to 62% of the least advantaged; 74% of the most advantaged recognize barriers to equal opportunity as opposed to 59% of the least advantaged.<sup>14</sup>

With the advent of black power ideology, the claim has been made that the disadvantaged young, particularly the blacks, have become more militantly class conscious (e.g., Rustin, 1970). Our data do not allow us to determine whether the disadvantaged are more class conscious now than similar children of the same age were in the past. However, it is apparent that those children who are penalized by the stratification order today and whose prospects are least good are less conscious of its nature than those who at present benefit most from the system and whose prospects are brighter. Whatever has been occurring over the past few years, today the black working-class high school student appears much less class and status conscious than his age-mates.

<sup>12</sup> A Pearson correlation matrix indicates fairly high correlations between these five indicators of status consciousness. The lowest correlation between items is .71; all the other items correlate at .78 or better, with .88 the highest correlation. However, since a major point of this analysis is that the different dimensions of class or status consciousness have different theoretical implications, the findings are presented separately for each dimension at this point.

<sup>13</sup> The relative impacts of race and class are not identical from dimension to dimension in Table 9. The overwhelming finding, however, is the difference between the extremes—between the white middle-class and the black working-class child.

<sup>14</sup> Since this is a cross-sectional study rather than a panel, we cannot be certain of our assumption that the differences between age-groups really represent change.

Table 9. Percent Reflecting Status Consciousness by Most Advantaged (MA), Least Advantaged (LA), and In Between (IB).<sup>†</sup>

Topic	Elementary School			Secondary School			Percentage Pt. Dif. between Elem & Second.		
	MA	IB	LA	MA	IB	LA	MA	IB	LA
Rating a job "poor in prestige"	62 (90)	68 (276)	57* (534)	85 (139)	83 (291)	74** (379)	23	15	15
Understanding term "social class"	10 (90)	13 (273)	15 (519)	74 (141)	50 (276)	48*** (378)	64	57	33
Knowing richer child	38 (92)	29 (278)	21*** (540)	80 (141)	55 (302)	49*** (383)	42	26	28
Knowing poorer child	36 (91)	40 (278)	32 (538)	75 (138)	66 (301)	62* (380)	39	26	30
Believing all do not have same <sup>a</sup> opport. <sup>†</sup>	56 (92)	55 (273)	43** (542)	74 (141)	71 (297)	59*** (382)	18	16	16

\*Differences between MA, IB, and LA for each variable significant at 5% level by  $\chi^2$  test.

\*\*Differences between MA, IB, and LA for each variable significant at 1% level by  $\chi^2$  test.

\*\*\*Differences between MA, IB, and LA for each variable significant at .1% level by  $\chi^2$  test.

<sup>†</sup>The other children either believed in equality of opportunity or answered "Don't know."

<sup>a</sup>Most Advantaged (MA) signifies middle-class whites; Least Advantaged (LA) signifies working-class blacks.

There is one further effect that bears upon this issue. Class consciousness may be a precondition for class action among the less privileged; but it may also have a radically different outcome, viz., an intensified desire to achieve success within the society. In other words, a class-conscious person who is disprivileged may consider the stratification system unjust, and wish to overturn it; or he may, on the contrary, seek to move up in the stratification hierarchy as far as he can. Table 10 shows that, among lower or working class children, those who are more class and status conscious are considerably *more* eager to exceed their parents in wealth than those with lower status consciousness. Sixty-four percent of working class teenagers who are very conscious of class and status differences expect to be richer than their parents, in contrast to 32% of the least conscious. Their class consciousness may thus lead them to protect, rather than to challenge, the stratification order at least during the school years. It is possible, however, that if these

high-aspirations are later frustrated in the search for a job, more rather than less tension will result, as compared to a situation in which aspirations have always been low.

#### SUMMARY AND CONCLUSION

The purpose of this study has been to examine children's perceptions of the stratification system and to consider the possible consequences of these perceptions for the larger social order. We have attempted to extend one aspect of Davis and Moore's stratification theory by exploring the status attitudes requisite for an adequate number of children to strive toward prestigious, socially important occupations. In this sample of urban school children, it was found that students have developed a type of status consciousness that should facilitate such striving: as early as third grade they have a surprisingly clear picture of the occupational prestige hierarchy; and as late as senior high school the majority appear optimistic about their

Table 10. Percent Expecting to be Richer than Parents by Degree of Class or Status Consciousness.\*

School & Class	Low	Med.	High	Test of Null H
Elementary School:				
Middle Class	13 (112)	21 (47)	42 (24)	$\chi^2=10.4$ $p<.01$
Work. Class	13 (434)	21 (169)	31 (113)	$\chi^2=21.7$ $p<.001$
Secondary School:				
Middle Class	20 (40)	36 (31)	51 (135)	$\chi^2=13.0$ $p<.01$
Work. Class	32 (173)	46 (154)	64 (282)	$\chi^2=43.0$ $p<.01$

\*An index was constructed in which the following factors were considered indicative of class or status consciousness: recognition that some jobs were rated poor in prestige; understanding of social class; ability to recognize richer or poorer peers. A score of 0-1 indicates low class-consciousness; 2, medium; and 3-4, high.

own personal chances to achieve desirable jobs. This knowledge of the occupations that carry high rewards, combined with the expectations that such occupations can be attained, seems to encourage high mobility aspirations.

The acute consciousness of occupational prestige differences shown by these children has raised the question of possible dysfunctions. Will the degree of class consciousness among deprived children be conducive to later class conflict? It was noted that certain mechanisms appear to be operating to reduce such potential class conflict during the school years. Most important is the optimism of these children and their high motivation toward personal upward mobility within the system. It is unlikely that persons committed to success within the structure will rebel against it, at least as long as the commitment persists.

Second, a consideration of other dimensions of class and status awareness reveals a considerable degree of false consciousness, particularly among the younger children. The younger children show an exaggerated tendency to enhance their family's class position,

occupational prestige, and success in life. They are frequently unable to perceive status differences among their peers. Anger against the system would be expected to be at least partially deflected by the inflation of one's own position, and the failure to relate knowledge of the status structure to one's own environment. Although the older children exaggerate their own class position, their class consciousness along most dimensions has increased substantially.

This class consciousness, however, seems to have developed more sharply among the privileged, who have an objective stake in maintaining the system, than among the oppressed, whose objective interests may be hostile to the extant social order. In senior high school, black working-class students are less likely than their white middle-class peers to recognize that some occupations are rated "poor" in prestige, to understand the meaning of the term "social class," to be able to distinguish richer and poorer peers, or to believe that there are barriers to equal opportunity.

The reasons for the higher degree of status consciousness among the socially advantaged remain to be explored. First of all, motivations for self-enhancement may be operative. It certainly may be psychologically easier to recognize differences which are flattering to the self than to become conscious of a system in which one has a low, unprestigious, disadvantaged position.<sup>15</sup> Secondly, in a city which is so heavily black and working-class, it is possible that the middle-class child has exposure to persons from a wider range of occupations than the "ghettoized" working-class child, or for that matter, than the more isolated suburban middle-class child. Based on the proportions in our sample, it would appear that the majority of the middle-class Baltimore children attend schools in which they are outnumbered by working-class peers; whereas the majority of working-class students are in schools where over 85% of the children are also from blue-collar homes. In our sample, no middle-class student is in a school in which less than one-third of the student body is working-class. This differential exposure to children of a different class

<sup>15</sup> See Simmons (1969) for experimental support for this hypothesis.

level could help explain the greater status awareness of the advantaged children in Baltimore.

There are two additional important empirical questions that cannot be answered with these data. Although urban disadvantaged teenagers are less class conscious than their middle-class age-mates, are they more class conscious today than in the past? Secondly, what happens to the class attitudes of these urban children after they leave school and test out the job market? During the school years, disadvantaged children have become optimistically oriented to upward mobility, and they do not appear to have developed extremely militant class attitudes. Once they meet job-barriers, is the resulting frustration felt more acutely and the development of class-militancy sharply accelerated? What are the attitudes of those children who have dropped out of school to enter the job-market early, and who therefore were not in our sample?

In studies of adults (Rytina *et al.*, 1970; Erskine, 1969), there is evidence that in comparison to more advantaged citizens, the black and the poor have become more aware of inequality of opportunity and less satisfied with their own life and their own chances for getting ahead. However, differences in the populations sampled (particularly in terms of their urban, non-urban composition) make it difficult to compare some of these studies directly to ours. A clear contribution could be made by a panel analysis of the class attitudes of a cohort of disadvantaged youngsters as they move out of school into the first years of job hunting.

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# STUDENT UNIONISM IN THE NETHERLANDS: AN APPLICATION OF A SOCIAL CLASS MODEL \*

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American Sociological Review 1971, Vol. 36 (April):250-263

*A modified Marxian class theory is applied in an analysis of changes in the market situation of the Dutch student, as well as in his work and status situations during recent decades. On the basis of this analysis, five hypotheses are formulated as regards the determinants of student support for the union movement in Dutch universities. These hypotheses are tested; the analysis of results of local student-council elections and a secondary analysis of survey data corroborate these hypotheses. Student unionism appears to flourish particularly among students who (1) are of middle- or lower-class origin, (2) receive government assistance, (3) attend the larger universities, (4) do not belong to the traditionally prestigious student associations, or (5) belong to but do not participate intensively in such associations.*

BESIDES constituting a specific theory concerning the rise of the labor movement, Marx's notions about a *Klasse an sich* (a class in itself) and of the mechanisms by means of which such a *Klasse an sich* is transformed into a *Klasse für sich* (a class for itself) can serve as the components of a general theoretical model concerning the origin and consolidation of promotion-of-interests movements. For an adequate insight into the dynamics of such protest movements<sup>1</sup>, it is preferable to use the concept of class not in a strictly Marxian sense but rather in the modified version of Dahrendorf (1959: e.g. 136-141, 201-205), who proposes that the (latent or manifest) conflict of power interests between authority holders and those subject to their authority within the context of a society or of an

institutional order be stressed as the defining characteristic of an inter-class relationship.

Of course, it is a moot point whether one should follow Dahrendorf in substituting a rather exclusive emphasis on the conflict of *power* interests for Marx's overaccentuation of the conflict of *economic* interests. Class conflict presumably takes place in an authority context. However, the bone of contention between rulers and ruled may equally well be power distribution for its own sake as power distribution with a view to privileges, material gains, status distinctions, and opportunities for work and leisure.

Finally, we should also broaden the Marxian approach by taking into account social processes and cultural values that facilitate or hinder the spread of class consciousness and class-based ideologies. The operation of a status hierarchy and of reference-group processes must be analyzed in interaction with the distribution of power, economic, and other interests.

The possibilities of this approach can be demonstrated on the basis of an analysis of student activism in The Netherlands in the late Sixties. Although student protest in the United States originated as part of general social movements (Civil Rights; opposition to the Vietnam war; see e.g. Dunlap, 1970, for a survey of the literature concerning the issues of U.S. student protests), in The Netherlands student activism originally took the form of "bread-and-butter unionism"

\* This article is a revised version of a paper read at the Seventh World Congress of Sociology, Varna, Bulgaria, September 1970. My thanks are extended to all those persons and organizations who supplied me with the data on which the secondary analysis of this project was based. I am also indebted to Mrs. W. D. J. Docter, Mrs. H. A. Peeters, M. J. Koornstra, and W. J. van den Ende for their advice and help in analyzing the data, to Professor Ahrend Lijphart for his valuable criticism of an earlier version of this article, and to Miss M. E. Broks and Mrs. I. Seeger for their clerical and editorial assistance.

<sup>1</sup> Protest movements may be defined as: "collective actions by a group to express their discontent and/or to promote or to resist change" (see Lammers, 1969:559).

(cf. Pinner, 1964:187 ff.; Lammers, 1968: 12-21). To account for this development, we will examine changes in Dutch university and student life in the past century on the basis of the class model outlined above. Some specific hypotheses will be derived from this interpretation and tested on the basis of an analysis of election results and survey data.

#### CHANGES IN UNIVERSITY AND STUDENT LIFE <sup>2</sup>

As a convenient classification of variables playing a role in this context, Lockwood's distinction between market, work, and status situation will be used for the explanation of the rise of student unionism in The Netherlands.

##### *The market situation*

Lockwood (1958:15) defines the market situation as the "economic position narrowly conceived, consisting of source and size of income, degree of job security, and opportunity for upward occupational mobility."

In the past, the vast majority of all Dutch students belonged to the well-to-do aristocracy and depended for their support on parents, relatives, and (particularly in the nineteenth century) their creditors. However, particularly since World War II, a steadily growing percentage of students have enjoyed a government subsidy and profited from various kinds of public funds subsidizing student housing, student restaurants, student recreational facilities, and the like (Lammers, 1968:13).

In addition, the "means of production"—all kinds of educational equipment—are furnished predominantly by the state, whereas in former times the relatively simple tools of learning (books, microscopes, etc.) were of course mostly owned by the student himself.

To a degree hitherto unknown in the history of Dutch universities, the students' "opportunity for upward occupational mobility" too is controlled by academic authorities. In the 19th century young men went to college to consummate (if they were of noble or aristocratic birth) or to acquire (if they

came from middle-class families) the life style of the Dutch gentlemen, of the *Regenten* (originally the name of the Dutch commercial elite which ruled the Republic in the 17th and 18th centuries, later used generically to designate the Dutch upper classes).<sup>3</sup> Today students depend for success in society far more on the quality of their professional training, and thus to some extent on their teachers.

Professors control students' opportunities for upward occupational mobility more than ever before, not only indirectly by way of the quality of their training, but also directly as employers of their own graduates and as mediators between prospective outside employers and their graduates.

The changes in the market situation of the Dutch student during recent decades might be summarized as a conspicuous shift in the source of control of student destinies from a *large, unorganized, and dispersed* mass of parents, relatives, and creditors to a *limited* number of *organized and concentrated* public and academic officials.

Marx repeatedly draws attention to the importance of a more or less visible and organized "counter class" as a necessary condition for the transformation of the *Klasse an sich* into a *Klasse für sich* (e.g. 1953: 367, 195, 522-523; see also on this point Weber, 1947:179; Dahrendorf, 1959:133-136; Lipset, 1968:301; Bendix and Lipset, 1954:31; Ollman, 1968:577-578).

On this basis, it appears justified to state that at least a sizable part of the Dutch student body has gradually come into a class position *vis-à-vis* a "counter class" of officials on whom they are socioeconomically dependent.<sup>4</sup>

<sup>3</sup> For the *Regenten* and their style of life, see Boxer, 1965; Chapter 2; for the persistence of the *Regenten* mentality, see Daalder, 1966.

<sup>4</sup> It must be kept in mind that the whole system of higher learning in The Netherlands is in fact quite centralized. Therefore, public and academic authorities are forced to formulate and execute their policies within the limited scope of universalistic rules applicable to all universities. As already pointed out by Weinberg and Walker (1969), this more or less puts students in a class position *vis-à-vis* the centralized authority and fosters the rise of a central student union. However, in a strictly Marxian sense, this sort of authority is not an authentic example of a "counter class," for it is not deliberately organized to counter student demands,

<sup>2</sup> In the following account, the impressions of Dutch university and student life in former times are derived from van Arkel, 1950; Brom, 1923; Huizinga, 1951; de Vrankrijker, 1939.

### *The work situation*

According to Lockwood (1958:15), the work situation can be delineated as "the set of social relationships in which the individual is involved at work by virtue of his position in the division of labour." Before 1940, to the extent that they worked, i.e. studied, at all, Dutch students did so in isolation, in their rooms; only a minor part of their working time was spent in lecture rooms, laboratories, etc. The old fashioned teacher-centered lecture, of course, implied almost no interaction between student and student or between professor and student. Only in laboratory sessions, colloquia and the like did significant social relationships arise "on the shop floor." There, presumably, a master-journeymen or (for charismatic professors) a prophet-disciples relationship prevailed.

All that has changed. The average Dutch university, which had fewer than 600 students in 1900, now has about 6,000. The average number of students per professor rose from 16 in 1900 to 51 in 1966 (Lammers, 1968:16). Meanwhile, since 1945 a whole hierarchy of differentially ranked "scientific collaborators" has been inserted between the professor and the student. Thanks to their influx, it has to a considerable extent been possible—in spite of the growth of the student body—to replace the classic one-way lecture by many kinds of group-learning activities. Finally, collective and written tests have been introduced on a large scale and have been substituted for individual and oral examinations.

Consequently, present-day students have much more frequent contact with one another in the context of their studies than did their predecessors. Since all students in one department who start in the same year have to follow very much the same program, the chances are that many peer groups will develop. Such peer groups are in general no longer centered around a beloved (or hated) professor but are more likely to be based on similar personal and social characteristics and anchored within the much larger collectivity of departmental classmates (often numbering 100 or more students) who are subject to the same bureaucratic regimes of regulations, examinations, etc. as well as

being subordinate to the same hierarchy of professors and their collaborators.

The parallel between this picture and the development of small-scale industry into large-scale industry has often been noted (as early as Weber, and later on by Plessner, Habermas, and Schelsky; for a summary see Schelsky, 1963: Chapter 14 and 212–214). As Marx (1953:381) observed, ingroup feelings are bound to unite the journeymen working for the same master and to prevent association with other journeymen on a class basis. Also, such journeymen aspire to become masters themselves, and therefore accept the *status quo* and adopt their masters as a positive reference group (on this latter point see also Lipset, *et al.*, 1956:151 ff.). In large-scale industry, however, such patriarchal relations are replaced by a quasi-military discipline under which the laborers, like soldiers, are supervised by a whole hierarchy of noncommissioned and commissioned officers (Marx, 1953:533). Finally, the concentration of large masses of workers in big factories facilitates interaction and organized resistance among them (Marx, 1953:522).<sup>5</sup>

In all these respects the similarity between university and industrial trends is obvious. We may conclude that changes in the work situation of the Dutch student have greatly increased the chance that they will associate on a "horizontal," class basis, while opportunities for a countervailing ideological influence of the master-professor on the journeyman-student have diminished.

### *The status situation*

The status situation is defined by Lockwood (1958:15) as "the position of the individual in the hierarchy of prestige of society at large." Since we are dealing here with a class in *statu nascendi*, not in society at large but within student society, it seems

<sup>5</sup> In The Netherlands, interstudent competition for admission to certain departments or universities and for grades is probably less intensive than in the American situation. This state of affairs is of strategic importance as a condition for unionism, for, as Marx emphasized (e.g. 1953:522, 535), competition among workers is one of the main obstacles that a class movement has to overcome.

appropriate to focus in this context on the hierarchy of prestige in student society. The traditional student society in The Netherlands can be viewed as a stratification of organizations (for this approach, see Stinchcombe, 1965). Three strata can be recognized: (1) The *Corpora* and the women's student leagues affiliated with the *Corpora*. (2) The (Roman Catholic and Calvinist) denominational associations. (3) Other non-denominational associations. Below these strata consisting of organizations, a fourth stratum of students, not organized in such an association, must be discerned. These have been disparagingly called (4) The *nihilists*, a kind of outcasts.

Such student organizations are called *gezelligheidsverenigingen* (which means something approximating "sociability associations"; see Pinner, 1964:187-188, and Pinner, 1968:143-144) and generally have a much larger membership than do American fraternities or sororities. At every university there is normally just *one* (and only one) *Corps*, one league of female students, *one* Roman Catholic and *one* Calvinist, and one or more other associations.

In descending order from (1) to (3), the strata just mentioned enjoy decreasing prestige, because in this order the associations are more recent,<sup>6</sup> recruit their members to a lesser extent from Dutch upper-class circles, and adhere less closely to the prestigious culture pattern of the Dutch *Regenten* aristocracy. In all these respects, of course, the *nihilists* have the least status.

This prestige hierarchy is (was) solidly founded on the rock of a power hierarchy. The *Corpora* disposed of sufficient numbers of candidates for all key functions in student society at large (recreational clubs, student councils, professional clubs organized along departmental lines, etc.). Thanks to their upper-class anticipatory socialization and to their financial resources, many more *Corps* members than other students were able and willing to play a role in organized student life. Furthermore, thanks to their family

connections, social skills, and mastery of the *Regenten* style, *Corps* members were undoubtedly far more effective than other students in dealing with academic and civil authorities, usually exponents of the *Regenten* style themselves. All this was true to a lesser extent for the other *gezelligheidsverenigingen* and least of all for the *nihilists*.

This whole prestige and power hierarchy has been undermined. Due to the tremendous growth of the student body after World War II, the average size of most of the associations increased enormously. Presumably, this trend has weakened the capacity of the *gezelligheidsverenigingen*, particularly of the *Corpora*, to socialize middle-class students in the traditional way of life of the Dutch aristocracy. Moreover, there is evidence (Lammers, 1968:14-15) that not only the absolute number but even to some extent the proportion of students from nonelite strata in Dutch society have increased. Consequently, the number and percentage of *nihilists* have risen, while at the same time reduced cohesion and increased cultural heterogeneity of the *Corpora* are probably responsible for only partial enculturation with respect to the traditional norms and values of the *gezelligheidsverenigingen* on the part of an increasing proportion of their members.

In the student community before about 1960, various mechanisms of social control implied in the prestige and power hierarchy of the student associations militated against the spread of any kind of progressive ideology, let alone revolutionary ideology. At present, great numbers of students are no longer "vertically" integrated into the traditional stratification system, while a great many others, although to some extent "vertically" integrated, have not or not quite interiorized the prevalent *Regenten* culture. In other words, there arose a category of students ready to become integrated on a "horizontal" class basis.

#### *The Klasse an sich becomes a Klasse für sich*

The changes in the market, work, and status situations of Dutch students clearly created a *Klasse an sich*. The transformation

<sup>6</sup> The reasons brought forward by Stinchcombe (1965:148-150) to explain why older organizations stand a better chance of survival than young ones may also be referred to in explaining a correlation between organizational age and prestige.



into a *Klasse für sich* must be understood in the light of the strategy and tactics of the Dutch student union, the *Studenten Vakbeweging* or SVB. The SVB, established in 1963, offered a powerful alternative ideology for students not wishing to conform to the Regenten style of the *gezelligheidsverenigingen*. The very presentation of the SVB, as a "trade union" to promote the socioeconomic interests of the students, amounted to a rejection of the traditional image of student associations oriented toward the goals of "sociability" and the cultivation of a certain type of gentlemanly way of life. Democratic ideals of student life and society in general thus formulated were in sharp contrast to the aristocratic values of the *Corpora*. Instead of the traditional aloofness from political and social issues of the *jeunesse dorée*, the new SVB ideology stressed concern and activity with the university, with societal and international problems.

In short, the SVB provided  *nihilists* and those members of the *gezelligheidsverenigingen* who retained their membership for lack of an acceptable alternative with a new identity. In terms of this new ideology the old hierarchy was turned upside down, with the *Corps* student at the bottom, as a somewhat ridiculous anachronism, and the SVB *nihilist* on top, as the forerunner of the society to come.

The SVB has weakened the traditional stratification system of Dutch student society not only by ideological warfare and by arousing the "class consciousness" of students, but also by a direct attack on the power structure on which the prestige hierarchy was founded. The *Corpora*, and to a lesser extent also the other associations, lost their grip on the key positions of student society. After 1963, a democratization of student society started, which meant that election became, instead of coöptation, the primary means to allocate these key positions. Consequently, the chances of representatives of the student "Establishment" to gain office diminished. At the same time, thanks to hectic internal and external activities, the SVB acquired a sufficient number of *cadres* willing and able to replace the old ones. Furthermore, the SVB developed a new style of bargaining with the authorities: agitation,

demonstration, etc. Of course, in the hands of protesting students, negative sanctions or the threat of such sanctions are quite effective tools for stirring authorities to action. Since the old type of *Corps* student has a trained incapacity to use such unaristocratic means of reinforcing demands, the *Corpora* also lost their monopoly position as suppliers of capable representatives of students' interests and viewpoints *vis-à-vis* academic and public officials'.

The *Klasse für sich* as embodied by the SVB brought itself into being by successful ideological attacks on, and by wresting key positions from, the *Corps*-dominated student *élites*. Thanks to the partial breakdown of the traditional prestige and power hierarchy, they were able to gain a certain amount of mass support, and this support increased during the ensuing struggle with the authorities.

#### HYPOTHESES

The following hypotheses apply only to the active, sociopolitically involved student body. As will be pointed out presently, our data refer primarily to this type of student. Of course, from a theoretical point of view, this need not bother us. In all probability, any new protest movement recruits its adherents mainly from the more active, more involved elements of the social categories concerned.

As regards the *market* situation, not all students have become dependent to the same degree on the authorities for their means of subsistence and chances for upward occupational mobility. Obviously, students from upper-class *milieux* are usually still supported by their parents and not by the government, while thanks to their anticipatory socialization and to parental (or familial) influence and/or financial support they may be less dependent on their professors to obtain jobs than students of middle- or lower-class origin.<sup>7</sup>

<sup>7</sup> Some empirical evidence that in The Netherlands middle- and lower-class graduates are somewhat more dependent on their professors than their upper-class colleagues for their careers may be found in Lammers and Phillipsen (1966:200); sociology graduates of upper-class origin are less inclined to undertake an academic career than middle- or lower-class sociologists, and hence are in all

From this, two interrelated hypotheses follow: (1) The lower the general social status of a student's parents, the more likely he is to support the SVB, and (2) students receiving fellowships, grants, or loans will support the SVB more than those students not receiving such government assistance.

With respect to the *work* situation, it is a fact that neither all universities nor all departments have grown at the same rate. Therefore, in small universities the master-journeyman relationships may still prevail to some extent, and consequently under such circumstances students may be less ready to join the class struggle of the SVB than their fellow students working under "modern," "industrial" conditions of large-scale college teaching. This implies a third hypothesis: (3) The larger the size of the university, the more the student body will support the SVB.

As to the *status* situation, we concluded that members of *gezelligheidsverenigingen* are to some extent "immunized" against the "class ideology" of the SVB due to their values and norms, which represent the traditional Dutch student culture in its relation to the *Regenten* style. This culture is most widespread and most intensely professed in the *Corpora* and in the women's student leagues. Members of the denominational associations are less committed to this culture than *Corps* members, but more so than members of the nondenominational associations, while *nihilists* have the least faith of all in the traditional values and norms.

Since, in general, active participants in voluntary organizations are more likely than passive members to adhere closely to the culture prevailing in the organizations in question, it might also be supposed that nuclear members of *gezelligheidsverenigingen* would show more loyalty to the traditional student culture than peripheral members would. All of this provides a logical basis for two more hypotheses: (4) The higher a student's status in the traditional prestige (and power) hierarchy of student society, the less he will support the SVB. (5) The less actively a student participates in the social life of the *gezelligheidsverenigingen*, the more likely he is to support the SVB.

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probability less dependent on the sponsorship of their teachers.

## RESEARCH PROCEDURES

### *Election results*

To test these hypotheses, we gathered data concerning the voting behavior of Dutch students in elections for the local student councils. Since 1964, the SVB has participated in these elections as a student political party. Because SVB membership was rather limited and subject to considerable fluctuation, it was assumed that the actual vote for the SVB in such elections would serve to indicate where student support for the class movement of the SVB actually lies. At the investigator's request, shortly after the 1968 elections, all the executive committees of the student councils of the twelve Dutch universities and colleges supplied him with a detailed list of the election results, specified per ballot box. They also indicated the sites at which these ballot boxes had been placed locally and noted, on the basis of their local knowledge, what type of voters would be most likely to have voted at these places.

At seven of the twelve universities a number of ballot boxes had been placed at the clubhouses of the various *gezelligheidsverenigingen*, the "sociability" associations. This enables us to test Hypothesis 4 in a fairly direct way. Students other than members of the fraternity or sorority in question will very seldom have access to such a clubhouse, so that election results for a ballot box placed in a clubhouse will indeed reflect the degree of support the SVB enjoys among the active members of the association to which the clubhouse belongs.

The election results of ballot boxes situated at all other places (libraries, lecture halls, laboratories, administration buildings, etc.) probably give us a fair picture of the voting behavior of *nihilists* and of nonactive members of *gezelligheidsverenigingen*.

There is no reason to presume that the five universities or colleges where no ballot boxes were placed at clubhouses constitute a specific selection of the total population of Dutch universities and colleges. Moreover, it stands to reason that "ballot-box-placing policy" depends on all kinds of rather accidental considerations on the part of the committee in question.

Finally, the executive committee of the

(nation-wide) Netherlands Student Council kindly put at our disposal the results of the 1965, 1966, 1967, and 1968 elections, as well as the numbers of students entitled to vote as of 1968 per university or college. These data may serve to test Hypothesis 3.

### *Survey data*

Since the analysis of election results does not provide a direct means of verifying Hypotheses 1, 2, and 5, recourse had to be taken to survey data. A secondary analysis was made of three prior surveys carried out by other agencies and persons, and one survey was specially undertaken to test these (and some other) hypotheses. A short description of these four surveys is in order.

The first survey (see Muggen, 1967) consisted of a group-administered questionnaire for freshmen at the agricultural college in Wageningen in March 1967. More than 75% of all first-year students then present at this college participated.

The second survey—conducted by the Nijmegen Student Association for Political Science—was based on a questionnaire handed out to those Nijmegen students who had voted in the February 1968 elections of their student council, 63% of whom filled out the questionnaire (of the total of 7,000 Nijmegen students, only about 4,000 were entitled to vote by right of their membership in the general Nijmegen Student Union; of these 4,000 only 1,430 voted).

The third, a national survey, was based on a mail questionnaire sent to one out of every twenty Dutch students by the National Student Council in March 1968. The response amounted to 50%. Subsequent comparison of this sample with the population of all Dutch students (NSR 1968:24, n1) showed the sample to be generally adequate with respect to sex, year-group, university departments, student political orientation (as evidenced by the election results).

For secondary analysis, only the data pertaining to those who voted in the elections of the preceding month were used. Since for various reasons a subdivision of the sample according to year of arrival at the university or college had to be used in any case, it was decided to report the results of the correlation analysis separately for these seven sub-

groups; this method (replication) provides additional information with respect to our hypotheses and possibly for other purposes.

The fourth survey concerned a group-administered questionnaire among undergraduates in law and sociology at the University of Leiden. Part of this survey was designed to test the hypotheses developed above, and was carried out in March, 1968, during lecture hours attended mainly by second-year students.

Concerning the representativeness of these samples, the following can be said. Muggen compared his sample with known characteristics (sex, age, level of education of father, kind of high school, urban/rural background, membership in *gezelligheidsverenigingen*) of the total freshman population and found no statistically significant differences. No evidence on this point is available with respect to the Nijmegen survey. As regards the national survey and the Leiden survey, in both cases the sample yielded a much lower percentage of nonvoters for the last elections for student councils than the comparable figure for the student electorate at large. This bias presumably results from the fact that interested respondents are usually more inclined to return mail questionnaires than other persons (see e.g. Scott, 1961) and in all probability are also more inclined to participate in group-administered questionnaires. Therefore, it must be assumed that the sociopolitically interested, the active in student affairs, the sociable, and the younger students are rather overrepresented in these samples.

However, this particular bias in the direction of the sociopolitically involved students need not disturb us. In the first place, a separate analysis (not reported here) of the data of the national survey shows that, on the whole, voters do not differ very much from nonvoters with respect to the independent variables that come to the fore in our hypotheses. In the second place, it seems rather unlikely that the presence (not the magnitude) and direction of the predicted correlations would be appreciably altered had our sample included more of the sociopolitically apathetic students.

Nevertheless, the results of our analysis of these survey data may properly be gen-

eralized only to apply to the more active, more sociopolitically involved student body.

The dependent variable, i.e. degree of support for the SVB, could be indicated by rank-ordering the various student political parties according to their stance on the major issues first raised by the SVB, such as: university reform, proposed degree and form of student facilities, the amount of sociopolitical engagement required in society at large, negotiation *versus* confrontation. Therefore, it was possible also to rank-order the student respondents with respect to their degree of radicalism or degree of support for the SVB. For the national survey, a four-point scale was used:

- 0 = voted for the ANPS (the most rightist party in elections for student councils).
- 1 = voted for NSA (a moderately rightist party).
- 2 = voted for one of the smaller center parties (PSO, Stud. '75).
- 3 = voted for the SVB.

At Wageningen in 1967 and at Nijmegen in 1968, the ANPS did not participate, so that for these two samples a three-point scale was used to indicate our dependent variable.

In the Leiden survey an index was composed on the basis of reported voting behavior in 1966, 1967, and 1968 and the students' answers to the question: "What party would you vote for right now?" Presumably, this "averaging" of the student's voting behavior over a number of years increases the validity of the index by minimizing the influence on voting behavior of more or less accidental, situational factors varying from year to year. This composite index forms a five-point scale:

- 0 = voted (or would vote) exclusively for the NSA.
- 1 = voted (or would vote) for NSA or PSO.
- 2 = voted (or would vote) for NSA and SVB: voted (or would vote) exclusively for PSO; did not vote at all and would not vote now.
- 3 = voted (or would vote) for SVB and PSO.
- 4 = voted (or would vote) exclusively for SVB.

As to the independent variables, the indicators used are straightforward and need no more explanation than is provided in Table 1.

### Results

*Hypotheses 1 and 2* are on the whole clearly corroborated by the secondary analysis of survey data (see Table 1). Students from upper-class families (one may infer from nine out of ten samples) are less inclined to favor the "class struggle" of the SVB than students of middle- or lower-class origin. In nine out of eleven cases there is evidence that students receiving government support are more in favor of the SVB than their colleagues who receive no grants, fellowships, etc. Of course, these two factors (social origin and government support) hang together, since the father's income is one of the criteria for granting a student government subsidy.

The fact that neither of these hypotheses is upheld in the case of the freshmen at the Wageningen agricultural college is somewhat puzzling. Perhaps freshmen in Wageningen, a small college with a lower percentage of SVB votes than any other university or college in The Netherlands, are exposed to such strong pressures to conform to the (anti-SVB) norms prevailing there that only scattered individuals characterized by specific personal (and not sociocultural) earmarks resist this powerful tendency to conform.

To test *Hypothesis 3*, data for 1967 and 1968 were available for eleven universities and colleges. In 1966 the SVB took part in the elections in only nine, and in 1965 in only seven places. To make comparison possible, we computed additional correlations for 1967 and 1968 for the seven and nine universities or colleges forming the basis of the analysis for 1965 and 1966, respectively. Although for two (1965 and 1968) out of four years the coefficients do not reach the customary level of statistical reliability, jointly the results of the analysis for all four years definitely point to the presence of an association between size of the electorate and support for the SVB (see Table 2). The four "samples" may be taken together, since the election results for various years at one college can be taken as independent units of observation. This gives a Pearson  $r$  of .55, which is sufficiently significant in a statistical sense, given a number of 38 units.

Table 1. Correlations<sup>a</sup> between Degree of Support for the SVB and Various Characteristics of Students' Market Situation and Status Situation.

Characteristics	Cross-section of All Dutch Students						Leiden		Wageningen	Nijmegen
	1st (167)	2nd (172)	3rd (166)	4th (128)	5th (94)	6th (54)	7th (65)	Law (106)	soc. (104)	(906)
Father College Ed. <sup>b</sup> (0=has, 1=has not)	.24	.24	.10	.22	.15	.29	.18	.24	.18	(.04)
Mother College Ed. <sup>b</sup> (0=has, 1=has not)								.19	(.05)	
Government Support (0=no, 1=yes) <sup>c</sup>	.20	.28	.20	.14	.35	.31	(.06)	.22	.11	(-.02)
Status in Traditional Student Society <sup>d</sup>	.42	.24	.34	.36	.38	.49	.47	.31	.34	.19
Particip. in Tradit. Student Society <sup>e</sup>								.26	.34	.14

<sup>a</sup>Correlation coefficients (Kendall's tau, corrected for ties) not differing significantly (at the 5% level) from zero in the predicted direction are shown in parentheses.

<sup>b</sup>In the Leiden survey, questions were asked not only about the student's father's college background but also about his mother's. A four-point scale was constructed: 0=parent went to college and was active member of a gezelligheidsvereniging; 1=parent went to college and was just a passive member of a gezelligheidsvereniging; 2=parent went to college and was not a member of a gezelligheidsvereniging; 3=parent did not go to college.

<sup>c</sup>For the Nijmegen sample, a three-point scale was used, the midcategory being "wholly or partially earns his own living."

<sup>d</sup>A rank-order of high to low was composed, assigning the following scores: 0=belongs to the Corps or women's student league; 1=belongs to a denominational association; 2=belongs to some other nondenominational associations; 3=does not belong to any gezelligheidsvereniging. A somewhat different index was used in the Nijmegen survey. The Roman Catholic University of Nijmegen has no specific denominational associations. However, the local gezelligheidsvereniging could easily be ranked according to their degree of "traditionalism."

<sup>e</sup>For the Wageningen survey, the number of affiliations with more or less formally recognized subgroups of the gezelligheidsvereningen was counted. For Leiden, the following four-point scale was used: 0=holds or has held a function in one of the gezelligheidsvereningen; 1=an active member of one of gezelligheidsvereningen; 2=member, but not an active one, of a gezelligheidsvereniging; 3=not member of any gezelligheidsvereniging.

Table 2. Correlations<sup>a</sup> of Electorate Size and % SVB Votes per University or College.

	1968	1967	1966	1965
N=7 <sup>b</sup>	(.10)	(.54)	(.55)	(.51)
N=9 <sup>c</sup>	(.50)	.61	.63	
N=11 <sup>d</sup>	(.40)	.65		

<sup>a</sup>Correlation coefficients (Pearson's  $r$ ) not different from zero at the 5% level in the predicted direction are in parentheses. However, the average coefficient over the four years,  $(.40 + .65 + .63 + .51) / 4 = .55$ , is significantly different from zero (taken as  $N=38$ , being the total number of independent units of observation in those four years).

<sup>b</sup>Amsterdam (Municipal University); Amsterdam (Free University); Delft; Groningen; Leiden; Nijmegen; Utrecht.

<sup>c</sup>In addition to the seven mentioned above: Tilburg and Wageningen.

<sup>d</sup>In addition to the nine mentioned above: Eindhoven and Rotterdam.

We may conclude that larger universities, compared to smaller ones, imply more emphasis on horizontal, "class"-contacts for the student and therefore form a better breeding ground and shelter for radical groups. Moreover, the larger the student body, the greater the chances that a sufficient number of individual students with "deviant" views and interests will be present to form one or more groups of activists (see on this point the remarks of Lipset, *et al.*, 1956: 157-163, 170-171; on the correlation between university size and incidence of student protest in the U.S., see Scott and El-Assal, 1969). A word of caution is, however, in order. The raw data, as well as the investigator's familiarity with local conditions in various Dutch universities, suggest that certain other variables—hard to isolate by statistical manipulations—obscure the pure relationship between size and SVB support. For one thing, the smaller units consist preponderantly of professional schools (engineering, economics, agriculture), which all over the world select and/or cultivate a more conservative type of student.

*Hypothesis 4* is borne out quite well by the analysis of the survey data and of the election results. In all eleven samples we

find a statistically significant correlation between a student's status in the traditional student society and his student-political orientation (Table 1). When we look at the even more pertinent evidence of the voting returns per ballot box category (Table 3), we find that in all seven cases the SVB drew less support at the *Corps* and women's student league clubhouses than at those of the denominational associations, while the ballot boxes at the clubhouses of the denominational associations consistently yielded a lower percentage of SVB votes than ballot boxes placed at clubhouses of the other non-denominational associations. In two cases (Utrecht and Amsterdam) the remaining ballot boxes, i.e. those not placed in clubhouses, show the highest percentage of SVB votes. In the other two cases (Leiden and Delft) this is not so. However, in all probability this is due to the circumstance that—depending on local conditions—quite a lot of members of *gezelligheidsverenigingen* sometimes cast their votes outside their clubs. For Leiden this could be checked, because at that university some characteristics of each voter were registered at each ballot box. On the basis of these data, the correlation between the percentage of "nihilists" and the percentage of SVB votes could be computed for all 12 ballot boxes placed outside clubhouses, and proved to be positive and significant (Spearman's  $\rho = 0.58$ ).

The negative relationship, predicted in *Hypothesis 5*, between degree of participation in the world of the *gezelligheidsverenigingen* on the one hand, and, on the other, endorsement of the SVB movement by voting, could be ascertained only for Leiden and Wageningen (Table 1). All three correlations are positive and statistically significant. Since in these cases there are no systematic differences between the associations with respect to percentage of inactive members, we can be confident that this outcome is not due to the correlation between membership in *gezelligheidsverenigingen* and voting behavior.

### Discussion

Findings concerning the socioeconomic backgrounds of student activists and their

Table 3. Results of the 1968 Elections for Student Councils at Seven Dutch Universities or Colleges (% of Votes for the SVB).

Ballot Boxes Placed (at Clubhouses of):	Leiden	Utrecht	Groningen	Amsterdam <sup>a</sup>	Delft <sup>b</sup>	Rotterdam	Wageningen <sup>c</sup>
<u>Corpora</u> or Women Student Leagues	8	12	23	27	3	4	21
Roman Catholic or Protestant <u>gezelligheidsverenigingen</u>	46	39	46	42	34	29	25
Other Nondenominational <u>gezelligheidsverenigingen</u>	52	48	--	53	46	--	40
Elsewhere	44	52	47	63	39	39	--

<sup>a</sup>Concerns Municipal, not Free University (Calvinist).

<sup>b</sup>At Delft, the SVB did not participate. However, the CCL--a party akin to the SVB (but somewhat more to the center)--presumably drew votes that would otherwise have gone to the SVB and is for the purpose of this analysis put on a par with the SVB.

<sup>c</sup>Ballot boxes were placed in lecture halls during the day and clubhouses at night.

sympathizing fellow students yield a somewhat puzzling picture. The correlation between the general social status of the student's parental home and his readiness to protest is sometimes definitely positive (see e.g. Flacks, 1967; Lyonns, 1965; Watts and Whittaker, 1966; Westby and Braungart, 1966), at other times only weakly positive or about zero (e.g. Fortune, 1969; Kaase, 1970; Institut für Demoskopie, 1967; Somers, 1965), and finally, in the present instance of the Dutch situation in the late sixties, even negative.

Perhaps the seeming contradiction of the research evidence with respect to this relationship can be explained by differentiating between the protest-issues involved and the differential *appeal of various issues for students whose backgrounds differ according to socioeconomic position and according to cultural values.*

People join or endorse protest movements to realize ideals, to defend their interests, or for both reasons. As pointed out by Lipset (1963:318), the first type of "idealistic" issue usually appeals more to people from an upper-class background, because they are more educated, sophisticated, and probably enjoy more psychic security than their fellow

citizens from middle- or lower-class *milieux*. Perhaps it should be added that another reason why educated upper-class people are more prone to support idealistic causes is their socioeconomic security. They have less to worry about with respect to their daily bread and similar problems and can afford to spend energy, time, and money on lofty pursuits. However, the second type of issue—promotion of interests: economic, power, or other kinds of interest—for obvious reasons generally draws less support from people of upper-class backgrounds than of their underprivileged compatriots.

Following this line of reasoning, one would expect upper-class students to favor more or less idealistic movements but to shun promotion-of-interest movements, while the reverse would be true for students from middle- or lower-class origin. This indeed appears to be the case. The first set of studies mentioned above (positive relationship between parental status and protesting) has to do with student protest primarily aimed at general societal issues, while the last study mentioned (negative relationship between parental status and protesting; the present instance of support for the Dutch SVB) clearly represents a form of student syndicalism. More-

over, in the former studies, varieties of student *activism* (membership in radical groups; participation in sit-ins, demonstrations, etc.) are at stake. This means that in all likelihood for this type of protesters the protest actions in question signify a fight for idealistic causes and not a defense of student interests. On the other hand, for the rank and file of the Dutch SVB supporters, simple bread and butter issues were at stake.

Of course, quite often student protest movements do not represent for their adherents *either* an idealistic effort *or* a defense of interests. Sometimes, from the perspective of the *same* participants or sympathizers, *both* types of issues are involved; also, the same protest movement may mean one type of issue for this category of adherents, and another type of issue for a different category. In this vein one could surmise, for instance, that the activist nucleus of many specific protest actions will often be motivated primarily by idealistic concerns, while those who follow the music, or just sympathize, are more likely to be inspired by rather down to earth student interests as well.

Consequently, when one studies not *student activism* but rather *protest proneness* (or for that matter low-risk protest behavior such as joining a large-scale sit-in when it is already well under way), chances are high that one is actually studying a "mixed" type of protest movement in which for the protesters, in word or deed, both idealistic and interest issues are at stake. In such cases the direction of the relationship between the social status of his parents' home and the student's protesting becomes uncertain, *because the level of education of the parents and the level of prosperity of parental home* (both positively related to social status) *affect the dependent variable* (protest proneness or low-risk protest behavior) *in opposite directions*. This state of affairs could very well be the reason why, in the second series of studies mentioned (Fortune, 1969, etc.), no correlation, or only a weak correlation, was found between social status indicators on the one hand and protest indicators on the other. In every one of these studies, not student activism but general protest proneness was measured. Moreover, in the only study of this tenor in which level of education and level of income of

parental home were analyzed, *keeping the other variable constant*, educational level indeed showed a positive relation and level of income a negative relation with protest proneness (Institut für Demoskopie, 1967: 27-28).

Finally, what about the applicability of the social class model to the study of student protest? As Feuer (1969, especially Chapter 1) has convincingly argued, student movements are usually not of the syndicalist, promotion-of-interest variety, and therefore cannot be satisfactorily analyzed in terms of Marxian class theory. It is only in rather exceptional cases—like the one studied in this article—that an effort to apply the class model integrally is productive. Moreover, how "deviant" this case is at the moment can be inferred from the fact that since 1969 the Dutch student union, the SVB, has been defunct and has been replaced by a multitude of loosely organized action groups such as have appeared in other western countries.

Nevertheless, in other student movements too the promotion-of-interest element is not altogether absent, so that the class model may provide at least some partial or supplementary insight into what goes on. Even in the U.S., student protest in the Sixties at one point showed a trend toward student syndicalism (see e.g. Skolnick, 1969:96-98) and may take that direction again in the Seventies (Horowitz, 1970). Furthermore, in continental Europe and in many Third-World countries, syndicalist goals have always loomed larger in student movements than in the Anglo-Saxon countries. Finally, and most important—as implied in the arguments of this last section of our discussion—in many "idealistic" student protest movements, rather realistic student interests may be at stake under the surface. It could very well prove worthwhile as a hypothesis for further research that many students condone or join—when not too much risk is involved—protest actions regardless of the overt ideology of the movement, because they feel, rightly or wrongly, that such action is in their interest. In such instances, perhaps much more numerous than many scholars in this field suppose, the conflict of interests is in all likelihood not primarily centered around economic interests but rather around power and privilege interests,



interests such as the loosening of tight bureaucratic rules, getting better educational service from a mainly research-oriented faculty, or generally showing University, Administration, and Faculty that students do not like to be shoved around or dealt with as a closing entry in the time budgets of these authorities. In such instances a partial approach to the problem under scrutiny, with the aid of a class model, could prove fruitful.

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# CONJUGAL POWER STRUCTURE: A RE-EXAMINATION<sup>1</sup>

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*American Sociological Review* 1971, Vol. 36 (April):264-276

*A representative sample of 776 husbands and wives in the Los Angeles area were interviewed regarding the relative power of husbands and wives in various decision areas, following the basic procedures utilized by Blood and Wolfe in their 1959 study of wives only, in Detroit. Essentially, these results paralleled those obtained by Blood and Wolfe, extending their findings to responses from husbands and in a different area. Husband power was greatest among Oriental couples and least among Negro couples; it decreases with age, with length of marriage, and is less where a second marriage is involved; husband power increases with occupational status and educational level. The current study questions the effects of sampling of conjugal decision areas. With a somewhat more representative sampling of decisions, the distribution of power changes dramatically. Husband dominant families tend to show high authoritarianism scores for both husbands and wives. Least marital satisfaction is associated with wife dominance. While the current investigation centers on power relationships between husbands and wives, the basic approach can be extended to analysis of power in other types of groups.*

ACCORDING to students of the family today, a most important aspect of family structure is the power positions of husband and wife. The balance of power between them is a sensitive reflection of the roles they play in marriage, as well as a factor affecting many other aspects of their relationships. A major exploration into conjugal power relationships was conducted by Blood and Wolfe in a study carried out in 1954 and involving 731 urban and 178 farm wives in the greater Detroit area (Blood and Wolfe, 1960; Wolfe, 1959). Their study is of particular interest in their further application of theoretical developments by Herbst (1952) to the exploration of conjugal power, as well as through methodological developments. The basic approach which they utilize, both theoretically and methodologically, has important implica-

tions not only for the understanding of the family but to the analysis of power and influence structure in face-to-face groups in general. Of particular importance is their conclusion that power is not unidimensional—the simplified approach to power in which one merely determines the degree of power on a single scale does not apply.

Blood and Wolfe consider a number of important decision areas involving the family and determine the ways in which such decision problems are resolved when disagreements occur. As has been observed elsewhere, power in one area of decision does not necessarily imply power in some other area (Heer, 1963; Levinger, 1964; Sharp and Mott, 1956). The method which Blood and Wolfe utilized involved asking a representative sample of wives how final decisions are made with respect to eight areas: (1) husband's choice of job, (2) choice of car, (3) whether to buy life insurance, (4) where to go on a vacation, (5) choice of house or apartment, (6) whether or not the wife should work, (7) choice of doctor, (8) expenditure of funds for food. The degree of relative power of husband and wife, and the extent to which decisions were made individually or jointly were then measured and

<sup>1</sup> This study was supported, in part, by the Group Psychology Branch of the Office of Naval Research, Contract Nonr 233(54), and by the U.C.L.A. Latin American Studies Center. We are indebted to Dr. Marian Olson for her assistance and suggestions in pretesting the interview schedule and to Dr. Erhan Yasar and Mr. Alvin Rosenthal for their assistance in computer analyses.

<sup>2</sup> The study was conducted while Dr. Rodrigues was at the University of California, Los Angeles.

related to various demographic and social psychological variables such as age, socioeconomic status, religion, education, employment, income, race, marital satisfaction, goals in marriage, etc. Blood and Wolfe thus infer relative power according to the degree to which the husband or the wife are reported to make unilateral decisions in the various decision areas. While we recognize some problems with this operational definition, as discussed by Heer (1963), we have accepted it as the starting point for this investigation.

Although the results presented by Blood and Wolfe are of significance, several research questions are posed by their study. One of these relates to the representativeness of the sampling of decision areas. The number of decision areas which might be considered is undoubtedly extremely large. Blood and Wolfe stated several criteria which they used in their selection: (1) importance to the functioning of the family; (2) inclusion of a representative sample of both masculine and feminine decision areas; (3) decisions should affect the family as a whole; and (4) universality of decision area to all families. While we would agree that the decisions which they sampled were important, it did not seem to us that the sample satisfied the second criterion adequately. The eight areas sampled include an overrepresentation of areas traditionally or normatively within the male domain of competence, with less emphasis on decisions normally within the female's domain of power. Would their conclusions hold with a different sampling of decisions?

A second question concerned the possibly distorted picture of conjugal power balance which might be created by eliciting responses of wives only. Power in the family is a sensitive issue where there would be both conscious and unconscious instigations to distort reality. The manner in which such distortions might operate is not obvious. Heer, in his study of conjugal power, found that husbands reported less power for themselves than wives claimed for them, and vice versa (Heer, 1962). Kenkel (1957) reports that wives tend to overestimate the power of their husbands. Kenkel and Hoffman (1956) report considerable discrepancy in husbands' and wives' reports about their respective roles in decision making. Levinger (1966) found

husband and wife differences in reports regarding sexual behavior.

A third question concerned the applicability of Blood and Wolfe's results to other geographic areas. There might be reason to believe that the norms in Los Angeles, with regard to conjugal power as well as in other matters, would be quite different from those in Detroit or other parts of the United States.

A final major question has to do with the theory of interpersonal power. Although a substantial amount of empirical evidence shows such power to be a function of the resources commanded by the respective interacting parties (Heer, 1963), other factors must also be examined: Rodman (1967) examines conflicting evidence on family power in various countries and concludes that cultural norms regarding the appropriate distribution of power must be considered.

Needs and traits of the individual personalities may also be involved, although evidence to that effect is quite meager. Heer (1958), for instance, found no significant effect of dominance as a personality trait. The aspect of personality specifically dealt with by Blood and Wolfe was merely what satisfactions the wives desired in their marriage, and there are, of course, numerous others which might also be considered. In the current study we sought responses to scales measuring authoritarianism and feelings of self-competence, both of which we felt would be related to conjugal power.

#### PROCEDURE

*Sample:* To pursue the implications of the aforementioned questions, we conducted, in 1964, a person-to-person interview survey involving a cross section of the married population of metropolitan Los Angeles. Pretesting and interviewing were carried out by senior majors and graduate students in psychology at UCLA, trained in interviewing procedure as part of a course in survey research methodology conducted by the senior author. Sampling was accomplished by assigning quotas to interviewers on the basis of sex, age, and socioeconomic status, with the intention of proportionately representing in cross section, within the limitations imposed by factors

identified with local demographic specialization, the urban married population of the United States as it was estimated to be constituted in terms of information available from 1960 census and other data. The resulting sample from which complete data were obtained consisted of 747 respondents, 410 of whom were wives and 337, husbands. Of the total, 62% of family heads (males) were manual workers (the same as the percentage of manual workers in the Detroit sample). Approximately 47% of each sex were under 40 years of age. Non-whites were somewhat overrepresented, blacks constituting approximately 23% of the total sample. The sample of husbands, it should be noted, was independent of the sample of wives except for 86 couples, both members of whom were interviewed. In the latter case, of course, care was taken that each member of the pair was interviewed separately. Since sampling was based on 1960 census information, it cannot be claimed that it precisely reflected 1964 conditions.

**Decision Areas.** Our interviewers, after a brief introduction to indicate the general nature of the survey, presented an augmented sample of decisions to the respondent prefaced as follows: "In every family somebody has to decide such things as where the family will live and so on. Many couples talk things over first, but the *final* decision has to be made by the husband or the wife. For instance who makes the final decision about . . . ?" A list of fourteen decisions was then read to the respondent who was at this point handed a card on which was printed the following list of alternatives for his response to each decision question: husband more than wife, husband and wife exactly the same, wife more than husband, and wife always.

The decisions in their order of presentation were as follows: (1) what people you will invite to the house or go out with; (2) how to decorate or furnish the house; (3) which TV or radio program to tune in; (4) what the family will have for dinner; (5) what clothes you will buy; (6) what type of clothes your husband (or wife) will buy; (7) what car to get; (8) whether or not to buy life insurance; (9) what house or apartment to take; (10) what job you (or your husband) should take; (11) whether or not (you, if interviewing a wife) your wife

should go to work or quit work; (12) how much money the family can afford to spend per week on food; (13) what doctor to have when someone is sick; and (14) where to go on vacation.

This procedure duplicated essentially that used in the Detroit study (see above), except that our first six items were added to the list. These additional six items were selected from a larger pool submitted to a score of upper division and graduate students in psychology (as well as to the authors) for their judgment as to appropriateness in terms of the criteria of "universality," importance, effect on the family as a whole, and their likelihood of being matters in which the wife was likely to have maximum or substantial influence. Although we cannot claim that our list fairly represents the universe of all possible conjugal decisions, we believe that it provides a better opportunity for the wife's power to manifest itself than did Blood and Wolfe's sample of decisions.<sup>3</sup>

**Personality Items.** As mentioned previously, we included two personality trait measures in the interview: a seven-item scale<sup>4</sup> to measure authoritarianism and a three-item scale to measure feelings of personal competence. The brief scale measuring authoritarianism, derived from the F scale (Adorno *et al.*, 1950) and originally developed and used in a Philadelphia survey by Sanford and Older (1950), consisted of the items below, introduced with the following words: "Here are a few statements which are often heard. Will you please tell me whether you agree or disagree with them, and whether you agree or disagree a little, pretty much, or very much?" (1) Human nature being what it is, there must always be war and conflict. (2) The most important thing a child should learn is obedience to his parents. (3) A few

<sup>3</sup> There are undoubtedly many other decision areas which could have been included, or substituted. For instance, all decision areas involving parent-child relationships were excluded. This decision area was included in the original study by Herbst (1952). However, like Blood and Wolfe, we decided to limit our attention to decision areas which practically all families would have to face.

<sup>4</sup> Following an appraisal by the scale's authors, we omitted one item of the original battery of eight questions, as had been done in a further validation study of the scale by Eager and Smith (1952).

strong leaders could make this country better than all the laws and talk. (4) Most people who don't get ahead just don't have enough willpower. (5) Women should stay out of politics. (6) People sometimes say that an insult to your honor should not be forgotten. (7) People can be trusted. The score assigned to responses on each item varies from 1 to 7, 1 being assigned for extreme disagreement, and 7 for extreme agreement (except for the final item, which is reverse-scored). Because some respondents did not answer every question, the "don't know" was treated simply as a neutral point and assigned a value of 4, placing it between "disagree a little," having the value of 3, and "agree a little," having the value of 5.

The items making up the three-item Guttman type scale for measuring feelings of self-competence, as presented by Douvan and Walker (1956), consisted of the following questions: (1) Some people feel that their lives have worked out just the way they wanted. Others feel they've really had bad breaks. How do *you* feel about the way your life is turning out: satisfied or dissatisfied? (2) What do you think your chances are of living the kind of life you'd like to have. Do you think they are pretty good, or not so good? (3) Some people feel that they can make pretty definite plans for their lives for the next few years. Others feel they aren't in a position to plan ahead. How about you—do you feel able to plan ahead or not?

This scale, developed at the University of Michigan Survey Research Center, has yielded an index of reproducibility of .94. Scoring is in accordance with the following scale patterns: (a) *High*. People who give affirmative answers to all three questions. (b) *Moderately High*. Individuals who are satisfied, feel optimistic about the future, but do not feel they are in a position to plan their own futures. (c) *Moderately Low*. Individuals who are satisfied with their lives, but feel the chances for attaining their aspirations are poor and that they cannot be optimistic about their futures. (d) *Low*. Those who are dissatisfied, feel their chances are poor, and that they are in no position to plan ahead. Nonscale groups were collapsed into the scale pattern having the same number of affirmative responses.

Also included were the Blood and Wolfe

questions as to the part of marriage most highly valued by the respondent. After being handed a card on which were printed the alternatives, he was asked: "Thinking of marriage in general, which one of the five things on this next card would you say is the most valuable part of marriage?" (1) The chance to have children. (2) The standard of living—the kind of house, clothes, car and so forth. (3) The husband's (or wife's) understanding of the wife's (or husband's) problems and feelings. (4) The husband's (or wife's) expression of love and affection for the wife (or husband). (5) Companionship in doing things together with the husband (or wife). A second choice was also requested, and following that, respondents were queried as to their degree of satisfaction with the marriage.

*Background.* Finally, questions of a background nature were asked. These included ones on length of marriage, whether a first or later marriage of the respective spouses, occupation of each, educational level attained by each spouse, their religious affiliations, ages, country of birth, and ages at immigration if foreign born. Respondents were classified by the interviewers as to race and socioeconomic status.

## RESULTS

*Comparison of decision structure in Detroit and Los Angeles.* For specific comparisons with the results of the Detroit area study, Table 1 was prepared from our data, and Table 2, excerpted from Blood and Wolfe (1960:21) is also presented. To facilitate comparisons, we have first listed in Table 1 the items added in our sample of decisions and then in Table 2 the eight items of the Detroit study.

Attending first to the latter aspect of Table 1(a) and the whole of Table 4, but more specifically, to the figure for the husband's mean power in each, one is struck by the remarkably close agreement; the husband's mean power being 3.34 for the Los Angeles sample and 3.26 for the Detroit one. The concordance is even closer when responses of our "wives only," Table 1(b), are compared with the Detroit data (which was for wives only); for there the husband's mean power drops .02 of a point, being now

Table 1. Allocation of Power in Decision-making Areas: (a) Total Sample, N=747; (b) As Reported by Wives of the Sample, N=410; (c) As Reported by Husbands of the Sample, N=337.

Who Decides?	Guests	Dec- or- ate	TV or Radio Prog.	Din- ner Menu	Wife's Clothes	Hus- band's Clothes	Hus- band's Job	Car	In- sur- ance	Va- ca- tion	House	Wife's Work	Doc- tor	Food
5) Husband Always	(a) 7 (b) 7 (c) 9	3 3 3	13 13 13	2 2 3	5 6 3	52 49 55	79 80 82	50 50 54	44 44 44	9 10 8	11 10 13	19 19 20	4 4 4	8 9 6
4) Husband More Than Wife	(a) 11 (b) 12 (c) 10	5 5 5	18 20 17	3 1 4	3 1 6	18 20 16	11 10 12	19 19 18	16 14 17	10 11 9	10 8 12	9 10 9	5 4 6	5 3 7
3) Husband & Wife Ex-actively the Same	(a) 54 (b) 55 (c) 55	22 24 19	50 49 51	13 13 10	8 9 8	12 15 8	5 5 4	24 27 21	31 33 28	69 69 71	59 67 54	23 22 24	56 56 56	33 32 33
2) Wife More Than Husband	(a) 19 (b) 18 (c) 19	25 24 27	11 12 11	23 20 27	20 18 23	8 8 9	0 1 1	1 1 1	4 4 4	4 4 5	10 9 9	18 18 20	14 15 15	21 18 26
1) Wife Always	(a) 7 (b) 7 (c) 7	44 44 46	5 4 6	58 63 55	62 65 59	9 7 11	3 4 1	3 2 4	3 3 4	4 3 5	8 6 11	27 29 25	17 18 16	32 38 28
N.A.	(a) 2 (b) 1 (c) 0	1 0 0	3 2 2	1 1 1	1 1 1	1 1 1	2 0 0	3 1 2	2 2 3	4 3 2	2 0 1	4 2 2	4 3 3	1 0 0
Husband's Mean	(a) 2.92 (b) 2.94 (c) 2.93	1.97 1.99 1.93	3.23 3.26 3.19	1.66 1.59 1.70	1.67 1.64 1.70	3.96 3.95 3.97	4.67 4.61 4.74	4.16 4.15 4.20	3.96 3.94 3.97	3.17 3.23 3.12	3.06 3.06 3.06	2.74 2.71 2.79	2.65 2.61 2.66	2.33 2.27 2.38

Husband's Mean Power (Power Balance Index) for six new items: (a) 2.57, (b) 2.56, (c) 2.57.  
 Husband's Mean Power (Power Balance Index) for all 14 items combined: (a) 3.01, (b) 3.01, (c) 3.02.  
 Husband's Mean Power (Power Balance Index) for original eight B & W items: (a) 3.34, (b) 3.32, (c) 3.36.

\*The mean for each column is computed by assigning the weights shown at the extreme left..e.g., "Husband Always"=5; "Husband More Than Wife"=4; "Husband and Wife Exactly the Same"=3; etc.

Table 2. Allocation of Power in Decision-making Areas (731 Detroit Families).†

Who Decides?	Hus- band's Job	Decision						
		Car	Insur- ance	Vaca- tion	House	Wife's Work	Doc- tor	Food
5) Husband Always	90%	56%	31%	12%	12%	26%	7%	10%
4) Husband More Than Wife	4	12	11	6	6	5	3	2
3) Husband & Wife Exactly the Same	3	25	41	68	58	18	45	32
2) Wife More Than Husband	0	2	4	4	10	9	11	11
1) Wife Always	1	3	10	7	13	39	31	41
N.A.	2	1	2	3	1	3	3	3
Total	100	99	99	100	100	100	100	99
Husband's Mean Power*	4.86	4.18	3.50	3.12	2.94	2.69	2.53	2.26
Husband's Overall Mean Power=3.26								

\*The mean for each column is computed on the basis of the given weights shown, e.g., "Husband Always"=5).

†Adapted from Blood and Wolfe, 1960, p. 21.

3.32 as compared with 3.26 for the Detroit figure. The result for the husband portion of our sample differs only slightly, but raises the mean power figure to 3.36. The husband's mean power for each specific decision (e.g. what car to get) shows more variation from sample to sample amongst the several tables, but, even so, scarcely diminishes the over-all testimony of congruency. Los Angeles husbands and wives are clearly very much the same as Detroit ones in their allocations of decision making, at least as far as the eight original items reflect the power balance.

*Effects of Broader Sampling of Decision Items.* A somewhat different picture emerges when one examines the data resulting from the six new items. The husband's mean power on these drops to 2.57—Table 1(a)—with there being a high consistency in the reports as given by each sex separately; see Tables 1(b) and (c). Husbands' reports are again slightly augmentative of their mean power *vis-à-vis* the reports of wives; 2.57 and 2.56

respectively. But the difference is quite a trivial one. The stability of our results here is what is really impressive. As we anticipated, the result of including the six new decision items, intuitively arrived at or at best crudely empirical in their derivation, was to shift the power distribution of husbands and wives in a direction favoring the wives. Roughly, about a 10% loss in husbands' mean power is found, and the balance is now almost at 3.00, which represents a precisely equal power position for husbands and wives. The consistency between the report of the males and that of the females is again obvious; 3.02 for the former, 3.01 for the latter.

Wolfe, in a separate and earlier report (1959) of the Detroit area study presented an interesting conceptualization of conjugal power, using a four-fold scheme of categorization of power distribution. He points out that in the family behavioral field, the husband has a range of authority, the wife has also a range of authority, and a residual



range is shared between them. Further, there may also be regions in which neither husband nor wife has authority over the other. Authority relationships may differ from couple to couple in at least two respects: the extent of the ranges of authority of husband and wife and the extent of the shared range of authority. Wolfe defines his four-fold classification of power distribution as follows: (1) *Wife Dominant Type*, wherein the wife's range of authority is considerably larger than her husband's. (2) *The Syncratic Type*, consisting of couples between which there is nearly a balance of relative authority and the shared range is equal to or greater than the combined ranges of husband and wife. (3) *The Autonomic Type*, wherein there is also an approximate balance of *relative* authority, but the husband's and wife's ranges together are greater than the shared range. (4) *The Husband Dominant Type* is an authority relationship in which the husband's range is considerably greater than that of his wife.

In the comparisons effected by Tables 1-2, we have dealt with the relative authority (or power) of husbands and wives. This was arrived at by summing the weights assigned to the five different responses for all decision questions and computing an average, which has been referred to as husband's mean power for a given decision question. The mean of these means was called the Power Balance Index.

In Wolfe's scheme he uses the sum of the weights across the list of decision questions as an index of relative authority (RA), and an additional dimension referred to as the Degree of Shared Authority index (DS). The latter is based on the number of decisions of the total set with answers "husband and wife exactly the same." For the eight questions used in the Detroit study, a DS score of 8 would indicate complete sharing of authority in all matters; hence, a completely syncratic power relationship. The lower the DS score, the more autocratic or autonomic the relationship.

The RA and DS indices together were used by Wolfe to arrive at the couple's "Authority Type." The Husband Dominant Type included all couples with an RA score of 29 or more (where the range was from 8 to 40); placed in the Wife Dominant

group were all those having an RA score of 19 or less.<sup>5</sup> The Syncratic group was made up of all couples having an RA score from 20 to 28 inclusive, and a DS score of 4 to 8. The Autonomic group consisted of those with RA scores from 20 to 28 and DS scores of 3 or less.

In order to compare our Los Angeles sample with that from Detroit, scores for the several conjugal authority types were computed in accordance with the above scheme for the eight items of our sample of decisions which duplicated those of the Detroit study.<sup>6</sup> The result appears in Table 3.<sup>7</sup> As can be seen, while the percentages of syncratic and wife dominant families are almost the same in both samples, there are sizable differences in the percentages for those in the husband dominant and autonomic categories, with the augmentation in the husband dominant group for the Los Angeles sample being accomplished at the expense of a reduction in the autonomic category. ( $\chi^2$  computed for these data is significant at the .001 level.) Why there should be a greater percentage of husband dominant couples in Los Angeles than in Detroit is a question to which we have no answer as yet. That the difference is one linked to the difference in locale, or in time—the surveys were separated by a decade—rather than to sex of the respondent reporting seems indicated by the data presented in Table 4. The differences previously noted between the Detroit and Los Angeles

<sup>5</sup> Scoring follows that presented by Blood and Wolfe (1960). In Wolfe's article (1959), the 1-5 scoring system for husband's power is precisely reversed.

<sup>6</sup> The scoring scheme employed follows that utilized by Wolfe (1959) in assigning the categories, with an extrapolation to apply to the larger 14-item array. Given the enlarged range of relative authority (RA) scores from 14 to 70, Wife Dominance was defined in terms of RA scores from 14 to 34; Husband Dominance—51-70. Degree of shared authority (DS) scores was again defined in terms of number of "Husband-Wife" responses, and could range from 1 to 14. Thus those families ranging between 35 and 50 on RA scores were categorized as Autonomic if their DS scores were between 0 and 8; they were considered Syncratic if the DS scores were between 8 and 14.

<sup>7</sup> Although in this and several following tables  $\chi^2$ s are employed as the criterion of over-all significance, the data appear not as incidences but in percentage terms, thus facilitating description at a somewhat more detailed level.

Table 3. Percentages in Each Category as Determined by Scores for Wives on Original Eight Decision Questions.

Authority Type	Detroit	Los Angeles	Dif.*
Husb. Dom.	25%	32%	+7
Syncratic	31	33	+2
Autonomic	41	33	-8
Wife Dom.	3	2	-1
Total	100 (656)	100 (410)	

\* $\chi^2=16.34$ ,  $df=3$ ;  $p<.001$ .

samples of wives are similarly manifest between males and females of the Los Angeles groups ( $\chi^2=16.89$ ;  $df=3$ ;  $p<.001$ ). The scores derived from the husband's responses reveal an even greater percentage of husband dominant couples than in the scores derived from responses of wives. The difference is not statistically significant between Los Angeles husbands and wives, but when Los Angeles husbands are compared to Detroit wives there emerges a difference figure of 11 points, which is highly significant. The only statistically significant difference between the sexes in the Los Angeles sample (Table 4) is in the autonomic type where a greater percentage figure results from the scores derived from responses of wives. If the male figure for Los Angeles is contrasted with the cor-

Table 4. Percentages in Each Category as Determined by Scores Based on the Original Eight Questions, Los Angeles Sample.

Authority Type	Male's Report	Female's Report	Dif.*
Husb. Dom.	36%	32%	4
Syncratic	35	33	2
Autonomic	25	33	8
Wife Dom.	4	2	2
Total	100 (337)	100 (410)	

\* $\chi^2=16.89$ ,  $df=3$ ,  $p<.001$ .

Table 5. Percentages in Each Category as Determined by Scores Based on Fourteen Decision Questions, Los Angeles Sample.

Authority Type	Male's Report	Female's Report	Dif.*
Husb. Dom.	10%	9%	1
Syncratic	16	20	4
Autonomic	70	67	3
Wife Dom.	4	4	0
Total	100 (337)	100 (410)	

\* $\chi^2=5.037$ ,  $df=3$ (NS)

responding figure for the Detroit sample, there is revealed a difference of 16% in the autonomic type for the two samples. The overall power balance between husbands and wives may not differ between Detroit and Los Angeles, but distribution of power, the *power type*, certainly does.

The distribution of conjugal power is not merely different between the locales of the respective studies. It is strongly influenced by the make-up of the sample of decisions employed to reveal it. Tables 5 and 6 present data on the conjugal authority types obtained by using the augmented, 14-item sample of decisions.<sup>8</sup> The picture changes markedly when viewed by this "camera." What is revealed is a highly egalitarian distribu-

<sup>8</sup> In the computation of husband's mean power for this and all subsequent comparisons, the 14-item sample of decisions is used as the base.

Table 6. Percentages in Each Category for Eight and Fourteen Decision Questions, Los Angeles Sample (N=747).

Authority Type	Eight	Fourteen	Dif.*
Husb. Dom.	34%	9%	25
Syncratic	34	18	16
Autonomic	29	68	39
Wife Dom.	3	5	2

\* $\chi^2=595.31$ ,  $df=3$ ,  $p<.001$ .

tion of power, most commonly with each spouse having large allocations of autonomy in decision making, with the next most common pattern being an authority type—the syncratic—in which each has equal say.

Husband dominant families exceed wife dominant couples in incidence, but both are very much in the minority. It is, even so, a picture revealing an appreciable balance of power in favor of the husband, for the percentage of husband dominant families is still approximately two and one-half times the magnitude of the percentage of wife dominant ones (Table 5). The percentage of the latter, it is worth noting, has hardly varied throughout all the comparisons: between cities, between sexes, between decisional samples—remaining at a stable 2–4 percent for all.

*Personality Variables and Conjugal Power.* As was pointed out in the introduction to the present research, it was our hypothesis that the balance or distribution of power between spouses would be to an appreciable degree influenced by personality traits of the persons involved. In order to examine this, we included a brief Authoritarian-Egalitarian Scale, as well as a scale to measure feelings of Self-Competence. The latter scale yielded no response differences related to conjugal power which either proved to be statistically significant or indicated a trend. However, the Authoritarian-Egalitarian Scale did relate significantly to the conjugal power structure, and in a direction supportive of our hypothesis.

As stated originally, our prediction was that power dominance would be positively associated with higher scores on authoritarianism. That is, more specifically, individuals in husband dominant families and wife dominant families should have higher scores on authoritarianism than those in syncratic and autonomic families. As a test for the relationship, the data were subjected to a simple one-way analysis of variance. The resulting *F* ratio of 4.88 on 3 and 721 *df* is significant at the .01 level, thus supporting the hypothesis.<sup>9</sup> Individual comparisons between

the six pairs of means revealed that respondents in husband dominant families are significantly more authoritarian than those in the syncratic and autonomic families. Though “wife dominant” respondents also have higher mean *F* scores, the small *N* does not allow for an adequate statistical test, so that it is not clearly indicated that this group is significantly different from any of the others. The data, overall, indicate that authoritarian personalities, according to their own reports, are especially likely to be involved in authoritarian conjugal relationships. Further questions need be asked regarding whether it is a matter of authoritarians choosing mates who would support their personalities or whether they tend to force the relationship in that direction after marriage.

*Background Variables and Conjugal Power.* The Detroit study presented a rather exhaustive analysis of the conjugal power balance in terms of demographic variables assumed likely to influence it. Our study was designed to reappraise the contribution of such variables under the special conditions differentiating it from the aforementioned one.

*Place of Birth.* The Detroit survey found no difference between native born and foreign born wives as related to husbands' mean power. Our study confirms this, but also found nativity of husbands nonsignificant in this respect. Further, a separate analysis relating husbands' mean power to age at immigration to the United States also resulted in no difference in husbands' mean power.

*Religion.* We found a small, but statistically significant effect of religious affiliation upon the power of the husband ( $F = 3.17$ ;  $df = 4$  and  $740$ ;  $p < .01$ ). Blood and Wolfe had found no significant effect. Perhaps the margin of significance in the Los Angeles population comes from the somewhat greater percentage of Buddhists and other non-Judeo-Christian religious groups, and Catholic respondents, both of which were above average in husband's mean power.

*Race.* Consistent with Blood and Wolfe, we found a significant effect of race upon conjugal power ( $F = 15.67$ ,  $df = 4$  and  $740$ ;

<sup>9</sup> The number of observations within each category (treatment) varied greatly. The methods used for computation of the analysis and for the ortho-

gonal comparisons that followed it are those outlined by Winer (1962:97–100).

$p < .001$ ). The greatest husband power (mean = 3.17) was found among Oriental couples. Least husband power (mean = 2.79) is found in Negro families. This result would seem consistent with Blood and Wolfe's results and those obtained by King (1967). They are consistent with the observations of Moynihan (1965) regarding the effects of racial discrimination in destroying the power and dignity of the Negro male and contributing to a matriarchal Negro family structure.

*Age.* Age is another variable wherein our data essentially confirm Blood and Wolfe's findings. The power of the husband decreases as a function of age of husband, as well as age of wife. Both relationships proved to be highly significant when husbands and wives are grouped into six age-categories. ( $F = 4.21$ ;  $df = 5$  and  $741$ ;  $p < .01$  for age of husband.  $F = 5.31$ ;  $df = 5$  and  $761$ ;  $p < .01$  for age of wife.) We note that, as in the Blood and Wolfe data, husband power is lowest for couples over 70 (husband's mean power = 2.82 for wives over 70; husband's mean power = 2.78 for husbands over 70). Blood and Wolfe ascribe this decline in husband's power with age to the fact that the younger wife depends more upon the husband for love and affection, and with young children, does not have alternatives. As children get older, they provide the emotional support which had been previously provided by the husband, and her dependence upon husband declines, such that her relative power increases. An additional parallel between these two sources of data, which begs explanation, is the brief but marked upsurge in husband power in the 60-69 year period. This is the only reversal in the general trend toward decline in husband power with age.

*Duration of Marriage.* Blood and Wolfe present a rather detailed analysis of conjugal power balance in relation to the life cycle, attempting to show by their data that the husband's mean power is systematically related to childbearing and child rearing. They found the husband's mean power rising in families with children, from its value in the honeymoon period (married under 4 years) to a high point in the case of couples married 4 to 7 years, with children thus at preschool ages. The husband's power showed a decline

in the group married 8 to 15 years; a further decline with those married 16-22 years; and still more with couples married 22 years and over. They found a regularly declining pattern in the sample of childless marriages. The numbers of cases in several of their cells render their analysis suspect, but they attribute the rise in husband's power—in the 4 to 7 years married couples with children—as due to the wife's retreat from outside employment and confinement to the home because of the heavy demands of child care. All of this supposedly increases temporarily her dependence on the husband, and correspondingly increases his power.

Like Blood and Wolfe's data, ours also provided too few cases of childless couples in certain cells for us to attempt to make that particular comparison, but we did calculate present data on husband's mean power for length of time married. These calculations revealed there is a sharp drop in husband's mean power after the so-called honeymoon period (under 4 years), from where it is highest (3.10) to a fairly stable figure (approximately 2.95) for longer and longer marriages. An  $F$  ratio of 3.39 is significant at the .01 level.

Apparently, power of the husband decreases as the duration of marriage increases; this effect may be a function of the life cycle, but our data are too meager to permit the kind of analysis needed to verify this.

*Education.* Table 7 shows that having completed high school, having some college or having completed college is associated with higher mean power for the husband, whereas having had only grammar school or some high school education is associated with lower power for the husband. These results are consistent with Blood and Wolfe's findings. The trend from high to low is present in both comparisons of husband's education and wife's education, except for a reversal in the case of those wives who completed college. The husband's mean power for families where the husband or wife had no schooling at all represents another pair of exceptions to the trend, but the extremely small numbers of cases preclude any great confidence in the significance of the two figures. Analyses of variance for husband's mean power by husband's education and by wife's education were made.  $F$  ratios were

6.32 and 2.76, significant at the .001 and .02 levels respectively,  $df = 6$  and 740.

Blood and Wolfe found that "generally speaking the higher the husband's occupational prestige, the greater his voice in marital decisions." Occupation also is an index to relative earnings of the person, and both prestige and income derived from occupation are reflected in the social status of the person in the eyes of the community. We examined the influence of occupation in the present study, with a result tending to confirm the previous finding. With the exception of the professional husbands, the higher ranking occupations are associated with a higher mean power for the husband. We suspect that the professional category contains many individuals who are more liberal than others in their views on power distribution between the sexes, and that this ideological factor accounts for their deviation from the trend, which is otherwise one where greater power is associated with higher occupational rank. An analysis of variance of difference within and between seven occupational categories yielded an  $F$  ratio of 2.85, significant at the .01 level,  $df = 6$  and 739.

We also examined the relation of occupation of working wives—that is, those employed outside the home—to the husband's mean power. In this case the  $F$ -ratio was not significant.

#### NEEDS AND SATISFACTIONS IN MARRIAGE

*Marital Satisfaction.* Another variable which was systematically related to conjugal power in the Detroit study was that of marital satisfaction, assessed in that case through the construction of an index derived

from the ranking of the wife's needs and goals in marriage and her stated satisfaction with her need gratification in the marriage. The greatest percentage of Detroit wives with high satisfaction scores was found in the category of syncratic decision making couples. In the present study we employed a single question for assessment of marital satisfaction: "Taking everything in your marriage together, would you say that you are very satisfied, fairly satisfied, or not at all satisfied?"

The resulting percentages of "very satisfied" individuals were for Husband Dominant Families, 73; Syncratic, 70; Autonomic, 79; and Wife Dominant, 20. Our findings agree at least in part with the previous study in that it is in an egalitarian type of power division where we locate the highest percentage of very satisfied spouses, although in our case it is the autonomic rather than the syncratic. Both studies agree, again in finding Husband Dominant families not distinctly inferior to other types as far as proportion of very satisfied husbands or wives is concerned. Finally, both surveys reveal the Wife Dominant family to be a situation associated with least marital satisfaction.

Both sets of data then suggest strongly that families are particularly happy if they can develop procedures for mutual discussion and decision making, and least happy if the woman assumes a dominant role. Since our data are based on husband's responses, as well as wives', this finding is particularly interesting. Of course, by the very nature of the survey data, we cannot reject the reverse causal hypothesis—that happy marital relationships result in an atmosphere in which spouses feel that they can discuss and decide

Table 7. Husband's Mean Power by Education of Husband and Wife.

	Level of Education Achieved						
	Grade School			High School		College	
	None	Some	Completed	Some	Completed	Some	Completed
Husband	3.41 (5)	2.71 (41)	2.80 (51)	2.96 (156)	3.00 (241)	2.97 (147)	3.01 (106)
Wife	3.00 (7)	2.83 (39)	2.85 (44)	2.94 (151)	3.01 (251)	3.01 (140)	2.91 (115)

things mutually, whereas the tension in the unhappy families precludes joint decision making.

*Most Valuable Aspects of Marriage.* Our questionnaire included the same question utilized by Blood and Wolfe to determine what respondents considered the most valuable aspects of marriage. Husbands and wives in our sample did not differ significantly, ranking in order of importance: companionship (most important), children, understanding, love and affection, and standard of living. The wives in the Blood and Wolfe sample also rated companionship most important and standard of living least important; however, the intermediate orderings varied from ours somewhat (Wolfe, 1959). Wolfe found some support for his statement that conjugal power type would be determined by what the respondent considered the most important aspect of marriage. His result, however, was not statistically significant, nor did we find a significant relationship. We did find a significant interaction effect, suggesting that the relationship between marital needs and power was greater for husbands than for wives. A more complete report of these findings is presented elsewhere (Centers *et al.*, 1969).

*Prior Marriage.* There would be good reason to expect that the extent of conjugal power would be affected by prior marriage. However, the exact direction of such an effect was not readily predictable. Prior divorce and remarriage might serve to undermine the confidence of the partner, make him more fearful of a further marital failure, and thus reduce his power. It is also possible that power would be reduced through prior marriage if divorce would reduce his alternate possibilities of remarriage. On the other hand, the effects of prior marital experience might make the partner more experienced and adept in converting his resources into power over his mate. Thus we did not have specific prior predictions regarding the effect of this variable in general, or whether the effects would be the same for husband as for wife. Our findings suggest that loss of power through remarriage is particularly great for husband. The husband has the greatest power where both partners are in their first marriage (husband's mean power = 3.00); the husband has the least power where both

partners have been previously married (husband's mean power = 2.82). The wife's remarriage does not have a significant effect upon conjugal power. The husband's remarriage, however, significantly reduces his power in the family ( $F = 8.4$ ,  $p < .01$ ). It should be noted that these relations are possibly confounded by a number of other variables. For example, the remarried couples are likely to be older, and are therefore subject to the effects of this factor. These possibly confounding variables could not be controlled without reducing the N's in each cell to an unreliable level.

#### RECAPITULATION AND DISCUSSION

The present report undertook to reassess the conjugal power balance and distribution as reported in an earlier research, faulting the previous work in its seemingly biased sample of decisions, in favor of the husband, and with the aim of supplying comparative data from husbands' responses as well. Further, it sought to review the relationships of conjugal power to an array of demographic variables under our revised procedural conditions. Finally, the attempt was made to examine the relationship of some variables hitherto not attended to in relation to conjugal power.

On balance, our work is essentially confirmatory, but yet substantially modifies and augments the picture of conjugal power previously presented. Clearly the balance and distribution of power between married persons will reveal itself to be different with the difference in instruments designed to assess it. We think our picture a better reflection of the reality because of a better balanced sample of decisions. A reassuring finding is the essential agreement in reports of husbands and wives, thus strengthening confidence that the phenomena Blood and Wolfe described were not merely the creature of wifely vanity or timidity. Still more reassuring is the consistency with which we were able to confirm earlier findings with regard to the determinance of the various demographic variables.

New, with this study, is the finding that a personality variable such as authoritarianism is positively and significantly related to the exercise of power in marriage. Finally,

the revelation that the existence of previous marital experience can and does affect the power balance of spouses is new. Both findings suggest future research possibilities in the area of inter-personal power centering on the contribution that may be made to balance and distribution of that power by specific personality traits.

*Consideration of Factors Affecting Relative Power.* What factors, then, contribute to relative power of husbands and wives? We list some possibilities and indicate some directions from our data:

(1) *Role patterning.* It is clear that husbands do indeed have more to say about decisions in some areas than in others. Thus, in the Blood and Wolfe study and in ours, we can see a continuum of husband power ranging from low (choice of food) to high (choice of husband's job). The domain of authority and role relationships become especially important in selection of decision areas in development of the measure, as we have indicated above.

(2) *Personality.* We have gone beyond the original studies in showing the importance of authoritarianism, indicated in the F scores of husbands and wives. Both husbands and wives with high F-scores tend to be found in families where one spouse is dominant over the other—and, in all but a few such cases, it is the husband who is dominant. There are undoubtedly other personality variables which are also related to relative power.

(3) *Cultural factors.* Rodman (1967) has indicated the importance of cultural factors in affecting the degree of power of husbands and wives. Clearly there are differing norms as to the extent to which the husband should have power in the family. A subsequent study comparing current family structure with that reported in the previous generation indicates that couples did indeed report that their fathers had relatively more conjugal power than the husbands in the present generation (Centers *et al.*, n.d.). Data in the present study show cultural differences related to religion and race. It is also possible that the difference in power of husbands as a function of occupation reflects subcultural differences.

(4) *Control of valued resources.* Blood

and Wolfe (1960) found that much of their data was consistent with a resource control theory—the more a partner controlled resources of value to himself and his mate, the greater his relative power. Heer (1963), in his comment on Blood and Wolfe, emphasized the importance of considering the comparison levels of alternatives—gaining desired resources through divorce and remarriage. Unfortunately, our data are not as clearly supportive of the valued resources approach. We did find that husband's power increased with his level of occupational prestige and income. However, we did not find that his power decreased if the wife was employed. Husband's power increased with his level of education, but it also increased as a function of wife's education, which tended to be correlated. Our results with regard to power as function of length of marriage, initially parallels Blood and Wolfe's—husband's power is greatest during the honeymoon period (first four years). However, contrary to Blood and Wolfe's study, husbands in our sample of families did not show a further decline in husband power thereafter. Further, the Blood and Wolfe interpretation of the age differences—greater wife dependence upon husband for satisfaction of affectional needs—is not supported in our data on individual differences in needs.

(5) *Relative competence* and (6) *relative involvement.* Heer (1963) suggests both of these factors as important bases of power in the family. One would expect here, as elsewhere, that the spouse who is more knowledgeable in a given area would have more expert power. It should be the case that a partner who feels more involvement in a decision would exert greater power in that area. Though conceptually distinct, these two factors in family power cannot always be distinguished in the data. It is reasonable to assume that each person has more involvement in the selection of his own clothes, and to be sure he does have more power in that domain. But is this a case of mere involvement, or is expertise also involved? Both involvement and competence might account for the wife's greater influence on the dinner menu, decoration of the house, planning of dinner menu, and purchase of food. Similarly, both variables might account for

husband's greater power with regard to his choice of job and the purchase of a car. Some clarification could have been added with the simple questions, "How important is this decision to you personally?" and "How does your knowledge in this area compare to that of your spouse?" Unfortunately, even so, the results would likely be clouded by a rather high correlation between responses to the two questions.

The confounding of the last two variables is undoubtedly found to a considerable extent among the other variables as well. Cultural factors undoubtedly affect each of the others. Rodman (1967) does find national differences, for example, in the extent to which comparative resources determine conjugal power. Role patterning must be determined in part by personality differences. Relative competence and involvement must both, in turn, affect and be affected by role patterns and personality. The problems of disentangling the variables with one-shot survey data become immense, and we do not claim to provide ready answers in this study. What is clear, however, is the futility of attempting to explain conjugal power in terms of one variable, such as control of resources.

A subsequent study (Raven *et al.*, 1969) attempts to provide some clarification through focusing on the bases of power as presented by French and Raven (1959). By direct questioning, one can examine the extent to which power in a family, or in a specific domain of decisions, is determined by expertise, identification (reference), control of resources (rewards) or punishments (coercion), or acceptance of norms of legitimacy. It is hoped that such an analysis might provide some deeper clarification of the bases of conjugal power.

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## ADMINISTRATIVE RATIOS AND ORGANIZATION SIZE: A LONGITUDINAL EXAMINATION \*

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*American Sociological Review* 1971, Vol. 36 (April):278-286

*Data were obtained on the administrative components of 41 urban school systems in western Canada for a five-year period: 1964-65 through 1968-69. Cross-sectional analysis, using multiple definitions of administrative ratio, showed that larger system size tended to be associated with a smaller administrative ratio. The only exception was provided by the ratios based on the number of professional staff, such as psychologists, social workers and consultants, which showed increases with system size increases.*

*However, the 41 graphs of changes in administrative ratio in each system showed no consistent tendency to rise or fall over the five-year period. Longitudinal analysis of individual systems therefore did not seem to support the general cross-sectional inference of this and other studies. The findings tend to confirm the suspicions of some writers that cross-sectional analysis performed at one time can lead to faulty inferences concerning growth patterns of organizations.*

THIS article presents information concerning the association between the size of an organization and the proportion of its personnel occupied in administration and other supportive activities. The *American Sociological Review* has presented several articles on this topic as exemplified by those of Terrien and Mills (1955), Anderson and Warkov (1961), Hawley *et al.* (1964) and Haas *et al.* (1967). However, these authors tended to concentrate upon cross-sectional data, and inferred that the patterns of administrative ratio to organization size reflected growth curves. That is, the inference was made that the cross-sectional curve, obtained by plotting administrative ratio against size for different systems, would generally represent the way in which this ratio changed as a system grew

larger. Haire (1959:292) holds that such curves may be spurious because the static measurements do not reflect the dynamics within an organization.

Previous studies have also been limited by their adherence to one definition of "administrative component." Examination of the functions of "administrators" in various types of organizations shows that the gross placement into one category of people in different positions causes distortion and reduces comparability of results among different research projects. If the administrative component is classified as that prorated section which is not directly productive, then people who manage the organization, direct staff, or advise staff could all be included. However, the classification would vary in different organizations: for example, although social workers in a school system could be classified as professional support staff, in a social welfare organization they could be considered as production staff. This article emphasizes both the use of multiple defini-

\* The assistance given by David Friesen, Erwin Miklos and Rajinder Pannu, of The University of Alberta, is gratefully acknowledged, as is the research grant provided by The Alberta Advisory Committee for Educational Studies.

tions of administrative staff and the longitudinal dimension which is introduced by taking measurements in each system over time.

Although this area of study may appear to belong most properly to administrative science, it is also of interest and concern to organizational sociologists. As Perrow (1970:2) has stressed, most organizational analysts are primarily concerned with "the organizational explanation of organizational behavior," and with "the structures in which roles are performed." For example, in opposing the view that organizational performance can be upgraded by merely improving the calibre of personnel, he states (Perrow, 1970:4): "But sociologists challenge this view by arguing that people's attitudes are shaped at least as much by the organization in which they work as by their preexisting attitudes."

In this regard, and in the context of this article, one can legitimately propose that organizational performance is affected by the proportion of staff classified as administrative; obviously, varying administrative ratios can affect working relationships and individual aspirations. Further, because much of the work of administrative personnel is not directly productive, maintenance of the size of this component at the lowest level congruent with organizational efficiency is of considerable practical concern.

The bureaucratic model of organizations proposed by Weber incorporates the idea of dominance of the administrative component in direction of activities, communication and coordination. In addition to this idea of dominance, the notion is commonly held that disproportionate increases occur in the size of administrative components of organizations over time. Examination of growth in the relative size of this component can provide information which allows this proposition to be critically examined.

Much of the interest in the administrative ratio was generated by Parkinson (1957), who proposed that the number of officials in an organization increases regardless of the quantity of work to be done. Without directly saying so, Parkinson inferred that the ratio of officials to workers increased over time. Blau and Scott (1962:266) state that Parkinson's data concerning the dispropor-

tionate peacetime growth of the British Admiralty were misleading because this increase could have been expected in view of technological advances and the need for a peacetime elite to handle future emergencies. They further point out that the evidence does not support this widely held assumption of disproportionate administrative growth.

Starbuck (1965:519-520) states that the paucity of research in this area increases the hazards of drawing conclusions regarding organizational size and its relationship to the administrative component. The need for more data "on nearly every aspect of organizational growth and development" is stressed by Starbuck (1965:520), who elaborates in this way: "In the interim, the best approach is to work with multiple observations of each organization, taking maximum opportunities to compare each organization with itself rather than with other organizations."

#### PRIOR RESEARCH

##### *Studies of Noneducational Organizations*

A recent article by Tosi and Patt (1967) provides a summary of the findings of previous studies of the administrative ratio. Here, we note that business organizations were studied by Melman (1951), veterans' hospitals by Anderson and Warkov (1961), army hospitals by Tosi and Patt (1967), 30 different types of organizations by Haas *et al.* (1963), and five different types of organizations by Indik (1964). Haas *et al.* (1967) have also reviewed the conclusions of earlier studies, and investigated and discussed in depth the relationship between size, organizational structure and complexity.

All of these studies except Melman's were cross-sectional, and, despite slightly different definitions of administrative component and operational size, showed a tendency towards smaller administrative ratios with larger organization size.

##### *Studies of Educational Organizations*

Terrien and Mills (1955), in a study of California school organizations, found that the relative size of the administrative component increased as the size of the organization increased. However, Gill (1967), in a study of western Canadian school systems,

found that the relative size of the administrative component decreased as the size of the organization increased.

In a study of 97 institutions of higher education, Hawley *et al.* (1965) found that the number of full-time administrators per 100 faculty members tended to decrease with increasing size, and that faculty size was far more important in determining this ratio than were budget, complexity (number of departments and schools), or quality (per cent of faculty with a Ph.D. degree).

Anderson and Warkov have suggested that the contrary findings of Terrien and Mills may be associated with the number of places at which work is performed in an organization. Hawley's research tends to negate this suggestion, although the geographical areas occupied by a university campus and a school district are usually quite dissimilar.

In a longitudinal study of the very large school systems of New York, Chicago, Detroit, St. Louis, Baltimore and Philadelphia, Gittell and Hollander (1968) found that the number of administrators per 1,000 pupils or per 100 teachers doubled for New York between 1955 and 1965, and rose by slightly less than one-third for Detroit. For all other cities the ratios remained approximately the same.

#### METHOD

The purposes of this study were (1) to determine whether consistent changes occurred over a five-year period in the administrative proportions of different school districts; and (2) to examine the cross-sectional relationship between the size of school districts and the percentage of staff in the administrative positions. The longitudinal examination was restricted to the five-year period by reason of problems caused by amalgamation of school districts and the difficulty of obtaining district records for a longer period.

#### Hypotheses

Two major hypotheses, based upon the opinion of sociologists and research findings cited above, were postulated for testing in this study.

*Hypothesis 1.* In view of the reservations expressed by Haire and Starbuck concerning lack of congruence between cross-sectional

graphs of different systems and growth curves for individual systems, and by virtue of Gittell's findings, Hypothesis 1 was stated in this way: The cross-sectional association observed between administrative ratio and organization size will not correspond with the general growth pattern observed for each individual system over the five-year period.

*Hypothesis 2.* Because only one study (Terrien and Mills, 1955) of the seven studies cited above did not find smaller administrative ratios in larger systems, Hypothesis 2 was developed in this form: A negative relationship will exist between the administrative ratio and size of organizations.

#### Sample

The sample consisted of forty-one ( $N=41$ ) urban school systems in western Canada, with nine in British Columbia, eighteen in Alberta, eight in Saskatchewan and six in Manitoba. Most of the largest school systems in each of the four western provinces of Canada were included as well as systems with minimal numbers of administrative staff. The superintendent of each school system supplied the required data for the five school years, 1964-65 through 1968-69.

#### Definition of Terms

Most of the earlier studies have used only one definition of administrative component and one definition of system size, resulting in only one definition of administrative ratio, that is, size of administrative component divided by size of system. A penetrating investigation of the relationships, which allows for comparability of results from different studies, demands *multiple definitions*; computing flexibility now enables the values of these different administrative ratios to be easily obtained.

(a) *Administrative component* was defined in three ways:

(i) AP—*central office administrative personnel*, that is, staff not directly involved with pupils, but concerned more with planning, organizing, coordinating, etc.

(ii) PP—*central office professional personnel*, that is, university-trained, non-administrative staff, such as psychologists, social workers and teaching

consultants. (Although such personnel are not strictly managerial, they do constitute part of the administrative component in its broad sense.)

(iii) APP—*central office administrative personnel plus school principals.*

(b) *System size* was defined in four ways:

(i) NS—*number of schools*, that is, the number of service locations;

(ii) NP—*number of pupils*, that is, the number of clients;

(iii) NPA—*number of professional and administrative staff in the system*, that is, central office administrative and professional personnel plus school principals plus classroom teachers;

(iv) NCRT—*number of classroom teachers*, excluding principals.

(c) *Administrative ratio* was defined in seven ways.

<i>Admin. Ratio</i>	<i>Admin. Component</i>	<i>System Size</i>
1 AP/NS	(i)	(i)
2 100 AP/NPA	(i)	(iii)
3 100 PP/NPA	(ii)	(iii)
4 APP/NS	(iii)	(i)
5 1000 APP/NP	(iii)	(ii)
6 100 APP/NPA	(iii)	(iii)
7 100 APP/NCRT	(iii)	(iv)

Personnel who could not be categorized as teachers, professional employees, or administrators were excluded; that is, nonprofessional clerical, custodial, cafeteria, transport, stores, equipment and maintenance staffs are not represented in the data.

## RESULTS

The data were analyzed by product-moment correlation, analysis of variance, and regression techniques. Results from the cross-sectional analysis are presented first, and then the longitudinal growth patterns are described and compared with the cross-sectional findings.

### Cross-sectional Analysis

Significant negative correlations ( $p < .001$ ) were obtained between the appropriate measures of system size and each of the administrative ratios AP/NPA, APP/NP, APP/NPA, and APP/NCRT. This occurred in each of the five years 1964–65 through 1968–69—the ranges of the coefficients for the five separate years are listed in Table 1. Hypothesis 2 was therefore supported.

Administrative ratios AP/NS, and APP/

NS, which used number of schools as the denominator, both showed nonsignificant coefficients with all four measures of system size. However, this finding does not materially detract from the general support for Hypothesis 2, the number of people in an organization is presumably a more meaningful measure of size than is the number of locations.

Administrative ratio PP/NPA, which used central office *professional* staff as its numerator, showed consistent and significant positive coefficients ( $p < 0.01$ ) with all four measures of organization size. Thus, larger school systems are employing a higher proportion of professional staff, and Hypothesis 2 was not supported using this definition of administrative ratio.

In order to compare the findings of this study with those of Terrien and Mills (1955) and Gill (1967), the school systems in the sample were categorized on the basis of the total number of professional and administrative staff in 1968–69 into (1) small, medium, and large systems, and (2) very small, small, medium, and large systems. The means of administrative ratio APP/NPA using these subdivisions are shown in Table 2. In each case, a decrease in mean administrative ratio occurred with increasing size, supporting the correlation findings and Hypothesis 2. The Scheffe analysis of variance test showed a statistically significant difference ( $p < 0.10$ ) between 9.61 and 6.88; 9.66 and 7.07; and 9.66 and 6.62.

Using the Scheffe test and the .10 alpha level, analysis of variance was again used to determine whether or not significant differences existed among school systems of different sizes on the other six administrative ratios. The results are listed in Table 3 and shown in Figure 1.

The following observations were made:

1. No systematic increase or decrease occurred using number of schools as the denominator, i.e. in ratios Nos. 1 and 4.
2. A systematic increase occurred in the mean value of ratio No. 3.
3. In ratios 2, 5 and 7, a general decrease in mean ratio occurred with increase in group size, but some curvilinearity occurred in the large group in ratios 2 and 5.
4. In ratios 2, 3, 5 and 7, the differences between the smallest and largest ratios were all significant at the 0.10 level.

Table 1. Range over Five Years of Pearson Correlation Coefficients between Administrative Ratios and Four Measures of System Size (N=41).

Administrative Ratio	Extreme values of Pearson coefficients <sup>a</sup> for correlations between particular administrative ratio and particular measures of system size			
	Number of Schools	Number of Pupils	Number of Staff	Number of Teachers
1 Central office administrative staff per school	.04	.04	.06	.06
	-.21	-.13	-.12	-.07
2 Central office administrative staff per employee	-.31	-.33	-.30	-.31
	-.42	-.43	-.41	-.42
3 Central office professional staff per employee	.39	.41	.42	.41
	.31	.36	.37	.36
4 Total administrative staff per school	-.04	.04	.03	.06
	-.14	-.01	-.05	-.05
5 Total administrative staff per pupil	-.41	-.48	-.47	-.47
	-.45	-.51	-.48	-.49
6 Total administrative staff per employee	-.32	-.43	-.42	-.43
	-.39	-.47	-.45	-.45
7 Total administrative staff per teacher	-.32	-.42	-.42	-.42
	-.38	-.46	-.44	-.44

<sup>a</sup>A Pearson coefficient (*r*) of 0.26 is just significant at the 5% level on a one-tailed test.

### Regression Analysis

Regression analysis was used separately on each of the seven administrative ratios to determine whether the relationship between each of these ratios and the size of the school system in terms of the total professional and administrative staff was of the form  $x = e^{a-by}$ , as suggested by Indik (1964). In this equation, "x" represents the size of the school system and "y" represents a particular administrative ratio.

Using the above equation, regression analysis was applied to each ratio separately for each of the five school years and for all

five school years combined. The relationships between the size of school systems and each of the administrative ratios 2, 5, 6 and 7 were well represented by the graph  $x = e^{a-by}$  with a negative slope. The relationships between size and administrative ratios 1 and 4 (using number of schools as the denominator) were both not of that form. The relationship between size and percentage of staff in professional positions was of the form  $x = e^{by-a}$  with a positive slope.

The line of best fit for the relationship between administrative ratio 6 and total staff appeared as a straight line of negative slope on a semilog grid and as a curve on a normal

Table 2. Mean Ratios of Personnel in Administration for Three and Four Groups of School Systems

Group	N	Number of Personnel	Mean Admin. Ratio APP/NPA
Small	16	56-185	9.61
Medium	13	267-616	8.57
Large	12	904-3700	6.88
Very Small	14	56-143	9.66
Small	12	171-400	9.10
Medium	9	515-1147	7.07
Large	6	1387-3700	6.62

grid. The same relationship held for the data for each year. The curvilinear equation was a better fit to the data than was a straight line equation  $y = ax + b$ .

Taken collectively, the results obtained from correlation analysis, analysis of variance, and regression analysis supported Hypothesis 2.

#### *Graphs for Each System of Administrative Component vs. Time*

The individual district graphs of administrative ratios over the five-year time period of the study showed little consistency. Be-

cause education is a provincial responsibility in Canada, the graphs of the forty-one systems were arranged by province to see if any regularity of pattern occurred. The greatest tendency toward regularity of pattern occurred in the British Columbia school systems as shown in Figure 2: six systems showed decreases from 1964 to 1968, whereas the ratios were approximately constant for the other three systems. For the other three provinces, a diversity of graphs of administrative ratios over time was obtained. This diversity is exemplified in the graphs in Figure 3 for Manitoba school systems in which no overall trend towards increase or decrease is evident.

The obtained variety of graph patterns appeared to provide support for Hypothesis 1. That is, over the five-year period, most of the selected school districts did not display decreasing administrative ratios with increasing size which cross-sectional analysis would appear to indicate.

#### DISCUSSION

The above results can be summarized as (1) the larger school systems showed a strong tendency to have smaller administrative ratios than did the smaller school systems—this usually occurred regardless of the definitions used for administrative ratio and system size; and (2) this tendency to smaller administrative ratios as size increased over

Table 3. Means of Six Administrative Ratios for Four Groups of School Systems.

Group	N	Mean Administrative Ratio†						
		1	2	3	4	5	6	7
Very Small	14	0.67	3.84	0.00	1.58	4.86	9.66	10.78
Small	12	0.70*	3.69	0.49*	1.71*	4.34	9.10	10.11
Medium	9	0.52	2.51	0.63*	1.42	3.08	7.07	7.69
Large	6	0.76*	2.91*	1.34*	1.73*	3.16*	6.62	7.20
Significant Difference between Pairs		Nil	1-3	1-4	Nil	1-3 1-4 2-3	1-3 1-4	1-3 1-4

† See p. 284 for composition of ratios, Nos. 1-7.

\* Indicates a ratio which is not in decreasing sequence as size increases.

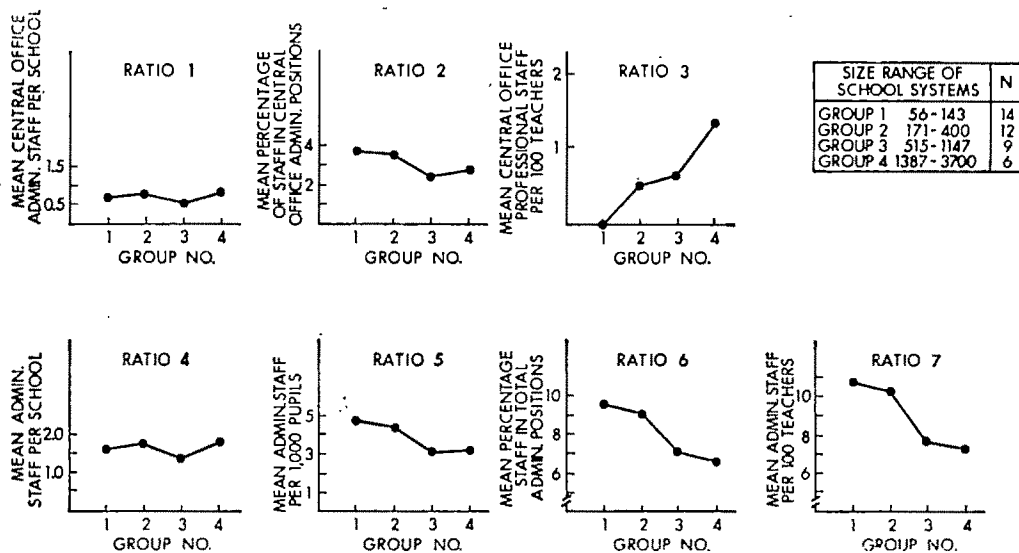


FIGURE 1: COMPARISON OF MEANS OF SEVEN ADMINISTRATIVE RATIOS FOR 41 SCHOOL SYSTEMS GROUPED BY NUMBERS OF PERSONNEL

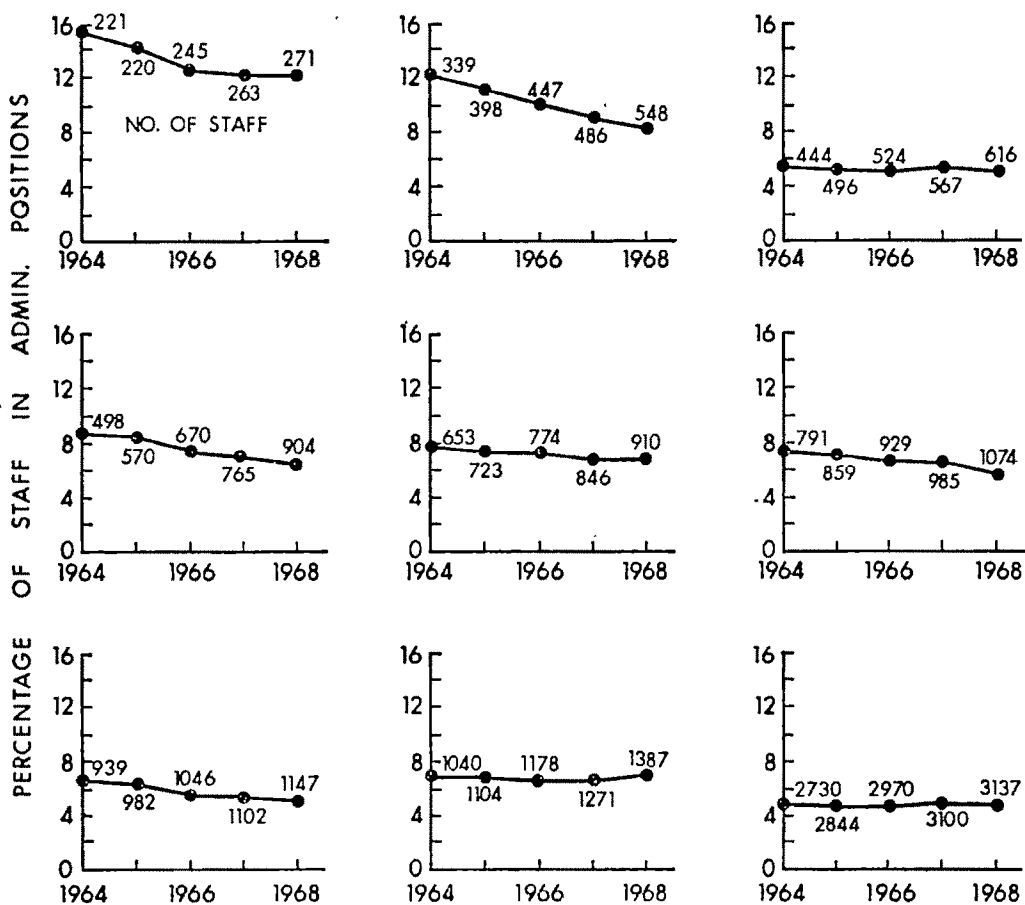


FIGURE 2: PERCENTAGES OF STAFF, PER YEAR, IN ADMINISTRATIVE POSITIONS (ADMINISTRATIVE RATIO 6) IN NINE BRITISH COLUMBIA SCHOOL SYSTEMS

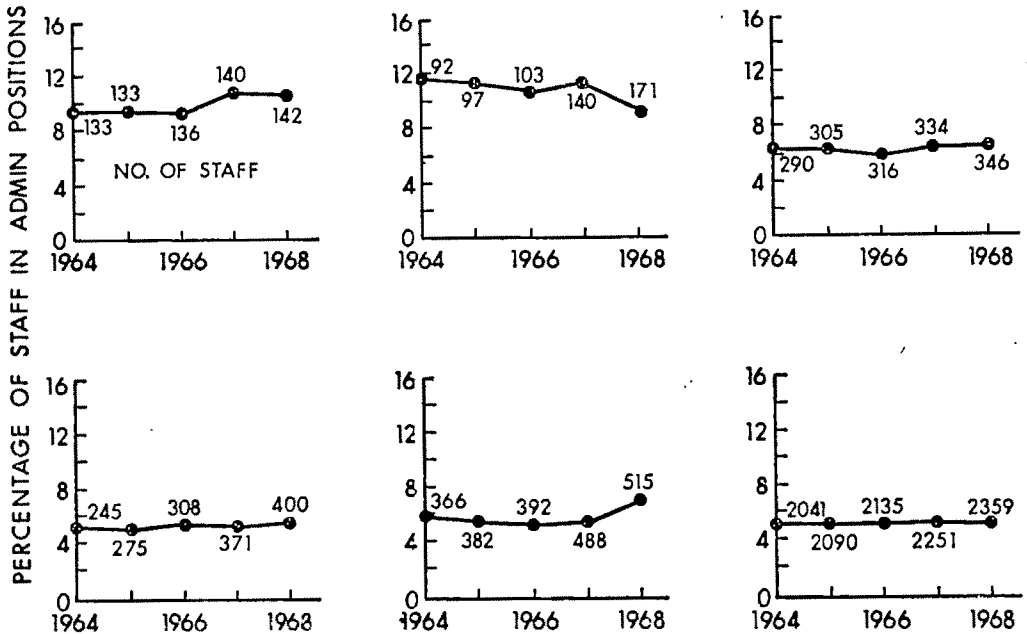


FIGURE 3: PERCENTAGES OF STAFF, PER YEAR, IN ADMINISTRATIVE POSITIONS (ADMINISTRATIVE RATIO 6) IN SIX MANITOBA SCHOOL SYSTEMS

the five-year period was not generally observed for the school systems taken individually.

This second finding provides evidence which supports the contention of Haire (1959) and Starbuck (1965) that cross-sectional data may not provide accurate growth curves. Researchers of organizations should be aware that this situation may also apply to the development over time of variables other than the administrative proportion.

These findings suggest that both time and organization size need to be considered when analyzing the size of the administrative proportion of different organizations; probably other variables also need to be considered. For example, in the educational scene, the perceptions of local school board members and senior officials concerning the need for existence of particular positions such as coordinators of instruction or directors of adult education obviously can affect the administrative ratio. So also could the appointment of a new superintendent, or the onset of periods of financial restriction, or amalgamations of separate districts. Similar factors could affect the size of the administrative ratios in other types of organizations such as social welfare, hospitals, and trade unions.

Further, differentiation within the administrative component seems to be of value. As reported earlier, the central office *professional* staff ratios showed positive relationships with organization size, whereas the administrative ratios, using "administrative" in the managerial sense, showed negative relationships. For the purposes of this study, "professional staff" included those providing *direct assistance* to staff and students, as compared with those occupying line positions and having *indirect contact* with in-school personnel. This greater proportion of professional staff in larger school systems suggests four possible factors: (1) a greater social need for providing assistance to students in the larger city systems, (2) a philosophy of concern for the individual, (3) larger systems are better able to provide the financial resources necessary to employ consultative staff, and (4) recognition of the need for specialization and division of labor in large systems. Consideration of the professional staff ratio findings led to the conclusion that this component should be further separated into those working mainly with students and those working mainly with teachers. Inclusion of those providing such assistance within schools is also necessary to establish a complete picture of the personnel utilization.



tion of the system, and this, along with inclusion of the prorated time of in-school administrators, is planned for an extension of this study. Such differentiation of staff according to function, and consequent formulation of different staffing ratios, would appear to have potential application to other types of organizations.

In examining changes in staffing ratios over time, researchers should be aware of changes brought about in the functions and attitudes of staff by the recognition that administration is a full-time requirement for certain positions, by the establishment of psychological and welfare services within organizations, and by movements such as professionalization. These changes, which have occurred in educational systems in recent years, can also be recognized in many private and public organizations. The number of people occupying particular positions usually reflects organizational ideologies and priorities in allocation of financial and human resources.

At this stage, reference to Starbuck's (1965) admonition concerning the need for more data on organizational development is appropriate. Detailed inference from the above findings is not warranted because of the limitations of small sample size and small time-period. However, the researchers became aware of the need to include variables other than organizational size when examining changes over time in administrative ratios. The aspect of rates of change of administrative ratios could also be included. Finally, comparative growth information of larger numbers of organizations, which should be readily available, would help to shed further light upon a vexing but important aspect of organizational theory.

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# THE SROLE ITEMS AND ACQUIESCENCE \*

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American Sociological Review 1971, Vol. 36 (April):287-293

*Twelve years ago Lenski and Leggett found that race and class factors produced a strong agreeing tendency to one of the Srole "anomie" items. Many subsequent applications of the Srole items have failed to take into account the implications of their findings. Most of the research on the subject of acquiescence has been done on the F scale by psychologists who have explained it as a personality characteristic. The technique of comparing scale items to the same items in obverse form has been the principal research technique. The same technique is used here in a sociological perspective. One-half of a sample of poor Southern Negroes was given the Srole items and the other half was given the same items in an obverse form. Analysis shows a strong tendency to agree with both forms despite the fact that they had opposite meanings. The implications of this for the use of the Srole items and similar scales are examined.*

THE purpose of this study is to show that the Srole (Srole, 1956:709-716) "anomie" items are, in large part, a measure of acquiescence when the items are administered to respondents with socially subordinate racial and class characteristics. Also, an examination is made of the extent to which the measurement of acquiescence is reduced by matching the race of the interviewer with the race of the respondent.

This study is a partial replication of the Lenski and Leggett (1960) research done in Detroit twelve years ago. In that study the Srole items were included in a cross-sectional survey along with an additional item which was the obverse form of one of the original Srole items. The Srole item and the obverse form of it were respectively:

1. It's hardly fair to bring children into the world the way things look for the future (Srole, 1956).
2. Children born today have a wonderful future to look forward to (Lenski and Leggett, 1960:464).

The researchers found that 8% of all respondents agreed with both statements even though they were contradictory. Two-thirds of those who agreed with the Srole item also agreed with the obverse form of it. Furthermore, 20% of the Negro respondents agreed with both items compared

with only 5% of the white respondents. The tendency to agree with both items was related to education for both races. However, with education held constant, the proportion agreeing to both statements was still higher for Negro respondents. Among whites, class was related to agreeing to both items but for Negroes this relationship was not tested.

These findings were interpreted to mean that norms governing interclass and intercaste relationships caused the respondents to agree with the items out of deference or acquiescence to the superior position of the interviewer. More specifically, the researchers called into question the identification, common in the literature, of the lower class as "anomic." When they controlled for acquiescence in the interview, they found no relationship between class and "anomie" scores. This, they noted, was a case very similar to the characterization of the lower class as "authoritarian" (Adorno *et al.*, 1950). That is, the Srole items, like the F-scale items, were "loaded" in such a way that, due to acquiescence, lower class people necessarily scored higher on the items than did respondents of higher classes.

Nevertheless, recent numerous sociological studies employing the Srole items have generally disregarded the acquiescence problem (see, for example, the annotated bibliography of Clinard, 1964:223-283 on anomie and deviance).

In psychology, on the other hand, there has been a great deal of concern with the problem of acquiescence and a large body

\* I would like to express my appreciation to Dr. Gerhard Lenski and Bruce K. Eckland for their helpful comments on this article.

of research has accumulated indicating the serious and ubiquitous nature of the problem. This research, however, has viewed acquiescence strictly as a psychological trait and not as a phenomenon in the class and racial structure of society. In particular, the effect of positive or negative scoring has been intensively studied in the F scale (see Bass, 1955 and 1956; Small and Cambell, 1960; Nadler, 1959; Gage and Chatterjee, 1960; Christie *et al.*, 1958; Couch and Keniston, 1960; Campbell *et al.*, 1967; Landsberger and Saavedra, 1967). Bass's conclusion about the F scale was that it "has less to do with the content validity of the items than with the response set to acquiesce to any generalizations about social issues—authoritarian or equalitarian" (1955:616).

In 1956, Bass developed a 56-item "social acquiescence" scale, including such items as: "Seeing is believing," "Make yourself honey, and flies will eat you," and "The feeling of friendship is like that of being comfortably filled with roast beef" (Bass, 1956:297). The inclusion of absurd items in the scale was both humorous and enlightening. His scale showed that in order to measure acquiescence the form of items should be positively scored and the content should both be ambiguous and express doctrinaire opinions about social issues (Bass, 1955:616).

Clearly, the Srole scale has these three characteristics: It has unilateral positive wording; it expresses opinions about social issues that are practically clichés; and the items are quite ambiguous.

Couch and Keniston's 1960 study of "yeasaying" and "naysaying" to positively and negatively scored statements confirmed the pervasive nature of the problem of response-set to word form in these types of scales. They concluded that the effect probably could not be eliminated and they noted: "The sum of any group of Likert-scale items scored positively will always be a somewhat valid, although perhaps unintended, scale for response-set" (1960:169). In contrast, however, several writers (Nunnally, 1967; Rorer, 1965; McGee, 1962) have argued that those who have examined acquiescence have failed to show that there is a broad personality trait of acquiescence, and they

have argued that acquiescence does not invalidate measures of personality.

Be this as it may, the concern here is with acquiescence as it is socially located. There is research which supports the findings of Lenski and Leggett (1960). Dohrenwend (1966), for example, found that Negro respondents had a clear tendency toward "yeasaying" and that the tendency was inversely related to education. Williams (1964) has reported that the biasing effect of white interviewers is less pronounced among high-status Negroes than it is among low-status Negroes. Price and Searles' (1961) study in North Carolina found that when Negro respondents were questioned by a Negro, in contrast to a white interviewer, Negroes mentioned higher educational aspirations for their children and more often agreed that changes must be made "in the way our country is run."

Pettigrew, likewise, reported in a Boston survey strong differences between the responses of Negroes to Negro and white interviewers. With Negro interviewers, 21% more Negro respondents agreed to the statement that "the trouble with most white people is that they think they are better than other people" (Pettigrew, 1964:51). It has been shown that Negro children only two years old display restricted verbal responsiveness when examined by a white person (Pasamanick and Knobloch, 1955) and that reactions of Negroes to racially derogatory terms were repressed in the presence of white experimenters (Whittaker *et al.*, 1952).

There is general evidence, then, that acquiescence, in the case of Negroes to whites, is a function not only of psychological factors but of the social situation in which interaction takes place and that such acquiescence may greatly influence the responses obtained. Therefore, the finding that Negroes are more likely than whites to have high "anomie" scores on the Srole scale (McClosky and Schaar, 1965; Killian and Grigg, 1962) may, in part, be explained in terms of racially based acquiescence. The same explanation may account for a significant part of the variation in Srole-scale scores between social classes. Numerous studies have shown that the various indices of class position are inversely related to "anomie." High "ano-

mie" has been associated with (1) low-socioeconomic status (Meir and Bell, 1959; Kornhauser *et al.*, 1956:189-200); (2) low occupational status and low education (Bell, 1957; Simpson and Miller, 1963); (3) lower class identification (Mizruchi, 1960); and (4) low income (McCloskey and Schaar, 1965; Bell, 1957).

If Srole scores are partially a function of class- and race-based acquiescence, "anomie" may have been spuriously correlated with other attitudinal variables for the same reason. That is, acquiescent respondents who will agree to almost any statement will necessarily be high on "anomie" and any other positively worded scale. Couch and Keniston (1960:169) commented on the seriousness of this problem: "No matter how a scale is developed, it is virtually impossible to eliminate the effect of agreement bias in an S's answer pattern. As a consequence questionnaire items seem almost possessed by a tendency to correlate positively with one another regardless of content." The question of the spurious correlation of the Srole items—and similar measures—with other class and racially related indices is one of the serious questions associated with the acquiescence problem. In the following study, acquiescence is examined in a lower class Southern Negro sample. In this research, all the items of the Srole scale were put in obverse form and compared with the original items. The effect of interviewer's race was also investigated.

#### THE SAMPLE

In the summer of 1968, a questionnaire survey was conducted in Chapel Hill, North Carolina, and the adjoining town of Carrboro. The sample was drawn from the list of the poor compiled in 1965 by the local Office of Economic Opportunity. Four hundred families were listed and 332 were interviewed. Those not interviewed either could not be located or refused to participate. This study is concerned with the 90% of those on the list who were Negro. Two white interviewers interviewed 23% of the Negro respondents and the remainder were interviewed by four Negro interviewers; all of the interviewers were college students. Due to differences in interviewing assign-

ments, that part of the population interviewed by Negroes had more respondents in the higher income and education categories. Since it is known that income and education effect Srole-scale scores, comparisons could not be made between white and Negro interviewers without controlling for these differences. Therefore, respondents with an annual family income above \$4000 or with twelve years or more of education were dropped from the analysis. There were not enough cases for separate statistical analysis of the high income and high education group. Of the remaining 151 cases, all of whom were lower class, 57 cases were interviewed by whites and 94 cases were interviewed by Negro interviewers.

The presentation of the obverse form of items often carries with it the risk that respondents may detect the similarity between the original and the obverse items and try to give consistency to their responses. One approach that has been used is to overwhelm the respondent with so many items that he is not likely to remember what he has said (Couch and Keniston, 1960). Another approach is to give the alternative items after some time has elapsed (Bass, 1955). In the Lenski and Leggett (1960) study, only one item of the Srole scale was repeated in obverse form and given at a different place in the interview with the original Srole items. This, of course, did not allow an analysis of the complete set of items.

The technique used here was to instruct the interviewers to alternate questionnaire forms in each successive interview. One form had the original Srole items and the other had the obverse items. Did this produce any systematic sampling biases? Analysis was conducted on the two groups, those who received the Srole-item and those who received the obverse form, for occupation, income, age, education, and sex; these are independent variables known to affect scores on the Srole-items. Upon inspection, the distribution of these variables proved to be almost identical, and statistical analysis showed no differences for the variables between the two groups.<sup>1</sup>

<sup>1</sup> The Srole-items (Srole, 1956:712-713) and the obverse form used in this study are as follows:

1. a. These days a person does not really know whom he can count on.

## THE SROLE SCALE AND ACQUIESCENCE

Table 1 compares the responses of those who received the positive Srole items with those who received the obverse items. If the content of the items, and not the form, had been the determining factor in the choice of responses, the second group would have disagreed with the obverse statements and the scores of the two groups would not have been significantly different. This was not the case.

Among those interviewed by white interviewers, 79% agreed with all or all but one of the Srole items, yet none disagreed with all or all but one of the obverse items. For those respondents interviewed by Negro interviewers, 61% agreed with all or all but one of the Srole items, yet none disagreed with as many as four of the obverse items. Both groups of respondents, then, were very strongly inclined to agree with the particular statements presented to them. Since the meanings of the two sets of items were directly opposed to each other, it is clear that the choice of response was primarily determined by acquiescent agreeing and not by the content of the items.

It will be noted that there was a reduction in agreeing to the obverse form. One way to interpret this is in terms of the meaning of the items. Edwards (1959) has reported that the social desirability of items—as determined by judges' assessment of how desirable it was to have various traits—was correlated with how often respondents ad-

Table 1. Agreement to Srole Items and Disagreement to Their Obverse Forms.

Inter- viewer	Number of Items						N <sup>d</sup>
	0	1	2	3	4	5	
White <sup>a</sup>							
Agree	0%	5%	3%	13%	32%	47%	38
Dis- agree <sup>c</sup>	42	26	11	21	0	0	19
Negro <sup>b</sup>							
Agree	0	2	11	26	26	35	46
Dis- agree	17	27	25	21	10	0	48

<sup>a</sup> $\chi^2=40$ ,  $df=5$ ,  $p<.001$ ,  $\tau_c=-.74$ ,  $p<.001$ .

<sup>b</sup> $\chi^2=38$ ,  $df=5$ ,  $p<.001$ ,  $\tau_c=-.81$ ,  $p<.001$ .

<sup>c</sup>These are obverse items. Relative to content, disagreement with them is interpreted as equivalent to agreement with Srole items.

<sup>d</sup>All respondents are of lower occupation, education, and income.

- b. These days a person can know whom he can count on.
2. a. It's hardly fair to bring children into the world with the way things look for the future.
- b. This is a good time to bring children into the world with the way things look for the future.
3. a. In spite of what some people say, the lot of the average man is getting worse, not better.
- b. In spite of what some people say, the lot of the average man is getting better, not worse.
4. a. Nowadays a person has to live much for today and let tomorrow take care of itself.
- b. Nowadays a person can't just live for today. You have to plan ahead for tomorrow.
5. a. There's little use writing to public officials because often they are not really interested in the lot of the average man.
- b. It is useful to write to public officials because often they are interested in the problems of the average man.

mit to them. Dohrenwend (1966) and Phillips and Clancy (1970) have reported similar findings. The Srole items differ from the items used in these studies in that the Srole items are projective and indirect while the others are direct. Nevertheless, the principle is probably the same; that is, the social desirability of items influences the choice of agreeing or disagreeing to them. In this case, however, the term *social undesirability* should be employed. That is to say, for respondents who want to present the least resistant, most acquiescent appearance, items which are self-deprecatative are consistent with the image desired. In the case of the Srole items, both form (agreeing) and content operate in the same direction. Respondents may "yeasay" to self-deprecating statements. In the case of the obverse form, there is some cross-pressure. Respondents must choose between yeasaying or self-deprecation. Here the choice was predominantly for yeasaying but there was some reduction in it.

This suggests an approach to the study of the interaction of question form and content, a phenomenon observed by Cronbach in

1946. He noted that "the final score of the person on any test is a composite of the effects resulting from the content of the item and effects resulting from the form of the item used" (1946:475). Future research could vary the degree of self-deprecation or social undesirability of items to determine at what point members of the underclass will stop agreeing to the items.<sup>2</sup>

To what extent does acquiescence explain the operation of the Srole scale here? In the study of the F scale, Bass (1956) and Couch and Keniston (1960) interpreted a positively scored scale of items in terms of its correlation with the obverse of the scale. The effect of acquiescence was stated in terms of the amount of variance explained in the positively scored scale relative to the negatively scored scale. This procedure cannot be replicated here since both scales were not administered to the same individuals. However, a rough notion of the effect of acquiescence in terms of the variance explained can be obtained by observing the correlation between the percentages of one group of respondents agreeing at each level to the positively scored items and the percentages of the second group disagreeing at each level to the obverse items.

The product-moment correlation between the positively and negatively scored forms was  $-.79$  with white interviewers and  $-.65$  with Negro interviewers. Squaring these two correlations, it is possible to estimate that acquiescence explains 62% of the variance in the positively scored items administered by white interviewers and 42% of the variance in the positively scored items administered by Negro interviewers. The use of Negro interviewers was not found to be a factor that greatly reduced agreeing. The differences seen in Table 1 between the two races of interviewers were not statistically significant for either the positively or negatively scored forms.

<sup>2</sup> Another variable related to content of items is reliability. Landsberger and Saavedra (1967) found that "randomness" as well as agreeing response-set explained a large part of the variance in the F scale, rather than content. If this had been tested for here, it would have been expected that randomness would have been greater with the obverse items due to the reduction in agreeing and the subsequent increase in the arbitrary selection of responses.

In conclusion, it appears that for respondents of this type who will agree with practically anything, using the obverse form of the Srole items does not really solve the problem of agreeing response-set.<sup>3</sup> The construction of a "balanced" scale which combined positively and negatively scored items would indeed reduce the scores, but this would be an artifact of the ratio of positively and negatively scored items in the scale. One could not conclude that the reduction meant that acquiescence had been balanced out and that the responses had been, therefore, to the content of the items and not the form. In fact, it may well be that when questions of this type are used with respondents of lower class and lower ethnic status, agreeing response-set cannot be eliminated from the responses, and instruments like the Srole items will inevitably measure acquiescence to some degree. It may also be that a number of the correlations that have been obtained between "anomie" and other attitude scales are spurious, to some degree, because of acquiescence.

Finally, it is suggested that acquiescence is not just a psychological phenomenon or just a measurement problem. It is a behavioral fact located in the class and racial structure of this society—a proper subject for sociological inquiry. In the case of the respondents in this study—poor Southern Negroes—acquiescence has proved to be, historically, an effective, rational tactic for survival. As Ellison's (1947:440) "Invisible Man" put it: "Oh, but wouldn't they gag. I didn't know what my grandfather had meant but I was ready to test his advice. I'd over-

<sup>3</sup> Although the Srole items may not measure "anomie" at all with respondents of the type studied here, it, or a similar set of items, could be a useful tool for the sociological study of acquiescence. For example, I have found in preliminary analysis that aggregate Srole item scores for Negro communities are significantly lower where racial activism has been higher—holding class and education constant. To use the items in this way, one should maximize the social distance between respondent and interviewer and use the positively scored form. In this case, scale scores need not be interpreted in terms of the inference of some attitude "projected" through responses to the items. Rather, agreement with the items can be considered as an act of acquiescent behavior, and the respondent scored according to the frequency with which he exhibits such behavior.

come them with yeses, undermine them with grins, I'd agree them to death and destruction . . . I'd yes them till they puked and rolled in it. All they wanted of me was one belch of affirmation and I'd bellow it out loud. Yes! Yes! Yes!" From this perspective, acquiescence can be seen more as a tactic used in social interaction rather than a "deep-rooted personality syndrome.

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# SOME CONSTRAINTS IN ANALYZING DATA ON ORGANIZATIONAL STRUCTURES: A COMMENT ON BLAU'S PAPER \*

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*American Sociological Review* 1971, Vol. 36 (April):294-297

BLAU's recent article, "A Formal Theory of Differentiation in Organizations," *ASR* 35 (April, 1970):201-218, reviews a number of interesting empirical findings, but whether or not they justify theoretical generalizations at a high level of abstraction is a moot question. No one can take exception to efforts to construct generalizations which in turn suggest hypotheses for research. Confirmation of such hypotheses obviously lends credence to the earlier generalizations while disconfirmation or a null finding renders them questionable. Blau presents a number of findings which seem to confirm his general propositions, but I wonder if the findings are so nearly equivalent that the theoretical generalizations Blau claims they support rest on a very narrow empirical base. Specifically, I will show that under certain conditions and given only one of the empirical findings, almost all of the other findings necessarily follow. I will do this, first, by constructing a hypothetical model of organizational structure and doing algebraic manipulations on its properties, and, second, by pointing out some difficulties that arise when one controls for organizational size.

## THE HYPOTHETICAL MODEL

The hypothetical model of organizational structure makes but two simple assumptions. The first is what classical theory calls unity of command: each office (save for the chief executive's) reports to one and only one superordinate office. The second assumption is that of symmetry. Two conditions are necessary for symmetry: each branch of the inverted tree which is the model for

tables of organization must have the same number of levels, and spans of control must be equal at all levels. The first assumption, that of unity of command, Blau accepts in his research as do many administrators when they are asked to design a clean, neat organization chart. The assumption of symmetry, of course, is not built into Blau's research, but it has to some extent been drummed into the heads of people who design organizations either by prescription ("The ideal span of control is between six and eight") or through imitation ("We have thirteen levels of hierarchy—just like the Army") and may therefore be reflected in his data.

The properties of symmetrical hierarchies are easily described algebraically. Let us denote spans of control (which by definition of symmetry do not vary *within* organizations) as  $X$ ; the number of levels in an organization is  $n+1$ . The number of positions,  $S$ , in such organizations is therefore

$$S = \frac{X^{n+1} - 1}{X - 1} \quad (1)$$

Equation (1) is the sum of a geometric series. If  $X$  is fairly large as we shall assume to simplify matters, the equation can be rewritten as

$$S = X^n \quad (2)$$

The size of each branch in a symmetrical hierarchy which we shall call  $T$  is therefore

$$T = \frac{X^n - 1}{X - 1} \approx X^n, \quad (3)$$

\* I am indebted to Neil Henry for his comments on an earlier draft of this paper.

and the proportionate size of each branch,  $P$ , is

$$P = \frac{X^{n-1}}{X^n} = \frac{1}{X}. \quad (4)$$

If all positions above the lowest level are considered administrative, then the number of administrators is also described by equation (3) and the proportion of administrative personnel is shown by (4) above.

Let us now turn to Blau's first generalization, increasing size generates structural differentiation at declining rates (Chart 1). Let us also take proposition (1.4)—the larger the organization the wider the supervisory span of control—as given, noting that this holds for all levels of managers and supervisors (p. 209). In terms of my notation, Blau's data indicate that  $X$  increases with size. Equation (3) above, which corresponds to the size of branches in symmetrical hierarchies, must also increase with size provided that  $X$  is greater than unity and  $n$  does not decrease. (In fact,  $n$  too increases with size.) Blau's proposition (1.2) is thus anticipated by the symmetrical model of organizations. Equation (4) above, which is the proportionate size of each branch as well as the proportionate size of the administrative component,

CHART 1: BLAU'S PROPOSITIONS

1. Increasing size generates structural differentiation in organizations along various dimensions at decelerating rates.
- 1.2 The larger an organization is, the larger the average size of its structural components.
- 1.3 The proportionate size of the average structural component decreases with increases in organizational size.
- 1.4 The larger the organization is, the wider the supervisory span of control.
- 1.5 Organizations exhibit an economy of scale in management (i.e., the relative size of the administrative component decreases)
- 1.6 The economy of scale in administrative overhead itself declines with increasing organizational size.
2. Structural differentiation in organizations enlarges the administrative component.
- 2.1 The large size of an organization indirectly raises the ratio of administrative personnel through the structural differentiation it generates.
- 2.2 The direct effects of large organizational size lowering the administrative ratio exceed its indirect effects raising it owing to the structural differentiation it generates.

decreases with increases in  $X$ ; so Blau's propositions (1.3) and (1.5) are also anticipated. Finally, since the expression on the right side of (4) is the reciprocal of  $X$ , it decreases at decreasing rates as  $X$  increases linearly, anticipating propositions (1.4) and (1.6) in the article.

In symmetrical hierarchies, then, Blau's propositions (1.2), (1.3), (1.4), and (1.5) are equivalent—three of the four immediately follow from any one. To the extent to which real organizations are indeed symmetrical, the first major generalization rests on only one empirical finding—spans of control at all levels increase with size—and not a series of findings which are logically independent. This raises the question of how symmetrical the state and local offices of the Federal Employment Security System are.

#### CONTROLLING FOR SIZE

Blau's second generalization is that structural differentiation enlarges the administrative component, but the phrase "controlling for size" should be added. Proposition (2.1) states that the indirect effect of size is to increase the proportion of administrators through the structural differentiation that size engenders. This is the same as saying that, net of size, differentiation enlarges the administrative component. It will be remembered that the proportion of supervisors in a symmetrical structure,  $P$ , is  $1/X$ . Rearranging the terms in equation (2), we find that

$$\frac{1}{X} = S^{-1/n}. \quad (5)$$

Since size is held constant in Blau's formulation, we need only differentiate the right side of (5) with respect to  $n$  to find how proliferation of hierarchial levels affects the administrative component. The equation

$$\frac{d}{dn} S^{-1/n} = S^{-1/n} \left( \frac{1}{n^2} \right) \log S \quad (6)$$

indicates that, net of size, the administrative component always increases with hierarchial differentiation since the first derivative of  $S^{-1/n}$  is always positive. This much, as Blau observes, is nearly trivial and by itself does not support proposition (2.1). The proposition, Blau claims, is validated by the finding

that "horizontal differentiation into divisions or sections are both positively related to the proportion of supervisors (p. 213).

This finding, of course, is inconsistent with the assumption of symmetry— $1/X$  cannot increase with  $X$  even if size is held constant—and it calls into question the hypothetical model of organizational structure. If, however, the model is modified slightly so that spans of control at the lowest levels of organizations need not equal spans of control elsewhere, Blau's data can be anticipated. In an organization of size  $S$  with spans of control  $X$  at all levels but the lowest, and  $n$  levels within branches, the number of administrators from equation (3) is  $X^{n-1}$ . The proportion of administrators which we shall now call  $P'$  is therefore

$$P' = \frac{X^{n-1}}{S}, \quad (7)$$

which of course equals  $1/X$  when equation (2) holds. We now assume, however, that  $S$  is constant while  $X^{n-1}$  varies. Thus the first derivative of (7) with respect to  $X$  indicates how the administrative component changes with spans of control net of size. The equation

$$\frac{d}{dx} \frac{X^{n-1}}{S} = \frac{n-1}{S} X^{n-2} \quad (8)$$

is positive for  $n \geq 2$ , showing that, net of size, increasing spans of control at higher levels (i.e., the number of divisions and of sections per division) contribute to growth of the administrative component in hierarchies that are symmetrical above the lowest level.

#### THE MAGNITUDE OF INDIRECT EFFECTS

Proposition (2.2)—direct effects of size are greater than the indirect ones—is difficult to evaluate, but an attempt will be made here. Solving equation (2) for  $n$  we find that

$$n = \frac{\log S}{\log X}. \quad (9)$$

Let us now construct a path diagram, Figure 1, which shows the direct effect of size on

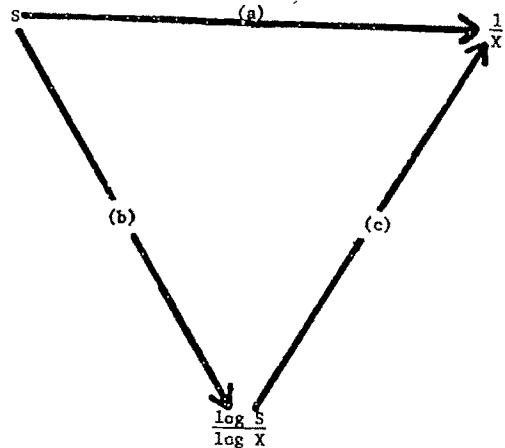


FIGURE 1. DIRECT AND INDIRECT EFFECTS OF SIZE ON THE ADMINISTRATIVE COMPONENT WHERE DIFFERENTIATION IS MEASURED BY NUMBER OF LEVELS.

the administrative component and its indirect effect through hierarchial differentiation which is expressed as the right side of equation (9). Note that in this diagram, the proportionate size of the administrative component is expressed as  $1/X$ .

By itself, Figure 1 does not indicate whether the direct or indirect effects of size are greater in symmetrical hierarchies. Path (c) in the diagram always has a positive coefficient since we know from equation (6) that the administrative component always increases with hierarchial differentiation. The coefficient of path (b) varies; it is the correlation of size and number of hierarchial

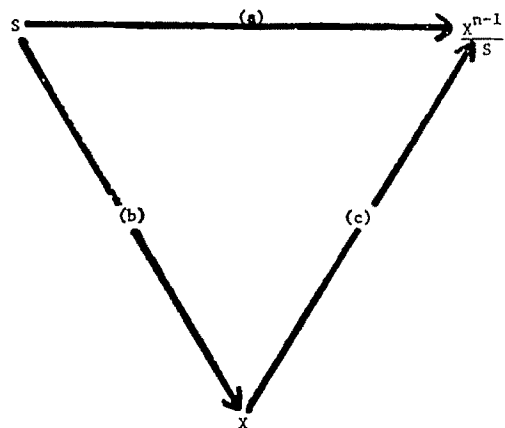


FIGURE 2. DIRECT AND INDIRECT EFFECTS OF SIZE ON THE ADMINISTRATIVE COMPONENT WHERE DIFFERENTIATION IS MEASURED BY SPANS OF CONTROL.

levels. If the logarithms of  $S$  and  $X$  are highly correlated—that is, if their ratio is nearly constant—the number of hierarchial levels is almost invariant, so the coefficient of path (b) is quite small. Indeed, Blau's data indicate that this is the case. From the outset, we have taken, as given, a substantial association between  $S$  and  $X$ ; furthermore, Blau notes that the number of hierarchial levels in headquarters offices varies only between 3 and 7 whereas their size ranges from less than 100 to over 9,000. From these data, it can be predicted that the indirect effects of size on the administrative component, when hierarchial differentiation is the intervening variable, will be much smaller than the direct effects. If, by contrast, there were no association between  $S$  and  $X$ , the number of hierarchial levels would necessarily increase with size and the coefficient of path (b) would be substantial. However, the correlation of  $S$  and  $1/X$  would at the same time diminish to nearly zero; there would be no simple correlation left to decompose!

Figure 2 indicates direct and indirect effects of size on the administrative component when the span of control is the intervening variable. Here, as in equations (7) and (8), we are assuming that symmetry does not hold throughout organizations; spans of control at lowest levels may differ from elsewhere. Path (b) in Figure 2, the correlation of  $S$  and  $X$ , is given as positive and substan-

tial. From the right side of equation (8) we know that when  $S$  is large relative to  $n$ , the association between spans of control and the proportional size of the administrative component net of size, path (c), will be positive but slight. Thus, the indirect effect of size on the administrative component is positive, but probably much smaller than the direct effect. Should the magnitude of  $n$  relative to  $S$  increase, the coefficient of path (c) would grow. But at the same time, the association of size and the administrative component would approach zero, leaving again no simple relationship to be decomposed.

#### DISCUSSION

Values of variables describing organizational structures are often constrained by one another. If the investigator assumes that organizations are hierarchial (as Blau did) and symmetrical or nearly so (as his informants may have), certain correlations among organizational attributes such as size, spans of control, hierarchial levels, and proportionate size of the administrative component are inevitable. No data could make them vanish. The lesson for researchers is plain. Social meaning cannot be attached to statistical relationships among variables that are necessarily interdependent. Rather, such correlations serve only to validate one's initial assumptions about organizations.

## A MATHEMATICAL THEORY OF DIFFERENTIATION IN ORGANIZATIONS \*

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*American Sociological Review* 1971, Vol. 36 (April):297-303

**M**OST theory and research on the structure of organizations is founded on the work of Weber (1946:196-197), who discussed several forms of organizational differentiation, but who did not

specify a relationship between size and differentiation.<sup>1</sup> Three research projects with Weberian foundations have attempted to determine this relationship. Hall reported that he

\* The author would like to thank Loren Cobb (Department of Sociology) and Fred Solomon (Department of Mathematics) for their numerous suggestions. Any errors are the responsibility of the author.

<sup>1</sup> Weber (1946:211) did state: "The increasing bureaucratic organization of all genuine mass parties offers the most striking example of the role of sheer quantity as a leverage for the bureaucratization of a social structure." But the "leverage" relationship is far from precise.

found no relationship between organizational size and bureaucratization (Hall, 1963). The Aston Group reported that size is the best variable for predicting the structuring of activities of an organization (Pugh *et al*, 1969). Finally, Blau (1970) found that size can be used as the main independent variable in a formal theory of differentiation in organizations. His work is the latest development in the progression from Weber's ideal types to formal theory in the study of the structures of organizations.

In this paper, Blau's work is used as the basis for a more efficient and rigorous statement of a theory of differentiation in organizations. In the first section of the paper, Blau's assertions are translated into mathematical axioms, which are then used to derive results that are consistent with his theory. In the second section, Blau's assertions are derived from more general mathematical axioms. These axioms are proposed because they increase the explanatory power of Blau's theory by making it dynamic in time, which is posited to be the ultimate independent variable.

### *Mathematical Statement of Blau's Theory*

Blau's propositional numbering system is used here with the modification that  $V_{n_1 \cdot n_2}$  indicates a verbal proposition and  $M_{n_1 \cdot n_2}$  indicates its mathematical translation. A third heading,  $A_{n_1 \cdot n_2}$ , is used to indicate the mathematical argument demonstrating  $M_{n_1 \cdot n_2}$ .

In this section, Blau's propositions 1.0 and 2.0 are treated as axioms. In some cases, his propositions cannot be unconditionally demonstrated, and are therefore stated with additional conditions. Also, an additional result is obtained from the analysis which Blau did not, and probably could not, derive by means of his verbal approach.

V1.0 "Increasing size generates structural differentiation in organizations along various dimensions at decelerating rates."

Let  $S$  = size of organization in number of members.

Let  $D$  = differentiation, "... any criterion on the basis of which the members of an organiza-

tion are formally divided into positions" (Blau, 1970:203).

M1.0

$D = f(S)$  (See footnote 2)

$$a. \frac{df(S)}{dS} = \frac{df}{dS} > 0.$$

$$b. \frac{d^2f(S)}{dS^2} = \frac{d^2f}{dS^2} < 0.$$

V1.1 "As the size of organizations increases, its marginal influence on differentiation decreases."

This proposition is redundant with the second part of V1.0, and its mathematical formulation is identical with M1.0.b.

V1.2 "The larger an organization is, the larger the average size of its structural components."

Let  $C$  = average size of a component of the organization.

$$\text{Then } C = \frac{S}{D} = \frac{S}{f}.$$

$$M1.2 \quad \frac{dC}{dS} > 0.$$

A1.2

$$\frac{dC}{dS} = \frac{f - S \frac{df}{dS}}{f^2}$$

To evaluate the sign of this derivative, we will first discuss the concept of elasticity. In economics, elasticity of demand is defined as "... the absolute value of the rate of percentage change of output divided by the rate of percentage change of price" (Henderson, Quandt, 1958:168). Or stated mathematically,

Let  $q$  = quantity,

$p$  = price,

and  $q = f(p)$ .

Then,

$$e_p(q) = \left| \frac{p}{q} \frac{dq}{dp} \right|.$$

<sup>2</sup> Strictly speaking  $S$  is an index set comparable to time and  $D = \{ < s, d > \}$ , a set of ordered pairs. The convention  $D = f(S)$  is used because it is simpler.

The concept of elasticity is useful in economics because it summarizes many conditions about demand curves into a single number. Specifically, knowing whether elasticity is less than 1, equal to 1, or greater than 1, is sufficient to determine whether a monopoly's marginal revenue is negative, zero, or positive.

In organizational analysis the concept of elasticity can be used to summarize discussions of the marginal influence of size on differentiation or the economy of scale of some organizational component. This should become evident later in the paper.<sup>3</sup>

Returning to A1.2, note that if

$$\frac{dC}{dS} > 0,$$

$$f - S \frac{df}{dS} \text{ must be greater than } 0,$$

or

$$\frac{df}{dS} \frac{S}{f} < 1.$$

In other words, the condition for the derivative  $\frac{dC}{dS}$  to be positive is that the elasticity of differentiation with respect to size be less than 1.

A sufficient condition for  $e_s(D) < 1$  is  $\frac{d^2f}{dS^2} < 0$  (see footnote 4). Since this condition

is met by M1.0 b, we know that  $\frac{dC}{dS} > 0$ . The sufficiency of the argument may not be obvious. Consider writing the elasticity condition as  $\frac{df}{dS} < \frac{f}{S}$ . For any point on a curve of  $f(S)$ , the derivative is the slope at that point. The

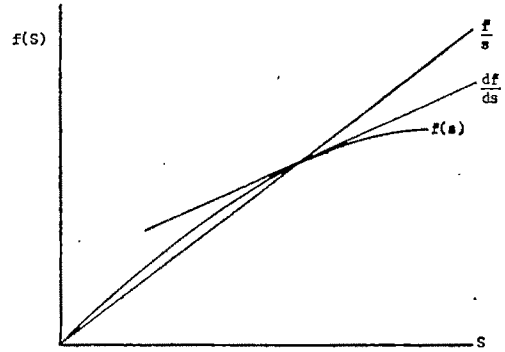


FIGURE 1. GRAPH OF  $f(S)$  VERSUS  $S$

ratio of  $f$  to  $S$  is the slope of a line through the origin and the point. Thus in Figure 1 we can see that for any point on  $f(S)$  the tangent slope is less than the slope of the line through the origin.

V1.3 "The proportionate size of the average structural component, as distinguished from its absolute size, decreases with increasing organizational size."

Let  $P$  = proportionate size of the average component.

Then

$$P = \frac{C}{S} = \frac{1}{D}.$$

$$\text{M1.3} \quad \frac{dP}{dS} < 0,$$

$$\text{A1.3} \quad \frac{dP}{dS} = -\frac{\frac{df}{dS}}{f^2}$$

The derivative is strictly negative by M1.0.a, demonstrating M1.3.

V1.4 "The larger the organization is, the wider the span of control."

Let  $\text{SPAN}$  = span of control.

Then  $\text{SPAN} = kC$ , where  $k$  is a positive constant.

This formulation was chosen because Blau argues that span of control can be thought of as a subset of the members of a component, namely, the subset of subordinates in a component.

$$\text{M1.4} \quad \frac{d\text{SPAN}}{dS} > 0.$$

$$\text{A1.4} \quad \frac{d\text{SPAN}}{dS} = k \frac{dC}{dS}.$$

By M1.2, this derivative is positive.

<sup>3</sup> Neil Henry, Department of Sociology, Cornell, has suggested two interesting applications of the concept of elasticity. In the case of linear regres-

sion,  $D = B_1S + B_2O$ ,  $\frac{\partial D}{\partial S} = B_1$ ,  $\frac{\partial D}{\partial O} = B_2 \Rightarrow 1 = e_s(D)$

+  $e_o(D)$ ; in the case of the multiplicative model,  $\log D = B_1 \log S + B_2 \log O$ ,  $B_1 = e_s(D)$  and  $B_2 = e_o(D)$ . This can be seen by taking partials with respect to  $S$  and  $O$ .

$\frac{\partial \log D}{\partial S} = \frac{1}{D} \frac{\partial D}{\partial S}$ ,  $\frac{\partial B_1 \log S}{\partial S} = \frac{B_1}{S} \Rightarrow B_1 = \frac{S \partial D}{D \partial S}$ .

<sup>4</sup>  $\frac{d^2f}{dS^2} < 0 \Rightarrow e_s(D) < 1$ , but the converse is not necessarily true.

V1.5 "Organizations exhibit economy of scale in management."

This proposition is mathematically equivalent to M1.3 if it is agreed that management is a component of the organization and that its proportionate size decreases with increasing organizational size.

V1.6 "The economy of scale in administrative overhead itself declines with increasing organizational size."

Since the first derivative is negative, it follows that

$$M1.6 \quad \frac{d^2P}{dS^2} > 0.$$

$$A1.6 \quad \frac{d^2P}{dS^2} = -\frac{d^2f}{f^2} + 2\left(\frac{df}{dS}\right)^2$$

Noting that  $\frac{d^2f}{dS^2}$  is negative, the derivative is strictly positive.

An additional result can be obtained from these mathematical propositions that would be extremely difficult to deduce from the verbal ones. In practical terms it states that with increasing organizational size, the rate of increase in the size of the average component itself increases.

$$M1.7 \quad \frac{d^2C}{dS^2} > 0, \quad \text{if } \frac{d^2f}{dS^2} < -\frac{f}{2S^2}.$$

$$A1.7 \quad \frac{d^2C}{dS^2} = -\frac{S}{f^2} \frac{d^2f}{dS^2} - \frac{2}{f^2} \frac{df}{dS} + \frac{2S}{f^3} \left(\frac{df}{dS}\right)^2$$

Multiplying this equation by  $Sf$ ,

$$Sf \frac{d^2C}{dS^2} = -\frac{S^2}{f} \frac{d^2f}{dS^2} - 2 \frac{S}{f} \frac{df}{dS} + 2 \frac{S^2}{f^2} \left(\frac{df}{dS}\right)^2$$

Substituting

$$e_s(D) = \frac{S}{f} \frac{df}{dS} \text{ and } q = -\frac{S^2}{f} \frac{d^2f}{dS^2},$$

$$Sf \frac{d^2C}{dS^2} = 2e_s(D)^2 - 2e_s(D) + q.$$

$$\text{For } \frac{d^2C}{dS^2} > 0,$$

$$2e_s(D)^2 - 2e_s(D) + q > 0.$$

Multiplying this inequality by  $-1$  and rearranging terms,

$$2e_s(D)(1 - e_s(D)) < q,$$

$$\text{or } w(e_s(D)) < q.$$

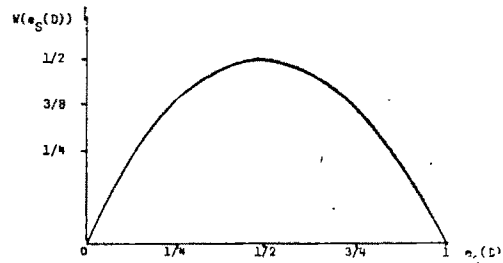


FIGURE 2. GRAPH OF  $W(e_s(D))$  VERSUS  $e_s(D)$

Figure 2 is a graph of  $w$  and indicates that its maximum value is  $1/2$ . Thus if  $q > 1/2$ , or equivalently  $\frac{d^2f}{dS^2} < -\frac{f}{2S^2}$ ,  $\frac{d^2C}{dS^2}$  is

strictly positive. The most negative value  $\frac{d^2C}{dS^2}$  could have occurred when  $q \rightarrow 0$  and  $e_s(D) = 1/2$ . Thus,

$$\frac{d^2C}{dS^2} > \frac{1}{2Sf}.$$

Blau's second axiom is more difficult to understand than his first, and therefore the mathematical translations are less clear. As an aid to understanding, the verbal axiom and propositions are presented together, followed by their mathematical forms.

V2.0 "Structural differentiation in organizations enlarges the administrative component."

V2.1 The large size of an organization indirectly raises the ratio of administrative personnel through the structural differentiation it generates."

V2.2 "The direct effects of large organizational size lowering the administrative ratio exceed its indirect effects raising it owing to the structural differentiation."

Blau is saying that the administrative component is different from the average structural component developed in the first set of propositions. In the discussion of V2.0, Blau states that with size held constant, changes in differentiation cause changes in the administrative component. In the first set of propositions, differentiation is only a function of size, implying that if size is held constant, differentiation is also constant. Thus the administrative

component changes with changes in differentiation and size is held constant, differentiation must be a function of size *and other variables*. Thus, for the first set of propositions to be correct, all derivatives must be changed to partial derivatives,  $D = f(S, \text{Other Variables})$ , and V1.0 must include a *ceteris paribus* condition.

The diagram of proposition V2.2 in Figure 3 describes the situation in V2.0, V2.1, and V2.2. This diagram shows that the administrative component is a function of both size and differentiation. Thus the mathematical statement of V2.0 is:

Let  $A$  = administrative component, a subset of  $S$ .

$$\text{M2.0} \quad A = g(D, S),$$

$$\frac{\partial g}{\partial D} > 0.$$

Let  $\frac{A}{S}$  = administrative ratio.

$$\frac{A}{S} = \frac{1}{S} g(D, S) = h(D, S)$$

$$\text{M2.1} \quad \frac{\partial h}{\partial D} > 0.$$

$$\text{A2.1} \quad \frac{\partial h}{\partial D} = \frac{1}{S} \frac{\partial g}{\partial D}.$$

M2.1 describes relationship  $c$  in Figure 3.

V2.2 is concerned with the total change in the administrative ratio due to changes in size and differentiation. Thus the appropriate mathematical form is the total derivative,  $dh$ .

$$\text{M2.2} \quad dh < 0.$$

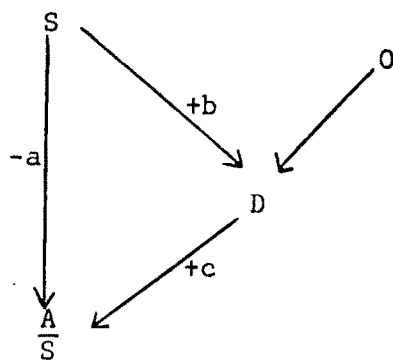


FIGURE 3. DIAGRAM OF BLAU'S 2.2.

$$\text{A2.2} \quad dh = \frac{\partial h}{\partial D} dD + \frac{\partial h}{\partial S} dS.$$

$$\text{If } \frac{\partial h}{\partial D} dD < -\frac{\partial h}{\partial S} dS,$$

then M2.2 holds.

It must be remembered that differentiation is a function of both size and other terms. Therefore,

$$dD = \frac{\partial f}{\partial S} dS + \frac{\partial f}{\partial O} dO,$$

$$\text{and } dh = \frac{\partial h}{\partial D} \left[ \frac{\partial f}{\partial S} dS + \frac{\partial f}{\partial O} dO \right] + \frac{\partial h}{\partial S} dS.$$

Since Blau does not include information about the magnitude of the change in the administrative ratio with respect to size,  $\frac{\partial h}{\partial S}$ , M2.2 cannot be derived unconditionally.

The total derivative form of  $dh$  suggests the possibility of a parametric statement of M2.2. If differentiation and size are functions of time, we get the interesting relationships

$$\frac{dh}{dt} = \frac{\partial h}{\partial D} \frac{dD}{dt} + \frac{\partial h}{\partial S} \frac{dS}{dt},$$

$$\frac{\partial h}{\partial D} \frac{dD}{dt} + \frac{\partial h}{\partial S} \frac{dS}{dt} < 0.$$

This suggests a dynamic theory. This aspect of the mathematical theory of differentiation in organizations is discussed in the next section of the paper.

Blau's last proposition V2.3 "... is not as well knit into the system as the others and should be regarded as mere conjecture" (Blau, 1970:214). This proposition is as follows:

"The differentiation of large organizations into subunits stems the decline in the economy of scale in management with increasing organizational size."

Blau seems to be saying that if an organization is divided into subunits, the subunits can be treated as whole organizations and one is operating from a regressed point on



the graph in Figure 4. In other words, the decline in the economy of scale at point A is not as great as that at B.

#### *A Dynamic Mathematical Theory of Differentiation in Organizations*

Blau's theory was developed from cross-sectional data and, therefore, equivalent to the economist's quasi-dynamic system of demand analysis. At the end of the preceding section, the possibility of stating D and S as functions of time was suggested. This section develops such a set of mathematical axioms from which Blau's axiom V1.0 can be derived. These axioms then translate Blau's work into a dynamic theory of organizational growth and development. It is also assumed that other variables are held constant.

Let V = the volume of work done by the organization.

Let T = the number of different tasks employed to do the work of the organization.

Let t = time.

$$V = R(t).$$

$$T = Q(t).$$

$$D = F(V, T).$$

$$S = G(V, T).$$

V represents the demand for the organization's "product" over time. T represents a set of decisions about the technology used by the organization over time. It seems reasonable that D is a function of volume because of the need for increased coordination and administration with rising volume.

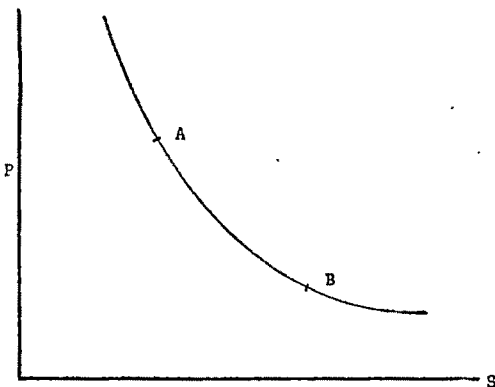


FIGURE 4. GRAPH OF P VERSUS S

Also, D must be a function of the number of different tasks. Similar reasons can be given for S being posited as a joint function of V and T.

The following mathematical development specifies the conditions that are sufficient, but not necessary, to derive Blau's V1.0. Let

$$S = G[R(t), Q(t)] \\ = H(t).$$

$$t = H^{-1}(S) \text{ (if } \frac{dS}{dt} \neq 0.)$$

$$D = F[R(H^{-1}(S)), Q(H^{-1}(S))], \\ D = f(S).$$

The condition on the invertibility of H simply states that if t is to be a function only of S, then S must be a strictly monotonic function of time.

$$\frac{dD}{dS} = \frac{\partial D}{\partial V} \frac{dV}{dt} \frac{dt}{dS} + \frac{\partial D}{\partial T} \frac{dT}{dt} \frac{dt}{dS}.$$

$$\frac{dD}{dS} > 0 \text{ if,}$$

$$\frac{\partial D}{\partial V}, \frac{\partial D}{\partial T}, \frac{dV}{dt} \frac{dt}{dS}, \frac{dT}{dt} \frac{dt}{dS} > 0.$$

These conditions state that differentiation increases with increases in both volume and the number of tasks. Also, volume must increase with time *and* time with size, or volume must decrease with time *and* time with size. The same conditions must hold for the number of tasks. Many other combinations of conditions could also satisfy  $dD/dS > 0$ . To examine the conditions under which M1.0.b, that  $\frac{d^2f}{dS^2} < 0$ , we have

$$\frac{d^2D}{dS^2} = \frac{dV}{dt} \frac{dt}{dS} \left[ \frac{\partial^2 D}{\partial V^2} \frac{dV}{dt} \frac{dt}{dS} + \frac{\partial^2 D}{\partial V \partial T} \frac{dT}{dt} \frac{dt}{dS} \right] \\ + \frac{\partial D}{\partial V} \frac{d^2V}{dt^2} \left( \frac{dt}{dS} \right)^2 + \frac{\partial D}{\partial V} \frac{dV}{dt} \frac{d^2t}{dS^2} \\ + \frac{dT}{dt} \frac{dt}{dS} \left[ \frac{\partial^2 D}{\partial T^2} \frac{dT}{dt} \frac{dt}{dS} + \frac{\partial^2 D}{\partial T \partial V} \frac{dV}{dt} \frac{dt}{dS} \right] \\ + \frac{\partial D}{\partial T} \frac{d^2T}{dt^2} \left( \frac{dt}{dS} \right)^2 + \frac{\partial D}{\partial T} \frac{dT}{dt} \frac{d^2t}{dS^2}.$$

Thus,

$$\frac{d^2D}{dS^2} < 0 \text{ if}$$

$$\frac{\partial^2 D}{\partial V^2}, \frac{\partial^2 D}{\partial T^2}, \frac{\partial^2 D}{\partial V \partial T}, \frac{d^2 V}{dt^2}, \frac{d^2 T}{dt^2}, \frac{d^2 t}{ds^2} < 0.$$

This set of conditions states that the *rate* of increase of all the variables in the first set of conditions is decelerating. Also implicit in this set of conditions is the existence of the second derivatives, implying that differentiation is more complex than a linear function of size.

The total set of conditions is quite imposing, but it must be remembered that the theory is highly abstracted from empirical reality. In using the theory, the researcher can relax some of the constraints, particularly where the theory specifies that some terms are approximately zero. Of particular importance to the researcher is the possibility of generating new hypotheses from the mathematical formulations. Finally, the theory is useful for suggesting the nature of relationships between empirical variables. For example, the nature of the conditions suggest that  $D$  could be a logarithmic function of  $S$ . The same function may represent volume and number of tasks with time. Thus, the theory limits the search for possible relationships between empirical variables. A similar procedure to that suggested in the discussion of V2.2 can be used to derive Blau's second axiom.

### *Summary and Conclusion*

Blau's verbal propositions have been formulated more efficiently by the use of mathematical functions and conditions on their derivatives. In some cases, Blau's propositions were found to hold unconditionally, while in others additional conditions were seen to be necessary. Blau's second axiom was found to be insufficient to derive the theorem that the direct effect of size in decreasing the administrative ratio is greater than the indirect effect in increasing that

ratio. Additional knowledge is needed about how the administrative component is related to size. A further result, that the size of the average component increases at an increasing rate with increasing size, was derived. Because of the complexity of this result, it is improbable that it could have been deduced verbally.

Finally, axioms were proposed that translate Blau's quasi-dynamic theory into one that is dynamic. Differentiation and size can be thought of as functions of the volume of work done by an organization and the number of tasks used to accomplish the work. Volume and the number of tasks were in turn proposed as functions of time. A set of conditions on these functions was offered that permit Blau's axiom 1.0 to be derived as functions of volume and the number of tasks, and ultimately of time. It is hoped that these conditions provide adequate opportunity for testing the theory.

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# COMMENTS ON TWO MATHEMATICAL FORMULATIONS OF THE THEORY OF DIFFERENTIATION IN ORGANIZATIONS \*

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*American Sociological Review* 1971, Vol. 36 (April):304-307

THE aim in developing a deductive theory is to discover a few general principles that can account for many empirical relationships, some of which have already been observed and others are predicted to be observable in the future. Attempts to construct such a deductive theory inductively, starting with research findings to be explained, involve at least two steps. First, general concepts are introduced to subsume several empirical variables under a single concept. Thus, I used the concept of differentiation in organizations to encompass the division of labor, the number of hierarchical levels, the number of functional divisions, and some other variables (Blau, 1970). Many consistent findings are reduced to much fewer propositions by substituting general concepts. Second, the remaining propositions are organized into a deductive system by inferring some basic propositions from which all the others can be logically deduced. The two basic generalizations I inferred are (1) that the increasing size of organizations promotes structural differentiation at declining rates and (2) that differentiation enlarges the administrative component. The assumption is that all other propositions under consideration are logically implied by these two and that future, as well as all existing, empirical findings are consistent with the predictions implied by the theory. Further steps in refining the theory may entail explicating generalizations through new unmeasured concepts—for example, I used the concept of need for coordination to explain the influence of differentiation on the administrative component—and reformulating the propositions in mathematical terms.

*Hummon's Paper.* An important advantage of mathematical formulations of a the-

ory is that they make it possible to test its logical structure by showing whether the lower-order propositions can indeed be derived from the definitions and the higher-order ones without further assumptions. Hummon's analysis illustrates this. For instance, his discussion of elasticity under A1.2 proves mathematically that my proposition 1.2—the average size of structural components increases as the size of the entire organization does—follows strictly from the second part of the first generalization—that the positive influence of size on differentiation occurs at a *declining rate*. He thereby confirms my “geometrical” argument that the shape of the regression curves of size on various aspects of differentiation implies proposition 1.2 (Blau, 1970:207-208). In other instances, however, the mathematical translation reveals shortcomings of my verbal formulations.

In my generalization that increasing size promotes differentiation, I implicitly assumed that differentiation depends not only on size but also on other factors. But assumptions must not be left implicit in formal theorizing lest misleading implications be drawn. In his discussion of V2.0, Hummon notes the need to make explicit the assumption that differentiation does not depend on size alone. He discovers a more important failure of mine to be fully explicit in his analysis of A2.2, in which he concludes that my proposition 2.2—the direct effect of size reducing the administrative ratio exceeds its indirect effect raising it—“cannot be derived unconditionally.” The question that I failed to answer is whether my proposition 1.5—that there is an economy of scale in administration—is meant to imply that the *gross* effect of size reduces the administrative component or that only its *net* effect, controlling other conditions, produces such a reduction. If one assumes that size has a negative *gross* effect on the administrative component,

\* I am indebted to William H. Weber and Roland Wulbert for helping me interpret the mathematical formulations in the two preceding papers by Norman P. Hummon and Marshall W. Meyer.

proposition 2.1—that size has some indirect positive effects on this component—does imply that its negative effects must outweigh these indirect positive effects (otherwise, the gross effect could not be negative), as I claimed in proposition 2.2. But if one limits oneself to the more conservative hypothesis that size has a negative *net* effect on the administrative component, which may be counteracted by other conditions, proposition 2.2 does not follow from the premises, as Hummon states.

Mathematical operations are used by Hummon also to derive from my first axiom a new proposition (M1.7): “With increasing organizational size, the rate of increase in the size of the average component itself increases.” Here he is wrong, however. Although my knowledge of calculus is not adequate to indicate the error in his mathematical argument (A1.7), I shall use a simple arithmetic example to show that the axiom implies the very opposite of his new proposition. The issue is whether the increase in the average size of components with increasing organizational size occurs at an *increasing rate*, as Hummon claims, or not, as I do.

To illustrate the argument, four organizations are examined, whose size is 100, 200, 400, and 800 employees, respectively. Now let us look at two limiting extremes between which all empirical cases conforming to the axiom must fall. If increasing organizational size is accompanied by increases in the number of components at decreasing rates (axiom), these increases in components must occur at a rate *less* than the increases in organizational size (the upper limit) and *more* than zero (the lower limit). An example of the upper limit—that the number of components increases at the same rate as size does—is that the four organizations have 5, 10, 20, and 40 components, respectively. This would mean that the average size of the components would be 20 in all four organizations ( $100/5$ ,  $200/10$ ,  $400/20$ ,  $800/40$ ). But if the number of components (the denominator) increases with size *less* than this upper limit, as postulated, their average size does not remain constant but increases with organizational size.

An example of the lower limit—that the increase in the number of components with organizational size is zero—is that the four

organizations have 10 components each. This would mean that the average size of components is 10, 20, 40, and 80 in the four organizations ( $100/10$ ,  $200/10$ ,  $400/10$ ,  $800/10$ ), a rate of increase identical with that for the entire organizations. But if the number of components (the denominator) increases with organizations' size *more* than this lower limit, as postulated, their average size must increase at a lower rate than the size of the entire organizations. Only if increasing organizational size is assumed to *decrease* the number of components (which, though an empirical possibility, contradicts the axiom) would it increase their average size at *increasing* rates. In sum, the extremes between which all cases must fall, according to the axiom, indicate that increases in organizational size are accompanied by increases in the average size of components at a rate greater than zero but less than constant, that is, by increases at a decreasing rate, and not at an increasing rate, as Hummon assumes.

In the last part of his paper, Hummon develops a dynamic theory designed to explain my first generalization by postulating some axioms that make size a function of the demand for the organization's services and technology. But does taking the total differential of size with respect to time really create a dynamic theory? The time derivatives of change in demand and change in technology Hummon postulates are themselves in need of explication. Conceptual analysis and clarification must precede the construction of mathematical models (as exemplified for dynamic economic models in Adelman, 1961). The question we must ask is what new concepts can help us explain, for example, why the influence of size on differentiation declines with size, or why there is an economy of scale in administration. Conceptual insights are necessary to answer these questions, which must be followed up by careful analysis of the connections between the new concepts and the others in the theory. Constructing mathematical models cannot substitute for these conceptual explanations but only complement them after they have been advanced.

*Meyer's Paper.* Meyer presents an intriguing reformulation of the theory of organizational differentiation, putting it on its

head—he would undoubtedly say, putting it right side up—by using one of the derived propositions as the fundamental axiom. But his discussion is vitiated by the misleading assumption on which it rests.

He starts with a model of a perfectly symmetrical organization, with the same number of levels in all branches and the same span of control of all managers and supervisors, regardless of rank in the hierarchy and duties of subordinates. The fact that this symmetrical model is capable of generating several of my propositions apparently leads him to assume that the model is probably valid. But a false theoretical model can yield correct predictions, and our data on government agencies demonstrate the symmetrical model to be false. The number of levels in various parts of most organizations differs widely, and so does the supervisory span of control, by rank in the hierarchy and by duties of subordinates, as Meyer knows quite well, because he analyzed data showing this in his dissertation. My theoretical formulation permits these variations and is applicable whether or not particular organizations are symmetrical. To be sure, one might use a model based on assumptions known to be false in order to analyze departures from this model (for example, from the model of perfect mobility), but this is not Meyer's aim.

In his section "Controlling for Size," Meyer is forced to modify the assumption of perfect symmetry, but he does so by a mere trifle. Although the span of control of first-line supervisors is now permitted to differ from that of higher managers, the assumption is still that there is no variation in span of control among first-line supervisors and among other managers, and neither is there any variation in levels among various parts of the organization. In any case, I fail to see the presumed equivalence between my proposition concerning the influence of differentiation on the administrative component and the conclusion of this section that "increasing spans of control contribute to growth of the administrative component in hierarchies that are symmetrical above the lowest level."

The fundamental question Meyer raises concerns the tautological nature of my theory. In the first section of his paper, he

shows that, given his assumption about symmetry, a number of propositions in my theory are not logically independent. He criticizes me for using logically interdependent propositions as part of the theory and for accepting empirical evidence in support of these propositions. But this criticism rests on misconceptions about the very character of deductive theories and about the inductive procedures usually employed to build these theories.

A deductive theory consists of logically interdependent propositions, that is, of a few basic propositions or axioms from which all others logically follow. My aim was to develop a deductive system of propositions that logically imply many of the empirical findings from a study of government bureaus. Hence, I built the theory inductively, first introducing general concepts to subsume seemingly disparate findings under a single proposition, and then discovering among the remaining propositions the two from which all others are logically deducible. In short, the empirical findings supporting the various propositions, far from not being needed, are the foundation that shaped the deductive theory and on which it rests. I was able to infer this theoretical system of interdependent propositions from the empirical data without making Meyer's unwarranted symmetry assumption.

The problem of tautology is inherent in deductive theorizing. If lower-order propositions logically follow from higher-order ones, the former are contained in the latter and therefore tautological. Why would one want empirical evidence for more than one of the propositions in a deductive theory? One reason is that the logical connections are often not completely tight, and another is that theoretical propositions are never definitely validated. Although empirical evidence supporting one proposition indirectly corroborates the others logically connected with it, we can never rest content that the empirical evidence suffices to establish the entire theoretical system, particularly not in the social sciences. A final question the tautological nature of deductive theorizing raises is whether deducing lower-order from higher-order propositions can ever produce new ideas. Even though the lower-order propositions must have been implicit in the

higher-order ones, complex derivations of lower-order propositions often produce entirely unsuspected conclusions, as marginal analysis in economics shows. While the most profound and original theoretical insights are the result of new conceptualizations, in my opinion, deductive theorizing also can produce important insights.

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## NOTES AND COMMENTS

### COMMENT ON DAVIS' GRAPH MODELS

Davis (*ASR*, October, 1970:843-851) has criticized his and Leinhardt's (forthcoming) earlier model of sociogram structure in small groups, and then presented a new model with the advantages that it has "fewer embarrassments when tested against the data bank, and . . . is simpler" (1970:846). The following model appears to be simpler and more accurate than Davis' newer model.

The reader may find it helpful to consult Davis' Chart 1 (1970:844), particularly when we discuss the fact that some of the triad types (e.g. 1-2-0-A) have two configurations. Note, however, that the diagram for the 2-1-0 triad has a typographical error, and one of the configurations for the 1-1-1 triad type is missing (the asymmetric pair running into the mutual positive pair).

We start with a simple proposition: *Friends are likely to agree, and unlikely to disagree*; *close friends are very likely to agree, and very unlikely to disagree*. We will also assume that a mutually positive pair relation indicates "close friends," and an asymmetric relation indicates "friends." (A mutually nonpositive relation would mean that members of the pair are not friends at all.) We may now calculate which of Davis' 13 possible triad types are more or less likely to occur in an empirical sociogram.

In order to operationalize this process, we will calculate a "score of likelihood" for each type of triad. If a given triad type has a characteristic which is "very likely," we give it a score of 1; if it has a characteristic which is "likely," we score it  $\frac{1}{2}$ ; a characteristic that is "unlikely" gets a score of  $-\frac{1}{2}$ ; and a "very unlikely" characteristic gets a score of -1. If a triad type has more than one relevant characteristic, then its score is the sum of the scores for each separate characteristic. The result of this procedure is an ordinal ranking of triad types, those with higher scores being more likely to occur in the sociogram than those with lower scores. The calculation is illustrated with a few examples.

The type 3-0-0 triad contains three pairs of close friends. Considering these pairs one at a time, we see that each pair agrees in its appraisal of the third person: they both like him. Therefore each pair gets a score of 1, being pairs of close friends who agree (which is very likely). Since there are three such pairs, the

3-0-0 triad type gets a total likelihood score of 3.

The type 2-1-0 triad contains two pairs of close friends and one pair of friends. One of the pairs of close friends agrees on the third person (they both like him); so they receive a score of 1. The other pair of close friends *disagrees* on the third person (one likes him and the other does not), and since this is "very unlikely," that pair receives a score of -1. Members in the third pair are only friends, and they agree on the third person (both like him); so they receive a score of  $\frac{1}{2}$ . The total likelihood score for the type 2-1-0 triad is then:  $1 - 1 + \frac{1}{2} = \frac{1}{2}$ .

The calculation of likelihood scores is quite straightforward for most of the 13 types of triads. However, some of the triad types with two configurations (1-2-0-A, 1-1-1, 0-2-1-A) are more complex since the scores are different for each configuration. In these cases, the score of the triad type will simply be the average of the scores of the two configurations in that type. Thus for triad type 1-2-0-A, one configuration had a score of 0, and the other, a score of 2; so the average score of 1 is assigned to triad type 1-2-0-A.

The 13 triad types are ranked on the basis of their likelihood scores in the first column of the chart below, beginning with the one

Comparison of Theoretical and Empirical Rank Orders

Triad Type	Likelihood Score	Triad Type	Percent of Matrices With Triad Frequency Less Than Chance Expectation *
2-0-1	(-2)	2-0-1	(90)
0-3-0-B	(-3/2)	0-3-0-B	(90)
1-2-0-B	(-1)	0-2-1-B	(83)
1-1-1	(-1/2)	1-1-1	(78)
0-2-1-B	(0)	1-2-0-B	(75)
0-0-3	(0)	0-1-2	(42)
0-2-1-A	(0)	0-0-3	(41)
0-1-2	(+1/2)	2-1-0	(37)
2-1-0	(+1/2)	1-0-2	(37)
0-3-0-A	(+1/2)	0-2-1-A	(27)
1-0-2	(+1)	0-3-0-A	(16)
1-2-0-A	(+1)	1-2-0-A	(7)
3-0-0	(+3)	3-0-0	(1)

\* Davis, 1970:845.

least likely to occur in a sociogram. (Note that there are a number of ties in the ranking.) The second column is taken from Davis' Table 1 (1970:845) and is based on data from 742 sociomatrices. The percentage figures show the proportion of sociomatrices in which the triad type occurred less than would be expected by chance. Therefore, the higher the percentage, the lower than chance occurrence of that triad type in an empirical sociogram. By comparing Columns 1 and 2, we see that the rank ordering derived from the present model is quite close to the empirical ordering, particularly considering the degree of irregularity in the data (Table 1 in 1970:845).

Notice that the present model subsumes the successes of the Davis-Leinhardt model (i.e. that triad types 2-0-1, 0-3-0-B, 1-2-0-B, 1-1-1, and 0-2-1-B are critical), and eliminates its failures (i.e. that 0-1-2 and 2-1-0 are also supposed to be critical). It also subsumes the major conclusions of the newer Davis model (i.e. that 2-0-1's are very unlikely; and that 0-3-0-B's are unlikely relative to 0-3-0-A's—transitivity). It is also fairly successful in rank ordering the 13 types of triads according to their percentage of less-than-chance occurrences—a feature which is not included in either of the other models. Additionally, it appears that the present model is simpler than the others.

If the simplest model provides the best fit to the data, we may question the assumptions of the more complex models. In that vein, we emphasize Davis' point (1970:849) that there is no support here for the theory of structural balance. Neither, however, is there support for the general hypothesis of "clusterability" since "agreement among friends" is an adequate assumption to construct an appropriate model.

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#### COMMENTS ON PROFESSOR MAZUR'S HYPOTHESIS ABOUT INTERPERSONAL SENTIMENTS

Since the writing of the article cited by Mazur (Davis, 1970), we have been working together to improve our structural models. Our collective ruminations had independently led us to an approach quite similar to his but different enough to warrant this note.

We now believe that the central proposition in structural sociometry is this: *Interpersonal choices tend to be transitive—if P chooses O and O chooses X, then P is likely to choose X.*

This is not exactly a fresh concept mathematically, but we argue: (1) it is even simpler than Mazur's principle; (2) it predicts the results in the article better than Mazur's; and (3) it has definite theoretical fertility.

To compare the two hypotheses, transitivity and agreement between friends, it is necessary to distinguish between *triads* and *triplets*. Using conventional sociometric notation, relationships among P, O, and X may be viewed in two ways, as a triad of three pair-relations (PO, OX, PX) or as six ordered triplets (POX = P to O, O to X, P to X; PXO = P to X, X to O, P to O; POX = . . . . .). With positive and negative "choices" there are eight possibilities for a triplet, +++, ++-, +-+, . . . . .)

A +++ triplet is *transitive* (P chooses O, O chooses X, P chooses X); a ++- triplet is *intransitive*; and all other triplets are *vacuously transitive* (Holland and Leinhardt, 1970). Note that the existence of vacuous triplets means transitivity and intransitivity are not mutually exclusive.

The transitivity hypothesis may now be stated this way: transitive (+++) triplets tend to be common; intransitive triplets (++-) tend to be rare.

Mazur's hypothesis includes transitivity but goes beyond it. (See Editor's note.) Agreement between friends also implies that if P likes O and O *dislikes* X, then P will dislike X. In triplet terms this adds a second claim: +-+ triplets will be common, ++- triplets will be rare.

Thus, the transitivity hypothesis is literally simpler than the hypothesis of agreement between friends.

Let us now compare the two hypotheses in terms of ability to account for the results in the original article. As in Mazur's comment, we ask whether the hypotheses can account for the relative frequency of triads (*vis a vis* chance expectations). Table 1 presents the necessary information.

Column 1 in Table 1 gives the triad fre-



Table 1. Triad Frequency, Triplet Composition, and Mazur Index.

Triad	(1) Frequency	(2) +++ Triplets	(3) +-- Triplets	(4) Mazur Index
0-3-0-B	90%	0	3	- 3/2
2-0-1	90	0	2	- 2
0-2-1-B	83	0	1	0
1-1-1	78	0	1	- 1/2
1-2-0-B	75	1	2	- 1
0-1-2	42	0	0	1/2
0-0-3	41	0	0	0
2-1-0	37	3	1	1/2
1-0-2	37	0	0	1
0-2-1-A	27	0	0	0
0-3-0-A	16	1	0	1/2
1-2-0-A	7	2	0	1
3-0-0	1	6	0	3

quency data from the original article; Column 2 gives the total number of transitive triplets in each triad; Column 3 gives the total number of intransitive triplets in each triad; and Column 4 gives Mazur's index from his comment.

We note that the entries in Column 2 are bunched toward the bottom of the column, while those in Column 3 are bunched toward the top. This is impressionistic confirmation of our hypothesis. The rare triads tend to have fewer transitive triplets and more intransitive triplets.

To put the question more precisely, we calculated product-moment correlations between the predictors and triad frequencies.<sup>1</sup> That is, we calculated the correlations between the numbers in Column 1 and the numbers in Columns 2, 3, and 4. The results appear in Table 2.

Both triplet correlations are in the predicted direction and since the figures in Columns 2 and 3 have a zero-order correlation of -.231, we obtain a multiple correlation of .925 between transitivity and triad frequency. Comparing the  $R^2$  of .856 with the  $r^2$  of .362 for

Table 2. Correlations with Entries in Table 1, Column 1.

Predictor*	$r^2$	$R^2$
Col. 2	.348	
Col. 3	-.689	
Cols. 2 & 3		.856
Col. 4	.362	

\* Signs are adjusted so that a positive correlation means that triad frequencies increase with frequency or score of predictor.

Mazur's index, we conclude that the transitivity hypothesis, although simpler, is not inferior in predictive ability.<sup>2</sup>

Our final argument for the transitivity hypothesis is that of theoretical fertility. Transitivity is not a novel mathematical property but it is a powerful one. In a recent article (Holland and Leinhardt, 1971), it is proven that a variety of structural models, balanced graphs, clusterable graphs, transitive tournaments, quasi-series, and the Davis-Leinhardt model are special cases of transitive graphs (t-graphs). That is, given the absence of intransitive triplets in a graph, one may deduce the existence of each of these structures from further, simple assumptions about pair symmetry (M, A, N edges). For example, a transitive graph that has no A edges (no asymmetric choices) is clusterable. This leads us to our lone disagreement with Mazur. Mazur concludes, "... there [is no] support for the general hypothesis of 'clusterability' since 'agreement among friends' is an adequate assumption." On the contrary, we argue that because clusterability is implied by transitivity, the data provide considerable support for the clusterability principle.

In sum, our current position is actually closer to Mazur's than that stated in the ASR article. At the same time, we (naturally) prefer

<sup>2</sup> A more telling comparison would be to add two more columns to Table 1 for +-- and +--+ triads and then to look for the partial correlations of all four triplet types. This is impossible with the data used here because three of the triad types (120, 021, and 111) have U and D subtypes (Holland and Leinhardt, 1970) that differ in +-- and +--+ triplets. A recent computer program designed by Leinhardt will make such analyses possible, but not in time for this note.

<sup>1</sup> Michael Kearl, Dartmouth '71, was kind enough to assist in the tabulations.

our transitivity hypothesis to his closely related hypothesis of agreement between friends.

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*Editor's note:* Mazur's reaction to the view expressed by Davis *et al.* in the above comment is as follows:

The two above models are quite close in both method and content. As Davis *et al.* point out, our main difference is whether or not "+-- triplets will be common, +--+ triplets will be rare." (Or, in my terms, whether or not friends are more likely to agree on whom they like than on whom they dislike.)

I'm not sure that the transitivity assumption provides a better data fit. As Davis *et al.* point out, Columns 2 and 3 of their Table 1 support their hypothesis, but they also support my hypothesis, since the two hypotheses don't differ on those triplets. Their Table 2 comparisons, based on squaring product-moment correlations, are not very meaningful since my model uses an ordinal, not an interval, scale. The comparison suggested by Davis *et al.* in their footnote 2 is a good one and should settle the point.

#### COMMENT ON "SOCIOMETRIC LOCATION AND INNOVATIVENESS"

In a recent article Becker (1970) introduces to the literature on diffusion of innovation the concept of "system delay" and the suggestion that innovativeness determines centrality in the communication network. This very brief note comments on the second of these two points.

Becker states (1970:280) that "Health officers who value high-ranked sources of information tend to be consulted for advice for several reasons, but the most important of these appears to be their tendency towards early adoption of innovations." This is understandable because according to Becker's analysis professionals will

seek sources capable of providing information on innovation, for innovation confers prestige. However, Becker's Table 2 provides data that show that local professionals are not a very important source of information concerning innovations. Given that the major source of information for innovations is not other local professionals, it is possible that the factors that lead to innovativeness may also lead to centrality. In other words there may be psychological factors which predispose both to centrality and innovativeness. Let us consider some evidence.

One of the most interesting features of diffusion research is that the generalizations made apply across disciplines. Because of this it seems acceptable to consider work relating to consumer innovators. According to Boone (1970:137), prior to his study there was only one investigation (Robertson and Myers, 1969) that dealt with personality characteristics among early adopters of new producer or consumer goods. Although this is incorrect (see, for example, the study by Paul, 1965, and the literature he cites), it is true to say that there have not been many studies dealing with psychological variables. Moreover, many of the early studies were not particularly well planned. We shall discuss here only Boone's study, but first we must enter a caveat. A number of the studies using psychological variables have found no real differences between innovators and others. As a result we must be wary in discussing literature that does find differences. Such literature could reflect that with a significance level of 5% one in twenty studies will show "significant" differences even where none exist. This point is especially important when one considers the well known fact that journal editors have a bias against publishing studies that do not report statistically significant results.

Turning now to the Boone (1970:139) study, he found "The Consumer Innovators scored significantly higher than the later adopters on ten of the eighteen scales of the California Psychological Inventory: Dominance, Capacity for Status, Sociability, Social Presence, Self-Acceptance, Sense of Well-Being, Tolerance, Achievement via Conformance, Achievement via Independence, and Intellectual Efficiency. . ." The description of the scales suggests the sort of person likely to be sought for advice, the sort of person likely to be sociometrically central. Boone found that the innovators did belong to more clubs, etc., and held more offices than did followers. Is it more likely that personality predisposes both to centrality and innovation than innovativeness leads to indi-

viduals being members of social clubs? Reverting to Becker's study, we would wish to know something about the personality of the persons involved and also whether centrality applied to innovative health officers in the field of health only, or in all of their social life. If health officers exhibited centrality both in social life and professional life, this would support a psychological model but tend to reject Becker's model because it does not conform with his "desire to maintain prestige" explanation. Although such a test would be useful, its result would not be decisive either way.

Clearly the arguments put forward here are speculative, and there is some doubt about their validity, given that some studies have not found psychological variable useful in diffusion research. However, there are a number of conflicting suggestions in the general diffusion literature, owing possibly to the neglect of intervening psychological variables. It would seem useful if future studies controlled for psychological variables to establish their significance. We can do no better than echo Becker's (1970:281) sensible concluding point: "Future research, utilizing experimental designs in small-group situations, should yield the greater control needed to further test these causal hypotheses."

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#### REPLY TO BLACK

Since Rogers (1962:178) noted that "there are few studies that have related adequate measures of personality variables to innovativeness," recurrent pleas have been made (e.g., Hagen, 1962; Miles, 1964) for research to identify "psychological" characteristics asso-

ciated with differential adoption of innovations. This avenue for understanding the innovator is analogous to the "trait approach" employed in an earlier decade to study the quite similar phenomenon of leadership, where most of the California Psychological Inventory (CPI) scales mentioned by Black were also applied (Stogdill, 1948). That approach has been termed "futile" (Guilford, 1959:470), mainly because no consistent group of traits could be found to characterize "leaders" independent of the organizational or environmental setting. Attempts to depict the "innovator personality" will doubtlessly face this impasse. Moreover, quoting Rogers (1962:178) again: "Harp (1960) feels that the inclusion of personality variables in analyses of innovativeness will contribute little. He states that if other sociological variables are included in investigations of innovativeness, the effect of '... personality may disappear.'"

Nonetheless, Black fails to mention the few really interesting studies of psychological characteristics of innovators (Straus, 1956; Loy, 1969) which suggest the worthwhileness of including some of these personality inventories in research on innovation.

Regarding my own study, Black postulates the existence of psychological factors which would precede both centrality and time of adoption in some causal arrangement. I thoroughly concur, but would further contend that: (1) my research both suggests and quantifies several of these dimensions; and (2) at least for professionals (whose behavior may be quite different from consumer innovators), early adoption of innovations may still be best interpreted as a mechanism for meeting a psychological need, rather than as the fulfillment itself.

An important element in my proposed model of the diffusion process (Becker, 1970:280) is the argument that motivation for innovativeness is dependent upon the outcome of a kind of psychological "cost-benefit analysis," where the psychological variables are "desire to obtain, maintain, or increase prestige and professional status" and "perceived risks of adoption." Other study indicators which partake of personality dimensions (and which are offered as "predisposing" and "intervening" variables) include cosmopolitanism and political outlook. These measures together with others reported elsewhere (e.g., ideology, activism, and perceptions of community progressiveness and willingness to innovate—see Becker, 1969) provide at least a partial personality profile, with strong similarities to many of the CPI scales cited by Black.

Further, the measure of the extent to which

the drive for status is successful is "relative centrality," and innovativeness is viewed as a valuable technique employed by the professional for achieving centrality. Thus, when Black asks, skeptically, if "innovativeness leads to individuals being members of social clubs," I would note that many clubs invite to membership only individuals with high prestige, which, in turn, may well have been attained through early innovative behavior. Black makes no mention of the literature review and various correlation and partial-correlation analyses which I present in support of this causal chain.

Finally, Black argues that, if health officers with high professional centrality were also found to display high social centrality, it would "tend to reject Becker's model because it does not conform with his 'desire to maintain prestige' explanation." I can see no necessary reason why prestige gained from professional innovation cannot bestow some social status upon the innovator. In any event, the study does provide some data on centrality in social life, since it examines the relative centrality of each health officer in the friendship networks of the group. Correlations between innovativeness and friendship centrality are considerably lower than those for the other networks, implying (contrary to Black's position) that the innovative health officer is not necessarily "the sort of person likely to be sociometrically central" in other life areas.

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#### COMMENT ON GOVE'S EVALUATION OF SOCIETAL REACTION THEORY AS AN EXPLANATION FOR MENTAL ILLNESS

I must confess that when I turned to Gove's "Societal Reaction As an Explanation of Mental Illness: An Evaluation" in *ASR* (October, 1970: 873-883), I fully expected to find an article providing some support for the "societal reaction theory." Much to my surprise I discovered that Gove had brought together certain empirical data that cast much doubt on the validity of the societal reaction theory as an explanation for mental illness.

Thus, I had a most favorable reaction to Gove's empirically supported criticism because I have felt for some time that the "labeling school" was pushing to an extreme a well-recognized social psychological perspective that often was swallowed whole by many of our colleagues. That is, certain sociological investigators (all mentioned by Gove) had taken selected propositions from social psychology, such as Cooley's "looking glass self" and Mead's "taking the role of the other," used largely to gain insight into the nature of the self of normal people, and had extended them to apply to all forms of deviant behavior including mental illness. In stretching these conceptual notions to apply to mental illness, these investigators have literally turned their backs on two centuries of clinical data collected by psychiatrists, analyzed and interpreted in numerous articles and textbooks. Gove neatly documents the inability of societal reaction theory to explain mental illness with its primary emphasis on secondary deviation, as well as the inability of this theory to cope with the consequences of hospitalization, especially in the new climate of hospital treatment for mental illness.

However, because the societal reaction theorists in attempting to explain mental illness have fallen into an untenable position, it does not necessarily follow "that a person's behavior determines the expectations of others to a much

greater degree than the reverse" (p. 882). This applies clearly to psychotic behavior, but, when one moves to areas of more nonpathological behavior, the hypothesis can be advanced that the extent to which cultural or societal expectations determines behavior will vary with age, sex, family role, education, occupational status and possible numerous other factors. For example, cultural expectations are more crucial in the early years, but with growing up and testing their personal strength in certain contexts some men gradually develop the strength to stand alone and literally challenge cultural expectations. If a person's status, wealth and/or personality are strong enough, he may successfully ward off any societal reactions which attempt to define for him a role of secondary deviance.

In many forms of deviance—delinquency, crime, drug addiction—societal reactions certainly may contribute to defining and reinforcing the deviant role, but such reactions can hardly account for the initial behavior in these areas. I suspect it is in the more subtle areas of behavior—personality traits, social typing, situational definitions and the like—that societal reaction will most contribute to the form and direction of behavior in a person. In psychiatry, Ernest Gruenberg's social breakdown syndrome seems to be a good example where societal reaction plays a role in the development of disturbed behavior in a person. Both respond as expected; the others (society) because of long-held ideas about the psychotic, and the psychotic because he holds the same ideas and helpfully is prepared by the initial responses of officials (police, physicians, employers and the like) to his illness. It should also be noted that there are numerous agencies and devices in contemporary society—medical treatment, Alcoholic Anonymous, rehabilitation services, half-way houses and the like—that seek to combat any secondary deviance that society has helped to define.

Even though I think that Professor Gove somewhat shot past his mark in his conclusion, he is to be congratulated for attempting to nail down the sociological bias in an extreme form, and in so doing to bring the sociological perspective in line with existing facts.

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#### COMMENT ON "MENTAL ILLNESS"

Walter Gove ("Societal Reactions As an Explanation of Mental Illness," *ASR*, October

1970:873-884), in speaking of voluntary admissions, states that "Mechanic (1962) and Brown (1961:60) feel that public mental hospitals accept virtually all such patients, but they present no data." Gove is incorrect on both points, and apparently he misunderstands the thrust of my discussion.

Gove refers to observations I made on the definitional process at two public California institutions in 1958-59. Most admissions during that period were involuntary, and my observations refer primarily to such patients. Although I did not present quantitative data, I reported in the article that during three months of observation I never observed a case where the examining psychiatrist advised the patient that he did not need treatment, and that he should not be in a mental hospital.

Gove also seems to miss the entire context of my discussion. The article deals with the screening process, and my point was that whatever serious screening took place occurred before hospital admission. (Incidentally, Gove arrives at a similar conclusion.) The purpose of my article was to explain the contingencies under which the screening process varied. I noted, for example, that "Psychiatric hospitals filled well over capacity will attempt to control more carefully those they will accept for treatment. But should beds be available, as was the case with the hospitals studied, it is likely they will absorb whoever appears, at least for a time." I also noted that "In the crowded state or county hospitals, which is the most typical situation, the psychiatrist does not have sufficient time to make a very complete psychiatric diagnosis, nor do his psychiatric tools provide him with the equipment for an expeditious screening of the patient." Some of the contingencies affecting the treatment process have changed in recent years, and hopefully they will continue to change further. To treat such processes as patient flow as static and independent of institutional influences is to miss a key sociological issue.

I am not in major disagreement with Gove; nevertheless, the record should be corrected.

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#### REPLY TO DUNHAM AND MECHANIC

*Regarding Dunham.* I am pleased with Dunham's overall evaluation of my paper. However, at one point he has misrepresented what I said. In his third paragraph he quotes me as saying "that a person's behavior determines the expect-

tations of others to a much greater degree than the reverse." He then argues that although this clearly applies to psychotic behavior, it is not so clear that it applies in all other areas of behavior and he takes me to task for implying that it does (see his last paragraph).

He has, in fact, quoted me out of context; this is obvious if one looks at the sentence he quotes from. The sentence reads as follows: "The *evidence reviewed* suggests that a person's behavior determines the expectations of others to a much greater degree than the reverse" (italics added, Gove, 1970:882). The evidence referred to deals with the disturbed (generally psychotic) behavior that leads to entrance into the mentally ill role. I then go on to state: "In the long run, however, the expectations of others may play an important role in determining the behavior of a person, and such expectations should be taken into account in a general theory of mental illness" (Gove, 1970:822). This statement is followed by a long footnote discussing one way in which social expectations may contribute to the development of chronic mental illness.

Thus while I feel the societal reaction explanation of mental illness is largely incorrect, I am in agreement with Dunham that the social expectations of others are important in determining behavior, including deviant behavior. I thought this was clear from my paper, but if it is not, I welcome this chance to clarify my position.

*Regarding Mechanic.* I note that Mechanic is not in major disagreement with me. Nevertheless he says that in reporting on his paper I make mistakes in indicating that his observations were based on voluntary patients and in stating that he did not present data. Furthermore, he suggests that I missed the entire point of his discussion. I would like to comment on these remarks.

First, let me say that one of the reasons I incorrectly inferred that his observations referred to voluntary patients was precisely because I did understand one of the major points that he was making. As noted in his letter, Mechanic suggested in his article that when hospitals are overcrowded, they tend not to accept certain types of patients (those who are not seriously ill mentally), but when there are beds available, they will accept all comers. Because his observation regarding admission practices is presented in conjunction with this point, I assumed (incorrectly) that his observations had a bearing on this point, namely that they referred to cases where the hospital could exercise its discretion (voluntary admis-

sions) and not to cases which the hospital *had to accept* (committed patients). A second reason for my assuming that he was referring to voluntary admissions was that in the context from which my citation was taken he explicitly refers to different types of voluntary patients and does not refer to committed patients when discussing types of patients that could be legitimately not be admitted (Mechanic 1962:70).

Turning to the question of whether or not Mechanic presented data, let me restate that he, in fact, presented no data. This, of course, is not to say that his observations were not based on data, but only that he does not share these data with the reader. Mechanic has indicated to me that his observations were not part of a systematic study, that he did not keep formal records, and that his finding was incidental to his major concerns at the time. Such data can be heuristic (and thus important), but one should entertain questions regarding their reliability.

Regarding Mechanic's observation that I missed his major point, let me suggest that he may have reached this conclusion because he did not understand the purpose of my paper. I was attempting to *evaluate* the *empirical adequacy* of the societal reaction explanation of mental illness. Mechanic's paper involved a discussion of the factors involved in identifying and defining mental illness; but with the exception of the passage cited, his paper does not present any original *empirical* observations. Thus the passage of his paper that was cited was the only part that was really relevant to my paper.

Regarding another of Mechanic's comments, I would like to note that I do not consider patient flow as static, and I doubt that anyone familiar with the recent history of psychiatric hospitalization would. Probably the most drastic change in patient flow has been the recent increase in the admission rates that has been associated with the marked improvements in psychiatric treatment. For example, in the state of Washington (1966, 1967) the total admission rate went from 104 per 100,000 in 1955 to 136 per 100,000 in 1966. This increase is entirely accounted for by an increase in *voluntary* admissions. I believe that this is a type of change that the societal reaction perspective would have difficulty explaining. I would also like to note that this change cannot be accounted for by simply assuming the hospital accepted all comers. In my three years of working in a state mental hospital in Washington, I became aware (through chance remarks) of a number of prospective voluntary patients

## REVIEW SYMPOSIUM

**THE COMING CRISIS OF WESTERN SOCIOLOGY, by Alvin W. Gouldner. New York: Basic Books, 1970. 528 pp. \$12.50.**

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This is a big book: big in size, in conception, and in implication. In fact, it is three books. They run concurrently. Something must be said about each before the work as a whole can be correctly appreciated.

Book One presents the main argument. Gouldner asks us to assume that all of the following statements are true:

(1) There are at present only two significantly different approaches to explanation in sociology.

(2) The two are intrinsically opposed. This is so because they spring from groups in society that are inherently in opposition.

(3) One of these approaches derives from Comtian positivism and/or Comtian utilitarianism. The functionalism of Parsons is its most elaborated and characteristic incarnation in the modern scene.

(4) This is the regnant approach in Western universities and will therefore be called Western Sociology or Academic Sociology.

(5) The other approach is Marxism.

(6) Academic Sociology cannot deal with social change whereas Marxism can.

(7) Western societies are rapidly changing and their governors require explanations and information to guide decisions concerning the process and direction of change.

(8) Their demands for a sociology relevant for change are heightened by the commitment in Western societies to a welfare state, itself a novel venture.

(9) The requirements for a new sociology will shape the work of sociologists because the salaries and the research of many sociologists are supported by agencies of the welfare state. On these assumptions, it follows that:

(10) There are great pressures on Academic Sociology to move away from functionalist theories.

(11) Academic Sociology will begin to adopt more and more features of Marxism even at the expense of the purity of its functionalist architectonics. This will be a crisis for Academic Sociology.

(12) Academic Sociologists and their sponsors

will be accepting an approach that is antithetical to the interests of their "class" and supportive of their "class enemies." The crisis is thus personal and social as well as epistemological.

(In a parenthetical chapter (Ch. 12), Gouldner describes the growth of a complementary crisis in Marxism. The states of Eastern Europe are faced with the management of societies that, having changed, are now becoming stable. Marxism lacks the theoretical tools that are needed by their governing classes. Therefore Marxian Sociologists are turning to functionalism and are finding unsuspected values in it.)

It may be, however, that we do not quite believe statements 1-9. Is it really the case that there are only two sociologies that offer significantly different theoretical resources? Gouldner provides no technical criteria to undergird this division of existing theory. He relies instead upon the numbers and "prominence" of the supporters of these points of view. On the one side there is Talcott Parsons and his many distinguished students. On the other side are the intellectuals, planners, and administrators whose approach is applauded by "half the states of the world."

Faced with this impending confrontation between David and Goliath it may be pedantic to ask whether symbolic interactionism, and its relatives and offshoots, ought to be granted status as a third party. Or human ecology. Or some or all of the other varieties of structural and interactionist analysis that abound in our journals and textbooks. Or to note that the bulk of sociological research seems not to employ a functionalist or a Marxian framework for the definition or interpretation of variables. Or to observe that many of the best-known of modern Academic Sociologists make great use of Marx: Lynd, Selznick, Bendix, Barrington Moore, Lipset, Warner, Lenski, C. Wright Mills, Coser, Janowitz, Wilensky, Slater, Blau, S. M. Miller, and Alvin W. Gouldner—to name only a few. Or small-minded to suggest that Academic Sociology has managed to develop important theories of change, including the ones associated with the names of Ogburn, Redfield, W. I. Thomas, Blumer, Turner, and Smelser. And to find that theories of change existed even before

Marx, and that they came from men he would not have embraced as brothers—or have we misread Adam Smith and Machiavelli and St. Augustine? Perhaps, for the duration of this emergency, we should not take time to ask whether most or all of the major approaches to sociology found in Western universities in the last one hundred years are rationalizations in support of interests peculiar to the ruling classes of industrial capitalism. But here, Gouldner himself shares our doubts. He finds the essentials of functionalism even in Plato and in the new understanding of society that he sees emerging in the socialist states.

Does Gouldner fully accept his nine assumptions? As anyone knows who has followed his publications, Gouldner is immersed in the history of social thought and of sociological theory. He has made the sociological perspective his own and has offered a series of uncommon extensions and explications of its powers. He does not retract his nine assumptions, but he can scarcely accept them without important qualifications. It is therefore inevitable that, as Book One unfolds, the magnitude and certainty of the coming crisis diminish and the description of its nature becomes progressively vague. Gouldner's final position is worth quoting:

The central implication of a crisis is not, of course, that the "patient will die." Rather, the implication is that a system in crisis may, relatively soon, become something quite different than it has been. A system undergoing crisis will change in significant ways from its present condition. While some of these changes may be only temporary and may soon restore the system to its previous condition, this is not the distinctive implication of a system crisis. A crisis, rather, points to the possibility of change that may be more permanent, producing a basic metamorphosis in the total character. . . . (p. 341)

And, he continues:

. . . My view is that an infrastructure conducive to a functional theory will persist for the foreseeable future. At the same time, however, I expect it to have a less commanding influence on Academic Sociology as a whole, leaving increasing room for the development of social theories of a less conservative character and, indeed, I expect that a part of sociology will become increasingly radicalized. There will, in short, be a growth of a "radical sociology" which, while never the dominant perspective of Academic sociologists, will grow in influence, particularly among the younger, rising generation. (p. 437)

There is enough use of "may" and "possibly" here to suggest that Academic Sociologists need not immediately clean out their desks and turn in their keys.

Book Two would seem to be a natural subdivision of Book One, but it is developed along

independent as well as complementary lines. Its subject is Parsonsian functionalism in its ideological relevance, and Gouldner grants it more than 60 per cent of his pages.

There are two ways of coming at this subject. Gouldner takes both. He considers the formal character of Parsonsian theory and the special place given in it to factors like morality, religion, and culture, and to their system-conserving properties. He also makes an informal content-analysis of Parsons' writings to isolate the author's latent views on topics crucial from a Marxian perspective: change, property, power and wealth, moralistics and order, and the main drift of social evolution.

A great deal has been written about Parsons from the first of these approaches, some of it appearing in Gouldner's own papers on reciprocity and on functionalism. He recounts much of it here and draws the familiar conclusions. There are, however, some valuable additions. Gouldner on the evils of private property is a delight to read. So is Gouldner when he proposes that conflict and competition rather than normative commitments are the source and guarantee of social order, and when he suggests that these processes embody a superior morality and a superior value for individuals. Little of this is new or responsive to the more impressive counter-arguments, but the language is fresh, the juxtaposition with Parsons' conclusions is dramatic, and the level of sophistication is exceptionally high.

Many of the same things can be said about his content analysis of Parsons' personal ideology. It is not news that Parsons believes religion is important, that his treatment of social evolution has a strong Hegelian cast, that he tends to see Protestant Christianity and the social system of the United States of America as at the cutting edge of the social future, and that he believes it inevitable, and perhaps a good thing, that we have, in modernized societies, a large growth of rationality and science, of specialization, and of organizational complexity. Several previous commentators have observed that when he writes about stratification, power, wealth, and influence, Parsons has little to say concerning conflict, force, or strategies of manipulation. (Gouldner seems to regard this as evidence for a sin of commission—a purposeful, or almost purposeful, muting of everything that might upset an existing social order. Parsons himself would likely see his omissions as resulting, not from his ignorance of the fluidity or the rough and tumble of social life, but from his effort to focus on points that existing theories failed to systematize: on the sources and mechanisms of organized continuity.)



It is here in Book Two that Gouldner gives us some data on professional sociologists. He and J. T. Sprehe conducted a survey of American sociologists, asking for, among other things, their view of functionalism. The questionnaire was mailed in 1964 to 6,762 members of the American Sociological Association. Fifty-one per cent replied. Eight per cent of the respondents agreed "in varying degrees of intensity" with the statement that "Functional analysis and theory still retain great value for contemporary sociology" (p. 168). There was a negative relationship between the respondents' answers to this question and their age: five per cent of those over age fifty were unfavorable and 14 per cent of those of ages 20 to 29 (p. 377). It was also discovered that respondents who believed that shared beliefs and values are important for social order were more favorable toward functionalism than were others. The question was posed in this way: "The most basic sources of stability in any group are the beliefs and values which its members share." Eighty per cent of those favorable to functionalism agreed, as contrasted with 64 per cent of those who were unfavorable (pp. 247-248). Next, it was found that the respondents who claimed some specific religious affiliation were more often favorable to functionalism than those who claimed none (pp. 258-259). Finally, Gouldner and Sprehe discovered that respondents who were friendly to functionalism were more likely to have thought of becoming clergymen (27.6 per cent of all respondents) and to attend church more frequently than were those who were unfriendly (pp. 243, 258). Gouldner's general conclusion from these data is that Parsons' theory only makes articulate a point of view held widely among American sociologists. Since he did not ask his respondents for their evaluation of other theoretical perspectives or for comparative judgments about the relative merits of functionalism, this conclusion is pure conjecture.

And so to Book Three. This is the motivational core of the whole. It makes its presence known in the first paragraph of Gouldner's preface:

Social theorists today work within a crumbling social matrix of paralyzed urban centers and battered campuses . . . we theorize today within the sound of guns. The old order has the picks of a hundred rebellions thrust into its hide. (p. vii)

What characterizes that old order? Do not confuse it, Gouldner says, with urbanization, industrialization, accelerated social change, or bureaucratization. Do not identify it with the spread of rationality and impersonality at the expense of sentiment and community. All of these are

of lesser importance or are only outgrowths of what is central. The tough, deep-rooted, and corrupting center is free-market capitalism. Whether it is acknowledged as such or not, Western sociology is one long apologetic for this capitalist order. The time has come to cry it down from every classroom in the land. And not only that. The time has come to cry up the values of the psychedelic culture—the hippies and the New Left, the values of the young Marx who saw things human and whole and who would have rejected the rigidities of Stalinism and the crudities of Maoism, the values of the Romantics who embodied passion and particularity. And not only these. Gouldner is a bred-in-the-bone sociologist who offers as his epigraph for this book these words from Nietzsche: "Here are the priests; and although they are my enemies . . . my blood is related to theirs." A hundred years of specialized sociological theory and four thousand years of recorded social thought contains many and diverse conceptions of proven value and he will not let them go. They have been perverted but not outdated, and all must be renewed by a new integration and then reaffirmed. He calls us to effect that integration. He christens it "reflexive sociology." He calls us to enact it; to embody it; to cry it up.

This summons is attractive. We are people first and sociologists only second, third, fourth, or barely so. Our sociology necessarily serves the higher interests in our lives or it subverts them, and it does so whether we will it or not. The worth of sociology, as of any human enterprise, is its relevance for people. It can pass this test and fail to meet some canons of technical rigor or of formal comprehensiveness, coherence, and elegance. Or it can meet every scientific criterion and be worthless.

Of equal importance, the quality of science as science depends heavily upon the rigor and comprehensiveness and sophistication with which we articulate our larger interests and the steadfastness with which we serve them. The better developed is our foundation in social understanding and commitment, the greater is the likelihood that our approach in a science will be fresh and fundamental, the less is it likely that we will be seduced by scholasticism and technicism or limited by parochial concerns.

The implementation of these ideas requires more than a style of science. It requires a way of life, a *praxis*. It entails commitment, habit, and effort—in community and society, work and play, family and friendship. That is one of the deep implications of the sociology of knowledge and also of every faith sufficient for the whole of a human life.

To this point, most sociologists would concur with Gouldner's manifesto on behalf of a reflexive sociology. Nor would many challenge his statement that the problems addressed in a science at any given time are chosen because they are relevant for some wider concerns of the scientists involved, and that the findings of science will be interpreted and used according to that relevance. And they would be pleased that Gouldner, if I read him correctly, does not deny that technical considerations also have an important role in what we do, especially in helping us to judge the reality and reliability of observations and the consistency and the formal centrality of arguments. He does not deny that the things we observe are independent of our conceptions or that they often refuse to conform to our most trusted and elegant theories. Rather he chooses to stress the ways in which scientific work is *not* "value-free" and to scant those of its aspects in which the play of values is sufficiently controlled to enable the events we observe to "bite back"—to confirm or disconfirm our hypotheses.

But immediately Gouldner offers us this broad view of reflexive sociology, he begins to impose limits upon it. He will not accept just any wide and humane commitment as the basis for his practice or ours. The traditional humanisms will not do because they carry for him a deep taint of elitism (p. 503). No religious commitment, traditional or otherwise, is ever mentioned. Liberalism is not a candidate: "Reflexive Sociology premisses that the character of any sociology is affected by its political praxis and that further development of sociology now requires its liberation from the political praxis of liberalism" (p. 502). Nor will society itself provide a suitable framework. Man is the measure (p. 508). And this is only a partial catalogue of the negatives.

There are near-positives as well. "Reflexive Sociology . . . rests upon an awareness of a fundamental paradox: namely, that those who supply the greatest resources for the institutional development of sociology are precisely those who most distort its quest for knowledge. And a Reflexive Sociology is aware that this is not the peculiarity of any one type of established social system, but is common to them all" (p. 498). "A Reflexive Sociology can grasp this hostile information: all the powers-that-be are inimical to the highest ideals of sociology" (p. 499). "The integrity of a radical, and hence a Reflexive Sociology depends on its ability to resist all merely authoritative definitions of reality, and it is most authentically expressed in resisting the irrationalities of these authorities met daily in eye-to-eye encounter" (pp. 503–

504). More specifically; a man is no radical who "plays the sycophant to the most petty authorities in his university," "who is a coiled spring ready to punish the rebels among his own graduate students," who "subserviently fawns upon his Department Chairman," "who denounces opportunistic power politics, but practices it daily among his university colleagues" (p. 503). "Something of what this [Reflexive Sociology] means in a university context is suggested by Karl Loewenstein's personal appreciation of Max Weber: 'He could not hold his peace. In all the eight years that I knew him, he was forever involved in scholarly and political feuds which he waged with implacable intensity. . . .'" (p. 504). But Reflexive Sociology "is [not] only a nay-saying . . . ; it should be just as much concerned with the positive formulations of new societies, of utopias, in which men might live better. . . ." (p. 503). "It sees that history, culture, and society never exhaust biography, that everywhere men live with the 'loose ends' of an existence that they are constantly striving to pull together," and it is designed to help provide that integration (p. 510).

This last is really the crux of Gouldner's book. Taken as a whole, his treatise deals only peripherally with sociology. It is a call for the reformation of man and society in the West. (Implicitly, in the East as well.) Parsons and the Functionalists are to be taken seriously not for themselves but insofar as they represent Western civilization as it has existed and grown: beginning with the Greeks, continuing to the Marxist diremption, persisting ever since. It is in this context that it makes sense to juxtapose the official orthodoxy of half the states of earth against central tendencies in the thought of members of the American Sociological Association. It is in this context that the subtleties and complexities of Parsons' thought can be ignored—that the powerfully radical elements in his whole corpus can be discounted and only the conservative elements be brought to light and that Gouldner can dare to offer only the slender documentation that he provides for his assertions. And this context may explain why Gouldner's presentation of the methods employed in his and Sprehe's survey of sociologists is too fragmentary to allow us to judge its worth and why every statement he makes about the findings is flawed by technical errors or unjustified interpretations. If one says with C. Wright Mills that it is only the "main drift" that counts and if one knows what that drift is—knows it beyond the need for further argument or evidence—and if men and societies and "facts" are to be considered in one's exposition only in their status as representatives of some historical

tendency, then one need not bother to try to see them as they are in their historical particularity.

This context may also explain some of the grounds for Gouldner's appreciation of Marxism. Unlike Western Sociology, Marxism attempts no clear differentiation between social thought and social science or between social thought and social philosophy or between social philosophy and an ultimate metaphysic. True, the record shows the failure of almost every distinctive historical forecast based upon it, but it has a powerful attraction as a unified system of thought and one that is in touch with major elements in Western Humanism and that approaches in comprehensiveness the structures of religious faith. Indeed, Gouldner tells us that the task set for Reflexive Sociology is an integration of the whole of human existence that "was once the task of religion" (p. 510).

Perhaps I err. There is a sense in which sociology and sociologists play a central role in this book. Gouldner seems to see us as the po-

tential or implicit theologians of the new secular religion for which he calls.

A call for reformation is not reformation itself nor a prescription for life when once it is reformed. A call for a perpetual reformation in which every variety of social order and culture and personality is idolatrous is, by itself, a summons to free ourselves for nothing. It strips sustaining value from everything about persons that is particular to their time and place and bodies and it leaves, as a characterization of men and women, only a few biological imperatives around which to build the fundamentals of their existence: sex, hunger, motility, and death. And that is perilously close to the conception of man as man to which Gouldner comes in his concluding chapter. Gouldner prophesies that men thus unencumbered will be warmed by a new and loftier morality. That is hard to believe. To cite another social theorist, "if I speak in the tongues of men and of angels, but have not love..."

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It is fortunate that in this period of burgeoning publication there is on occasion the appearance of a truly monumental book; one which ought to be read and which reestablishes the notion that it is as important to read books as well as to write them. Alvin Gouldner's book is one of these uncommonly rare, important books. It is a book that should be read by all sociologists, as well as all others who are challenged by some of the most critical questions of our times. This is the second in Gouldner's trilogy on the social origins of social theory, and it goes to the heart of some fundamental questions which touch the lives of all men today.

In essence, this work outlines the role of ideology in dominant social theory today and traces the historical emergence of what Gouldner calls Establishment Sociology. In this discussion he offers some overriding observations—probably the most crucial of which is the contention that sociology is not pure science at all, but the work of humans who have failed to account for their own humanness while attempting to objectify the study of human conduct in their inquiries. The uses of sociology have changed over time in the West, and in the socialist nations as

well. Gouldner discerns an entropy phenomenon; that is, the convergence of intellectual traditions between academic sociology (i.e., Parsonian functionalism) and Marxist sociology. As he traces the challenges to academic sociology in the United States, Gouldner offers an urgent call for what he calls a Reflexive Sociology, about which I will have more to say below.

Most readers in the field of sociology will place this significant book in the context of Alvin Gouldner's other writings, and in the larger debate occurring within the field. One can point to Gouldner's sociology-of-knowledge analysis of the ideological role of the recent report on sociology by the National Academy of Sciences, with its non-scientific advocacy of welfare statism, in contrast to the practical-technical concern with efficacy and public accommodation in the remarks of two recent presidents of the American Sociological Association in the same symposium (*ASR*, April 1970). Or we might note the concern expressed by the current president (*ASR*, October 1970) as he assesses sociology and the distrust of reason, and contrast this with the coming crisis which Gouldner senses in the field of sociology. Establishment Sociologists decry the assault on the universities, plead that the academy is not able to deal with the societal crisis, and argue that intellectuals have recoiled from their frustration and sense of impotency and have wrongly converted the universities into political battlegrounds. Further, they see in contemporary

\*I am grateful for the comments I received on a draft of these remarks from my colleague Ted Goertzel.

youth the manifestation of unresolved generational-familial conflict which explains the unpleasantness of youthful protest within and without the halls of ivy. Non-sociological social analysts, on the other hand, have lauded the emergence of a new consciousness illustrated by contemporary youth with new mannerisms, values, and behavioral commitments (Charles Reich, *The Greening of America*). Gouldner's books will be a personal comfort to neither perspective, but his approach will clearly sit most uncomfortably with Freudian oriented and influenced social science community, as well as Establishment Sociologists in the functionalist tradition.

Gouldner's monumental work goes beyond the writings of C. Wright Mills, whose writings have had great impact on anti-establishment thinking, and whose concerns were with the ideological factors in social theory. Like Mills, he offers a very substantial and well-documented argument that makes other current work seem more significant (for example, David Horowitz, "Social Science or Ideology," *Berkeley Journal of Sociology*, 1970). In this present book Gouldner aimed to "... search out some critical understanding of the social mission of Academic Sociology, and to formulate some tentative ideas about the social mandate with which it operates, the ideologies it expresses, and the link it has to the larger society" (p. 26). Gouldner begins his book with an extremely insightful commentary on the importance of social theory and a strong word of caution to radicals who believe theory is irrelevant: "Radicals who believe that they can separate the task of developing theory from that of changing society are not in fact acting without a theory, but *with* one that is tacit and therefore unexaminable and uncorrectable. If they do not learn to use their theory self-consciously, they will be used by it" (p. 5). The argument is then assuredly not antitheoretical, but points out the ideological properties of social theory. While American cities burn and American military might destroys a people and culture in Southeast Asia, Talcott Parsons and his fellow Establishment Sociologists launch their celebration of American society. "Politically, Lipset's argument is the classical conservative brief against abrupt tensionful change which might disrupt legitimacy, continuity, and gradualism" (p. 48). But Gouldner does not contend that mainstream sociology is conservative today (see T. B. Bottomore's analysis of Lipset in "Conservative Man" in the *New York Review of Books*, October 8, 1970). Rather, he indicates that sociology is most congenial with corporate liberalism and serves state welfarism very well: "the academic social sciences embody an ac-

commodation to the alienation of man in contemporary society, rather than a determined effort to transcend it" (p. 53). If this sounds like the pietistic clergyman quieting his flock (à la Liston Pope's *Millhands and Preachers*), then we should have no difficulty in accepting L. Baritz' view of sociologists as the servants of power. And this is precisely the role which Gouldner sees sociologists performing. Since liberalism allows them to seek remedies for societal failures without challenging essential societal premises, the sociologist can work within and then for society—and with a substantial stake in the society, for the sociologist's "opportunities grow as the crises of his society deepens" (p. 59). "It has become the essential role of the sociologist-as-liberal-technologue to foster the optimistic image of American society as a system whose major problems are deemed altogether soluble within existent master institutions, if only enough technical skills and financial resources are appropriated" (p. 501).

Gouldner traces the historical formulations and evolution of social theory. He reviews the experience of the French and Industrial Revolutions and examines the emergence of utilitarianism and positivism as the 19th century accommodation to changing class relationships. Yet, since "functionalism thus served to defend existing social arrangements on nontraditional grounds" (p. 124) it was congenial in the American setting but not in the English. American functionalism, personified in the work of Talcott Parsons, "is engaged in constituting social worlds, rather than simply in researching them" (p. 84). Its function is not to research facts, but "to provide an anxiety-reducing reorientation to it" (p. 86). Gouldner's efforts are not to trace a detailed social history of sociology and its theorists, but to note that through the major periods of development—positivism, Marxism, classical sociology and Parsonian structural-functionalism—theorizing "has taken one of its motivations from political frustration and powerlessness" (p. 153). Since the objective of this book is to note the factors shaping social theory, Gouldner comes after his first chapters to the major recent phase of American sociology, and notes that a careful examination of "the World of Talcott Parsons" is called for. More than one-third of the book is placed in this section, which is in addition to the considerable attention given to Parsons throughout the rest of the book. The logic of such emphasis would seem apparent, and Gouldner's lucid, brilliantly charted, and persuasive analysis makes this analysis of Parsons the single most important work on the topic in the sociological literature to date.

In light of the very considerable debate about the young-versus-old Marx (something set aside in the socialist states and still an intellectual exercise among bourgeois social theorists, as pointed out by Gajo Petrovic, *Marx in the Mid-20th Century*), it is interesting to follow Gouldner's analysis as he indicates the shift in the writings of Talcott Parsons from the late 1920's, through the 1930's, and up to the present time. He notes that "Parsons never has cited a single one of Marx's own writings, not even in his 1965 paper on Marx" (p. 185). This, says Gouldner, is because of the congenial nature of early Marx to a voluntaristic theory, and since Parsons was committed to an anti-Marxist posture he basically ignored this complementarity which Gouldner sees. Parsons' change from the 1920's to the 1930's is seen as an intellectual response to the national and international crisis of the times: the depression and its socialist assaults. However, since functionalism is "not really committed to social order in general, but only to preserving *its* own social order" (p. 281), the theory based on a quest for social order is an ideology that is "more congenial to those who have more to lose" (p. 254). In brief, in Gouldner's overview we see a protected class of social theorists living out the depression on the elite Harvard University campus, removed from any of the social ills and yet sensitive to the ideological critiques of the American social-economic-political order.

From this vantage point comes forth a theoretical schema in Parsons' voluntaristic theory of action. With the shift in the posture of the society after the Second World War another kind of stress emerges. "In 1953 Parsons looked upon his American system and found it good" (p. 289). "The voluntaristic schema extolled individual striving, and the social system model extolled spontaneously regulated patterns of cooperation . . . ; both . . . are implicit generalizations from an image of a free market and a 'laissez-faire' economy that Parsons projected onto the society at large" (p. 348). This is all well and good for the 1930's when the free enterprise system needed defending, or in the post-World War II period when it was clear that *our* social system emerged militarily victorious and was still socially and economically more to be desired than the opposite models.

With the growing importance of the state, however, a shift in Parsonian theory was required. This was not difficult, for in Gouldner's words, "functionalists . . . are conscientious 'guardians' devoted to the maintenance of the social machinery of whatever industrial society they are called upon to service" (p. 332). What the State needed was a social theory that would

indicate the direction for domestic problem solving and extending American power abroad. This thrust has produced a new sensitivity to economic variables, and Gouldner examines the move away from the functionalism "which premises that social order can be maintained regardless of the level and distribution of economic gratifications, and thus treats economic arrangement as 'givens'" (p. 343). I see a breaking away from the functionalist perspectives toward economic sociology taking on significant proportions in the present period, and a return to classical political economy as an intellectual stimulus that I think will lead to important theoretical developments in the next period of sociological thinking. On this I probably agree with Gouldner, although as I shall suggest below, he does not paint the portrait for us in this work.

In short, Gouldner has given us an intellectual overview of the dominant theoretician in American sociology. He has shown how Parsons has evolved his theoretical posture in response to the empirical demands of the depression and the ideological challenges to the American system, again in response to the entry into the Cold War, again with the emerging importance of the welfare state. In this most recent period Parsons continues to extoll the virtues of the American system, but produces "a theoretical 'victory' in place of a socio-political, real one" (p. 367). In addition to Parsons' shifts, there has developed a "left functionalism" that has moved into a theoretical convergence with Marxism. Gouldner sees this in the writings of Neil Smelser and Wilbert Moore—students of Parsons, prominent members of Establishment Sociology, and sociologists concerned with social change theory and relationships between economic and social variables. Why this shift? But of course! After at least two generations of social research, we have accumulated a wealth of research data which should offer empirical and demonstrable evidence that would require a shift in theoretical orientations. Science *has* shown the way. But no! For Gouldner concludes: "none of the changes discussed have derived from the accumulating empirical basis of sociology . . . ; there is no evidence that it [social theory] changes in the way that the conventional 'methodological model' suggests it does, namely, out of its interaction with or as a response to new *data*" (p. 370). And here we have the point of it all: sociology is the product of humans who fail to appreciate the human and ideological traits that they project unto their research subjects among themselves, and yet their social theories are shaped by such ideologies, human experiences, and a range of inputs which preclude the acceptance of their

intellectual outputs as value-free, objective, or scientifically based.

Following his analysis of the development of sociological theory and the dominant functionalism of Talcott Parsons, Gouldner traces the evolution of the welfare state and the shifts in Parsonian theory to accommodate the new demands for a practical social theory that will give cause for celebration and yet assist in the need to smooth societal tensions and make the system more efficacious and harmonious. Gouldner then notes the emergence of contrary theoretical approaches, which share many assumptions in common with Parsonian theory, and yet diverge. Erving Goffman "is to the sociology of fraud what Fanon is to the sociology of force and violence" (p. 384); "dramaturgy reaches into and expresses the nature of the self as pure commodity, utterly devoid of any necessary use-value: it is the sociology of soul-selling" (p. 383). Juxtaposed to this approach to examining the social packaging of the individual, Harold Garfinkel and the ethnomethodologists delight in stripping away the social veneer and exposing the invisible features of social-cultural operation. "If Goffman's social theory was a 'cool' or 'hip' sociology congenial to the politically passive 1950's, Garfinkel's is a sociology more congenial to the activist 1960's, and particularly to the more politically rebellious campuses of the present period" (p. 395). Yet Gouldner does not see in Goffman, Garfinkel, or the exchange theorists—Homans and Blau—an adequate sensitivity to the dilemmas facing sociology. To be sure, the New Left, the character of the American university, and changing social and cultural structures are causing the crisis in sociology, and a range of contrary approaches do attempt to speak to the dissonance growing with Parsonian functionalism. But in Gouldner's eyes the continuities and pitfalls of the past are all too prominent in these existing theoretical alternatives; they have not resolved the sociological crisis in the West.

Chapter 11 of the book is titled "From Plato to Parsons: The Infrastructure of Conservative Social Theory." Here Gouldner suggests that there is a continuity from Plato to Parsons, with continuous emphasis on social stability and order, on permanence rather than social change. Platonism and Parsonism share a stress upon the rewards of social conformity and the costs of deviance; they conceive of good society and the good social actor who fits into this societal imagery. There are, however, a number of factors which are generating the crisis in the sociology of the West, and the Platonic legacy manifest in Parsonian theory is receiving criti-

cal intellectual challenges. One such element is the interaction with Marxism. And in the following chapter, Gouldner discusses the crisis of Marxism and the challenge of Western or Academic Sociology in the socialist states of Eastern Europe and the Soviet Union. Using a sociology of knowledge framework, Gouldner develops a model of the structural sources of the institutionalization of academic sociology (pp. 467 ff). "Whether symbolically or instrumentally significant, an Academic Sociology becomes institutionalized when the integration, the sector coordination, of an industrial society is defined as the responsibility of public authorities, and as a technical task rather than as a problem in policing and political mobilization" (p. 469). Since the integrative tasks are seen as more critical in the socialist states in Eastern Europe, there is a new receptivity to academic sociology. American sociology, according to Gouldner, is most supportive of administrative, rather than critical, sociology in the Soviet Union, which means the less liberal and more communist-controlled wing of Soviet sociology. But, after all, what is fundamental is that "a sociology with a nonpartisan self-image can become institutionalized when the elites of a society are confident that its social scientists are, in fact, *not* neutral" (p. 470).

Will the Marxist sociologists move in the direction of administrative technocrats working on the basis of an increasingly functionalist orientation and supported to do the bidding of state capitalism? Gouldner does not offer a complete picture, but sees much convergence between the Marxist and functionalist traditions, and notes the rapid rise in technical social science in the Soviet Union and Eastern European countries influenced by American sociology. I think he has inadequately dealt with the role of a critical sociology blending the Marxist traditions of the East and West. He does note the Marcuse-Fromm-Bottomore, *et al.* ties with the intellectuals, especially Yugoslav and Eastern European. Gouldner notes the systemic features of industrial societies and the concomitant needs and crises, and this discussion is an important contribution to recent work by Birnbaum (*The Crisis in Industrial Society*) and others. Perhaps in his next book more attention will be placed on the impact of non-Parsonian sociology in the West upon the Marxist scholars in the East, and will include an appraisal of the transnational intellectual developments in this vein.

Gouldner's book is not the brilliant commentary on American society that we see in, say, Philip Slater's *The Pursuit of Loneliness*, but it is a fine diagnostic analysis of sociology and

the factors which have shaped the sociological enterprise in the United States. As such, it is probably the most important book in the field in recent years. The trouble may be that people will follow Parsons' example of writing about Marx based on work about, rather than by, him; we may well see people talking about Gouldner's analysis rather than reading this exceptionally fine book. I hope that this is not the case—that it is widely read and has the impact on the field it deserves.

One thing the Gouldner book will do is offer some legitimacy to radical social science by documenting what many writers have attempted to show. The growing interest in serious study of Marxist sociology and political economy will be nurtured by this work, and it should be expected that this scholarly treatment by a well-regarded sociologist will illustrate alternate role models to students of sociology. The one thing that this book will not do is offer an articulated alternate, or radical sociology. Gouldner suggests some of these threads, but like most critics, he does not offer up a full substitute set of intellectual clothing. In a section on the potential of a radical sociology (p. 437 ff), he suggests that this will be developed first by the changing *praxis*, that is, the political activities of sociologists working to change their environments and relate social theory to practice. Second, the interaction with Marxism will be important in shaping a radical sociology. A third factor in forming radical sociology is the contradictory character of American sociology: the role of the sociologist as the market researcher for the welfare state, the demand for objectivity in the methodology of social research, and the historical belief in society as shaping men rather than men shaping society—a belief reinforced by the sense of impotency that academic sociologists feel as they serve the authorities. Gouldner states that "The task, then, is not simply to denounce Academic Sociology, but also to understand that it contains viable elements and liberative potentialities" (p. 442).

The thrust of what he says is that the liberative forces are distinctly here, that they are increasing in strength as the New Left pressures the universities and the social sciences while addressing the institutional arrangements in the society. But most importantly Gouldner sees that "It is precisely here that the *praxis* of the radical sociologist has its greatest intellectual potentiality for, through it, he learns and teaches a different set of assumptions: that men can resist successfully; that they are not simple the raw materials of social system; that they can be the shakers and makers of worlds that

are and worlds that might be" (p. 441). Here it seems Alvin Gouldner is looking at the Sociology Liberation Movement and saying: "Right on!" Or is he?

Gouldner closes his book with a discussion on "Toward A Reflexive Sociology," which "is concerned with what sociologists want to do and with what, in fact, they actually do in the world" (p. 489). Furthermore, this call argues for a new historical awareness of self on the part of sociologists, and ultimately Reflexive Sociology is radical sociology. "Radical, because it seeks to transform as well as to know the alien world outside the sociologist as well the alien world inside of him" (p. 489). In a word, this calls for decisions about how one lives as well as how one works as a sociologist. In that sense Reflexive Sociology is congenial with what I have written as well as a number of others who have attempted to develop a radical perspective in sociology (see the Winter 1970 issue of *Sociological Inquiry* devoted to this question). Yet, I sense in this discussion a prime emphasis upon the epistemological problems of self-awareness and the transformation of the sociologist through an engagement with the world, a *praxis* which shapes his experiences and his social theories. Radical intellectuals will not disagree, but they will find in Gouldner's writing the lack which characterizes most of the critical sociology today; namely, a prescriptive component which follows the diagnostic analysis. A Reflexive Sociology according to Gouldner does not build theory as an inductive process based on the accumulation of facts alone; nor is it characterized simply by what it studies. The self-awareness which is called for will recognize the inevitable tendency for all societies "to transform (a sociologist) either into an ideologue of the *status quo* and an apologist for its policies, or into a technician acting instrumentally on behalf of its interests" (p. 498). But what comes after this insight? Where does the self-awareness lead the reflexive sociologist? At the end, we have little to answer these questions in this present work by Alvin Gouldner. But, as he himself points out, his own intellectual career is illustrative of the movement in the discipline that he discusses. We may well expect additional future prescriptive commentary from this creative analyst.

For the present, however, there is the growing role of sociologists serving the needs of the military-industrial-educational interests. As social researchers increasingly lend their technical competencies to the state to assist in the rationalization of policy (oftentimes deluding themselves into believing that they are critical

for the formulation of social policy), the challenge for alternative directions mounts. Gouldner has attacked those whose work has been advocacy, or what he calls "underdog sociology." The message is not for advocate research for the victims of the society, but that the growing dependency of sociology upon the authority of the state militates against intellectual autonomy. The White House sociologist in residence has advised the present Administration to pursue a policy of "benign neglect" in relationship to Black America. Rank-and-file sociological researchers will now compete for research funds to facilitate this policy, to make social agencies more efficacious. In this context it is very important to turn to Gouldner to understand why social theory and the craft of sociology has come to its present state.

It is very important to discern the historical processes which have shaped sociology and to note the entropy or convergence between functionalism and Marxism, as well as the overall crisis in Western sociology. Gouldner's book is an extremely important contribution to make us aware of what sociology is, where it has been, and where it appears to be headed. To fault him on an incomplete prescriptive analysis of alternative future directions in sociology is only to share the criticism we have of ourselves and those who have attempted to contribute to the development of a critical and radical sociology. But Alvin Gouldner's contribution in this book is one almost unmatched, and he does challenge us by indicating some guidelines and future tasks to respond to the crisis in sociology he brilliantly analyzes.

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As a student of Gouldner's and then a colleague a decade ago, it was clear to me that his intellectual commitment was to the revitalization of functionalism. As I understood it, this was to be done by an infusion of Freud and Marx so as to make functionalism the foundation of a clinical (as opposed to an engineering) applied science, much in the spirit of the eighteenth century French philosophies.

The events of the 1960's and Gouldner's place in them have influenced the development of his program. Not an ivory tower theorist, he founded *Trans-Action*, a popular sociology magazine, directed a large-scale study of urban poverty, and chaired a first-rank department of sociology. More recently he has lectured widely throughout Europe and listened to young radical and humanist sociologists. Over the years, Gouldner's criticism of his sociological colleagues has become more intense. In a series of increasingly strident condemnations, he has—often tellingly—shown how sociologists, including some at his own university, make themselves serviceable to a Leviathan of "The Welfare, Warfare State." In this same period his study of Marx has increasingly focused on the humanism of the young Marx. His concern with Freudian imagery has led to an analysis of the Greece of Plato. His embrace of the heuristic functionalism of Merton has turned to a critical analysis of Parsonian statics. Thus, over the 1960's Gouldner's commitment to make functionalism a basis of clinical sociology has been transformed. The full meaning

of this change can be gauged in his wide-ranging and deep progress report, *The Coming Crisis of Western Sociology*.

This book cannot easily be reviewed in the usual summary-and-evaluation format. The diversity of its topics—which include "sociology as popular culture," "the limits of welfare state," "structural functionalism in the context of the great depression," "morality and imputed nonpartisanship," "pessimism: death and the human condition," and "reflexive sociology as a work ethic," prompt the reviewer to adopt some particular view. Thus, we will focus on Gouldner's call for the making of a "Reflexive Sociology," recognizing that while such a perspective clarifies much, it takes other valuable contributions, including most of his extensive commentary on Parsons, out of focus.

At a number of points it will be instructive to compare Gouldner's perspective with that of C. Wright Mills. The comment of Bennett Berger in *The New York Times* to the contrary notwithstanding, this book can be fruitfully understood as tracing the path charted by Mills in his famous work, *The Sociological Imagination*. Both books focus on the sociology of sociology—an analysis of the ways in which the organization, career, client, and personal commitments of sociologists influence their work and the thrust of their discipline. Gouldner addresses young radical sociologists who find the current ambiance of Academic sociology irrelevant to their personal experience. Mills, writing in the late 1950's, addressed those similarly ajar, but he nearly despaired of finding any among the ranks of sociologists—  
young or old.



Mills analyzed several "habitual distortions" that prevent sociology from fulfilling its liberating mission in society. These include grand theory, abstracted empiricism, ameliorative applied research, and the bureaucratic ethos. In other essays, Gouldner has treated each of these pathologies, but in the book under review, he concentrates on grand theory as exemplified in the works of Talcott Parsons. He reviews four decades of Parsons' writings, showing that grand theory has continually been modified in the direction of the evolving ideological needs of the American State. Curiously enough, this analysis of Parsons' changing ideas is made more in the style of the classical sociology of knowledge tradition, in which a man's ideas reflect his "class" interest, rather than in the sociology-of-sociology mode that Gouldner espouses. Much, but not all, of this extensive critique of Parsons has been made before by Mills, Merton, Dahrendorf, Sorokin, Lockwood, Buckley, Blumer, Stinchcombe, Anselm Strauss, Smelser, etc. and importantly by Gouldner himself.

What is new in the context of 1970 is Gouldner's declared reason for such a lengthy exegesis of Parsons—his assertion that "if it is the present we wish to understand, then it is above all with Talcott Parsons that we must be concerned" (p. 168). This is a legitimate and interesting premise for a sociology-of-sociology study, but no such inquiry is forthcoming here. Rather, Gouldner asserts that Parsons is present-day sociology because (1) he is the premier functionalist, and (2) some eighty percent of a sample of 3400 sociologists polled by Gouldner agreed with the statement "functional analysis and theory still retain great value for contemporary sociology" (p. 168). Both aspects of this rationale are open to question. In the first place, Parsons is not a representative functionalist. And more importantly, one wonders what proportion of sociologists would say the same for Marxism, ethnography, mathematical models, and survey research. Thus, Gouldner's assertion—that Parsons (not Otis Dudley Duncan, William Sewell, or Patrick Moynihan, for example) represent the sociology of the present,—which is the crucial rationale for focussing on Parsons in his book is unsubstantiated. This is but one of many such poorly reasoned assertions in the book.

Perhaps it is mean to fault Gouldner for not offering a better justification for his focus on Parsons, since an examination of the ideological implications of grand theory is important. But such singular attention on this "habitual distortion" in a book on *The Coming Crisis of Western Sociology* does divert attention from

much else that is now more problematic for our discipline. Perhaps I am too much a devotee of the older Marx, but I don't see theory, values, etc. as all that central in human—even sociological—affairs. The crisis of sociology comes more from the constraints of the economic, technological, occupational, organizational, and political matrix in which sociology is produced. Mills, and Gouldner (in other of his works,) have readily recognized this.

Rather than underscore Gouldner's contributions to the diagnosis of sociology's ills, I would like to focus on his proposed method of cure, for it is, like that of Mills, poorly articulated and thus most deserving of serious attention. Mills proposed a sort of structural analysis: the "sociological imagination," which "is the capacity to range from the most impersonal and remote transformations to the most intimate features of the human self—and to see the relations between the two." Gouldner's proposed Reflexive Sociology is much the same, but focusses even more than did Mills on *self-awareness*. As he says, "the historical mission of a Reflexive Sociology as I conceive it . . . would be to *transform* the sociologist, to penetrate deeply into his daily life and work, enriching them with new sensitivities, and to raise the sociologist's self-awareness to a new historical level" (p. 489).

While the sociological imagination could make for a fatalistic and passive "understanding," Gouldner insists that Reflexive Sociology infuses one's entire life praxis, impelling one to actively engage the world. He concludes therefore that it is necessarily *radical* sociology. Reflexive Sociology is radical in the same sense that any holistic life commitment—whether to revolution, art, God, or nihilism—is radical. As defined, however, there is no necessary affinity between a Reflexive Sociology and the revolutionary *left* radicalism. In fact, Gouldner devotes several hundred pages to showing the *capitalist* praxis of Talcott Parsons.

What then makes for *good* praxis? The sociological imagination of Mills was linked to left-leaning radicalism by its grounding in Marx and turn-of-the-century classical sociology of Weber and Durkheim. Gouldner is not sanguine with this heritage. He links Marx to Stalin and classical sociology to a fatalistic metaphysical pathos which sees men as everywhere bound by the strictures of the organizations he has created.

Gouldner only informally suggests the means of transcending the fatalism of the man-bound-by-society view. Within his proposed voluntarism one should use self-awareness, accept the validity of one's own sentiments, and trust

one's own impulses as authentic. (Perhaps in this spirit Gouldner does not feel compelled to use footnotes. "If the substance and logic of what I say here does not convince, neither will the conventional rituals of scholarship" (p.viii).) That Gouldner does not fully accept the romantic, subjective, anti-intellectualism implied here is suggested by his often repeated suspicion of psychedelic-hippie tendencies sometimes associated with the New Left.

Gouldner's voluntarism seems ultimately to be rooted in a moral absolutism which is never stated explicitly as part of Reflexive Sociology. This moral stance is, however, often seen in his critique of others. He shows, for example, that the logic of utilitarianism has led to the rejection of all values. This same tendency is seen again in his critique of Goffman's dramaturgical model which treats only of external appearances, Garfinkel's ethnomethodology which only exposes folkways through a sociology of contrived happenings, and Homans' sociology of exchange which he describes ironically as "a no-nonsense tough-mindedness that wishes to accept the reality of social life without the allusions of morality" (p. 396). All of the approaches he criticizes are said to be cut off from history, which is presumably the source of moral values for Gouldner.

This brings us to the final question, how then is one rightly to interpret history? In other words, what is the *methodology* of Reflexive Sociology? Gouldner's answer, again, is much like that of Mills. Both have an overpowering distrust of conventional quantitative methodology; they have extended the truism that "method has value implications" to the ultimate position that "methodology is nothing but ideology." Mills places his faith in a return to individual craftsmanship modeled on the free entrepreneurial middle-class ideal which he eulogized in his study *White Collar*. Gouldner, too, stresses individual craftsmanship. While his picture is less completely sketched, his individual craftsman has a *geist* more akin to the Consciousness III than to the Consciousness I

of Charles A. Reich as defined in his recent *The Greening of America*. Perhaps I am a hapless troll of the Welfare State drugged on Consciousness II, but a return to the methodology of individual scholarship seems to be literally Quixotic.

Though Mills and Gouldner are not entirely hostile to the powerful technologies of contemporary sociology, they follow Don Quixote in a more fundamental sense. The heroic knight charged the windmill thinking it to be a dragon. Similarly, Mills and Gouldner, with their dualistic view of good and evil, see the enemy as a monolithic Leviathan—in Gouldner's usage, the "Welfare, Warfare State." Thus conceptualized, it is impossible to see the multitude of ways in which society is ranged against itself; it is impossible to see ways that it might be changed or humanized from within. And it is impossible to see the relevance of much of the data of conventional sociology in these processes. Rather, they must call for new knowledge and total apocalyptic change.

That change must be revolutionary to be real is perhaps the least examined and the most historically dubious domain assumption of Reflexive Sociology. Contradictory evidence can be seen in the rapid development of the sociology of sociology itself. In the single decade since the time when Mills seemed almost alone in his criticism of academic sociology, a host of researchers have become much more self-conscious about the relevance of sociology for men and society. The range of workers who pay serious attention to the ills of sociology can be seen, for example, in every issue of *The American Sociologist* and in the anthology of recent articles, *The Sociology of Sociology*, edited by Larry and Janice Reynolds. This new self-consciousness is in no small part due to the caustic and insightful writings of Alvin W. Gouldner. I for one hope that he does not now, as he says he intends to do, turn his attention back in time to nineteenth century Romanticism.

## REVIEW ESSAY

**THE LOGIC OF COMPARATIVE SOCIAL INQUIRY**, by Adam Przeworski and Henry Teune. New York: Wiley-Interscience, 1970. 153 pp. \$8.50.

**THE METHODOLOGY OF COMPARATIVE RESEARCH**, edited by Robert T. Holt and John E. Turner. New York: The Free Press, 1970. 419 pp. \$8.95.

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At least four overriding implications for contemporary social science may be gleaned from these two books. Written predominantly by political scientists, they indicate that recent developments in their discipline place a heavy emphasis on (1) mathematical models or formal axiomatic theory, (2) the logic of scientific explanation espoused by certain philosophers of science (Hempel, Nagel, and Carnap), (3) comparative research at the macro-level, and (4) a combination of behavioralism and functionalism, which has gripped the field.

Most of the contributors write as if they have just discovered axiomatic theory, the philosophy of science, paradigms, models, logic, and mathematics (*not* including statistics). The authors overstress these aspects in their attempt to refocus political science along the scientific images of physics or biology. They emphasize, for the most part, only the formal or theoretical side of science and neglect (except for an excellent chapter by Frederick Frey in the Holt and Turner book) the research and data analysis dimensions. In their stress on axiomatic theory and model building, the authors leave data analysis, measurement, and empirical study in the background—they pay lip-service to these, but that is all. This is a rather peculiar emphasis, given the professed nature of these books as methodological.

Can we infer that many political scientists have just discovered Hempel, Nagel, and Carnap? Have they just found out about axiomatic theory and formal systems? Have they just stumbled upon mathematical models? The book by Przeworski and Teune and some of the chapters in Holt and Turner's book lead me to believe that at least these writers and perhaps a fairly large number of political scientists have just moved into a stage that many sociologists

have been in for 10 to 20 years or more. (I do not mean to imply that it is an "advanced" stage or even a "desirable" stage.) If so, can we expect a partial negative reaction to set in against the utility of the philosophy of science and the heavy emphasis on model building?

If the development of science is a dialectic process deriving from the interaction between theory and data, then stressing one over the other is unwise. Theory designates the kinds of data to extract and helps to interpret the data. Data may modify or reject theory. By such mutual interaction both the nature of theory and the quality and kinds of data grow together. If so, the heavy emphasis on formal theory and logic is misspent, and *not* to the advantage of a young growing discipline. In this light, I am quite critical of the orientations in both books.

Given their emphases, it is perhaps best to view these books as expositions of theory construction techniques and strategies, as overviews of the formal structure of models, paradigms, and taxonomies, and as explications of the logic of explanation as it applies to problems in contemporary political science. In these terms, both books are rather vague and incomplete in presentation, but contain some useful ideas for cross-cultural inquiry.

Przeworski and Teune make the claim that the

... emphasis on theory and the de-emphasis on gathering and analyzing unstructured data is ... the important issue for social science in the 1970s. Although the "data revolution" is not yet over, the advent of formal theory will revolutionize social science anew (p. xi).

The authors remain true to this orientation by emphasizing logic and mathematical models, and by analyzing comparative cross-cultural research in terms of the logic of research design, levels of analysis, and formal aspects of measurement and equivalence.

Przeworski and Teune take the position that

the social sciences should be oriented to establishing general statements about social phenomena, rather than specific statements about particular social groups. Rather than state the nature of technological change in New Zealand and in the United States, for example, social scientists should try to designate the nature of such change in all industrialized countries. Instead of proper names (such as the names of countries), general theory requires variables. (The authors do not seem to recognize that proper names may qualify as a qualitative variable.) Although particular social units may be used as predictors of social phenomena, the authors maintain, the reasoning or rationale behind the prediction is not clear. Social units do not have the same status as variables. The substitution of variables leads into the explanation and interpretation of relations between phenomena.

The above is a valid point that is well documented by the authors. Changing the nature of predictive statements from proper names of particular social systems to variables does seem to lead to more accurate predictions and reasonable explanations. That water boils at different temperatures in New York and Denver—an illustration cited by the authors—does not lead to general laws if we merely describe the differences between these two states and predict by their proper names (i.e., the boiling point is lower or higher in one of the states). Only when we disregard the proper names and substitute such variables as atmospheric pressure and disregard other characteristics of the two states do we approach an explanation of the phenomenon.

A beneficial by-product of switching from proper names to variables is that this often means stressing quantitative over qualitative distinctions. Proper names, at best, provide nominal or categorical distinctions, which are generally insensitive to analysis; and for which few adequate data analysis techniques exist. The emphasis on variables appears to facilitate quantification and the use of powerful, sensitive, and versatile techniques.

Besides stressing the general nature of theory and the use of variables rather than proper names, Przeworski and Teune maintain that observations must be interpreted within the context of specific systems or countries. They feel, consequently, that measurement statements must take into account the cultural context and the individual meanings that phenomena have for a population. Adequate theory and intimate knowledge of a particular culture are crucial for establishing equivalence, which is necessary for meaningful measurement of general or system

variables. Voting or party membership, to illustrate, may be adequate indicators or measures of political participation in some countries like the United States and Canada, but may be meaningless in countries like Yugoslavia or China or in nonliterate groups. For the latter, in place of voting behavior or party membership, perhaps threats of a tribal leader, spreading rumors, strikes, or other forms of personal influence would be more adequate measures of political participation.

In addition to knowledge of the context of specific systems, the authors maintain that established theory is necessary for deriving adequate equivalence statements that apply across cultures. Equivalence statements such as questionnaire items, furthermore, provide the necessary data to establish theories. If accurate equivalence statements depend on theory and such statements provide data to test theory, then we are in a bind. We must have the theory to know that equivalence has been achieved and we must achieve equivalence to establish theory. As maintained previously, adequate theory and data develop together dialectically; if one does not develop, then both do not. Overemphasizing theory and neglecting problems of obtaining adequate data, consequently, is detrimental to achieving equivalence.

The presentation by Przeworski and Teune could have been improved on a number of points. They did not adequately tie their notions of explanation to cross-cultural analysis and measurement. It was difficult to understand, furthermore, why they failed even to mention statistical control as a partial solution to help establish causes (Chapter 2). They only mention control by design, which can be difficult to achieve in cross-cultural research. Several statements, moreover, appear to be quite naive; for example, they claim that *all* research involves defining a population and selecting a sample (p. 31). They also claim that if two variables are positively related in several countries, then differences among the countries are unimportant in explaining the dependent variable (p. 35). This assertion reflects a naive notion about correlation. To make their assertion, the authors must assume that all other variables are controlled or their effects are randomized in some way. If this assumption is not met, the correlation coefficient can be the same in two different countries, but it may reflect a causal relation in only one. Repressive variables may be operating in one of the countries. The correlation coefficient reflects both the true nature of the relation between variables *and* the degree to which other variables have been controlled.

With their rather extensive presentation of regression and the emphasis on mathematical models, it is surprising that the authors do not mention path analysis (which currently seems to be running rampant in sociology). Could this be another stage that some political scientists will reach in the not-too-distant future?

The strongest points in Przeworski and Teune's book are (1) their selection and use of examples to help get across their major points, and (2) their detailed and helpful discussion of measurement and equivalence as they apply to cross-cultural comparative research (Chapters 5 and 6).

The reader edited by Holt and Turner contains eight original essays that grew out of a seminar in comparative methodology in political science at the University of Minnesota in 1966. The first six essays are by political scientists and the last two are by a linguist and an historian.

Chapter 1 is a general introduction by the two editors, stressing their general ideas regarding explanation, comparative research, axiomatic theory, the nature of theory structure and theory construction, sampling, and operationalism. Their presentation is kept at a fairly general level, and they simply do not discuss their major points in any depth. Their major concern seems to be that rules of interpretation and criteria for admissible explanation are crucial forerunners of adequate comparative study.

In Chapter 2, Holt and Jon M. Richardson, Jr., critically lay out the nature of a paradigm (which they equate with theory, but is more an orientation or approach). A paradigm, according to the authors, contains a conceptual element, a theoretical element, rules of interpretation, identification of the correct puzzles to solve, criteria for determining puzzle solutions, and an ontological-predictive element (which specifies the nature of conceptual and theoretical elements in a fully articulated paradigm).

On the basis of these criteria, they critically evaluate structural-functional analysis, systems analysis, and psychological approaches. They also evaluate some atheoretical approaches and even include a section on factor analysis, which simply does not belong in the chapter. They conclude that none of the paradigms come up to the stipulated criteria. All of the paradigms, they argue, have virtually no deductive power; and although one or two may be conceptually rich, they do not measure up on other criteria.

Holt and Richardson present a rather clear synoptic view of the major orientations in contemporary political science. The grand-scale ideas of several contemporary theorists (Almond,

Deutch, and Easton) are presented to illustrate several points. In contrast to this presentation, the chapter's weakest point revolves around the belabored emphasis on logical deductive theory as the ultimate by which all paradigms considered fall short.

After reading the first two chapters, I must ask the authors, "Is there no place for solid thought, modest hypotheses, and competent empirical study?"

The third chapter, by Fred W. Riggs, is entitled "The Comparison of Whole Political Systems." Do not let the title fool you. The chapter is little more than a mental exercise based on a taxonomy of politics and a proliferation of unfamiliar terms (acephaly, orthocephaly, syntonic, dyarchy). The taxonomy is based on "whole political systems" but the emphasis is on the peculiarly labeled concepts that completely diverted me from Riggs' comparative illustrations of existing polities. There is no attempt to measure the major concepts in the taxonomy of polities, although he does say that it needs to be operationalized. Because he leaves his problem in this state, I found the essay to be the most sterile in the book. His stated goals of the chapter, however, are important—to specify the relevance of the major governmental institutions for comparative politics and ascertain their relations to important variables.

Chapter 4, by Joseph LaPalombara, is a solid critique of whole-systems analysis and grandiose analytical schemes. LaPalombara is quite critical of such efforts and maintains that

... it is precisely in the area of concern and writing about whole political systems that we now find the greatest confusion, the most dizzying array of typologies of obscure utility, the most striking examples of historicism, unilinear notions of systemic development, cultural parochialism, lack of genuine concern with how one gets logically from broad theoretical formulations to indicated empirical research, and, if I may suggest it, a contemporary variety of scholasticism that masquerades as *systemic theory* (p. 126).

In place of whole-systems analysis, he advocates a segmental approach emphasizing decision-making, middle-range theory, and scientific evidence.

The fifth chapter, by David E. Apter, is essentially another attempt to develop a grand formal axiomatic system and vague typology, similar to those that characterize most of the first part of the book. Apter's model attempts to account for political development and modernization, and is based on a rather loose combination of behavioralism and information theory. He maintains that coercion and information are the crucial aspects in accounting for change in

political systems. His theory is complex, difficult to follow, and not operational; and his distinctions between industrialization and modernization may defy observation. The vague nature of the theory is exemplified in his general hypothesis:

As modernization grows in a system, the greater the complexity of differentiation in stratification-group competition, the more quickly a political system-type will reach its ceiling of effective response, and the greater will be the need for coercion.

Apter may have a genuine contribution in his theory if he can clarify it conceptually, operationalize its key concepts, and subject it to empirical test.

Frederick W. Frey's very long chapter (Ch. 6) on cross-cultural survey research is first-rate. He gives a worthwhile overall summary of the utility and problems of survey methods in comparative research. I recommend the chapter to any sociologist contemplating research outside the confines of the United States. Frey gives a detailed presentation of a variety of topics, including conceptualization designed to minimize culture-bound concepts, types of research designs, organization of large-scale, cross-national investigations, sampling of countries, and equivalence (in both the sampling of units and in interrogation procedures). He gives an exceptional and critical review of projective techniques (Rorschach Test, TAT, Sentence Completion Test, etc.), the Semantic Differential, and the Self-Anchoring Scale to establish interrogative equivalence cross-culturally. He points out their possible utility as well as their culture-bound nature.

Frey states five reasons for engaging in cross-cultural survey research: (1) it increases the range of variation on relevant variables in the natural setting, (2) it is the only way to test hypotheses containing variables at the national level, (3) it provides a baseline of data for evaluating political change, (4) it provides a broader perspective to evaluate any particular political system, and (5) the exaggerated problems in such surveys heightens a researcher's sensitivity to similar problems in single-culture studies.

Dell Hymes essay entitled "Linguistic Aspects of Comparative Political Research" (Chapter 7) provides some information on language that could be useful when ascertaining cross-cultural data. It is a good introductory chapter to the many problems and pitfalls in translating

questionnaire items from one language to another and trying to maintain equivalence. His section on the problems of interpreting linguistic traits in terms of actual thought and behavior (pp. 299-300) is exceptional. He stresses the point that the nuances of language and the social norms of a culture are necessary knowledge before an investigation begins. In fact, he maintains that the questionnaire and formal interview are not universal forms of data gathering, because they may be unintelligible or interpreted negatively by a given group. His presentation includes many well-selected examples.

The final chapter, by Sylvia L. Thrupp, is not really worthwhile. Addressing her thoughts to diachronic methods in cross-cultural study, she suggests as a historian that researchers should go as far back in time as possible in making comparisons. Few would argue with the desirability of such historical comparisons. Thrupp does not, however, discuss the crucial problems of reliability and validity of such information, and she does not detail good historical sources.

Except for three chapters in the Holt and Turner reader (LaPalombara, Frey, and Hymes), I take a rather dim view of the content of these books, and do not recommend them for use in methods classes in sociology. This conclusion does not apply to the three chapters just mentioned, which provide useful information for cross-cultural research. The other material in Holt and Turner and the book by Przeworski and Teune are not true to their titles of social inquiry and comparative research, which connotes a greater emphasis on research methods and data analysis. These authors emphasize formal axiomatic theory, model building, taxonomies, and paradigms.

Rather than making the field of political science more scientific, they are likely to make it a sterile theorizing dead end. Theory, measurement, and data develop together; each supports the other. Demanding too much rigor in a "young" discipline is likely to kill off its fruitful ideas, and lead it to an esoteric and incomprehensible stagnation. By playing more loose and relaxed, and basing measurement and data interpretation on modest theories or rationales, by developing theories consistent with the data, and by dialectic interaction between theory and data, a discipline seems to have a more realistic chance of growing.

## BOOK REVIEWS

*Symbolic Interactionism: Perspective and Method*, by HERBERT BLUMER. Englewood Cliffs, N. J.: Prentice-Hall, 1969. 208 pp. \$5.95.

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It has been remarked that sociology is "what everybody knows in words nobody can understand." Herbert Blumer's collection of essays could be characterized as "what everybody *should* know in words *everybody* can understand." It is a lucid, straightforward statement of ideas that have been very important in the development of American sociology. It presents some general tenets that have already entered the sociological consciousness, as Blumer's many students have refined them and used them in their work. The bulk of the book consists of articles published over the last few decades, but to these Blumer has added a new 60-page essay on the methodological perspective of symbolic interactionism. The other essays include examinations of George Herbert Mead's social thought, *The Polish Peasant*, group life, mass media studies, and public opinion polls. These essays are important for the history of sociology, as well as for contemporary theory.

Blumer defines symbolic interactionism as a perspective that seeks explanation for social life in the way in which participants define and interpret the situations they confront; collective activities, in this view, are formed through an on-going process of designation and definition that is continually modified by specific people as they adjust their actions to one another. Human consciousness, the internal process of interpretation, is considered central for (1) the development of the self (through self-interpretation, as explored in the essay on Mead); (2) social interaction (a process of "formative transaction" in which norms, statuses, and roles are only frameworks for interpretation and mutual adjustment); (3) the relations between social variables (such that human consciousness intervenes, for example, between two demographic variables); and (4) scientific methodology itself (for here the scientist may impose his own definitions on the empirical world as he engages in the process of interpreting it). From this view of the centrality of conscious definitional process in human group life, Blumer derives a statement of what to study

and how to study it. The symbolic interactionist should study social or group process; how actions are formed and reformed as the history of interactions proceeds—the process rather than "causes" or antecedent conditions. And he should do it via naturalistic observation, direct examination of the empirical world itself, with the scholar attempting to discover those categories and ideas that the actors themselves use as they choose and modify lines of behavior. Blumer criticizes public opinion polls, for example, for failing to explore the *processes* by which opinions are formed and the social niches of individual respondents in that portion of the social structure where these processes of formation are occurring.

The new introductory essay presents such principles and tries briefly to distinguish symbolic interactionism from other approaches which deny the directing role of interpretive processes in social action. The book is very general here, for it gives little detail on the other perspectives nor indicates with concrete examples how symbolic interactionism would differentially interpret the same situation. "[The] dominant view . . . ascribes human action . . . to such matters as motives, attitudes, need-dispositions, unconscious complexes, stimuli configurations, status demands, role requirements, and situational demands. . . . Such an approach ignores and makes no place for the process of self-interaction through which the individual . . . constructs his action . . . such matters as his wishes and wants, his objectives, the available means for their achievement, the actions and anticipated actions of others, his image of himself, and the likely result of a given line of action." That social action is purposeful and flexible rather than automatically elicited by organic drives, situational stimuli, or societal organization incorporated into the person comes through loud and clear; but symbolic interactionism could confront its explanatory competitors with a bit more force. It might have been helpful here for Blumer to have cited chapter and verse on the other perspectives, to have named names, to have provided examples of their explanations in contrast to his in much more detail. How exactly, in the above instance, do "wishes and wants" differ from "motives"; "anticipated actions of others" from "situational demands"? This kind of helpful and often necessary detail Blumer does not

provide. In fact, the book in general is philosophical and literate in the Median tradition rather than scholarly or empirically grounded or based on real-world examples of behavior. I noted no citations, bibliographical items, or references to research findings (except for a few studies that were the subject of essays). This might make the book a readable series of philosophic statements very important in the history of American sociology, but some readers may have to stretch at times to see its applicability to the vast social scientific enterprise of the moment. In other places the book argues for the formulation of concepts without giving any, promotes naturalistic observation as a method without suggesting many concrete steps that can be taken, and in general gives few examples of symbolic interactionism *in use*.

Yet at the same time I think Blumer's ideas have a great deal of relevance, not only as a perspective for sociology, but also to the world "out there." Applied behavioral science, and specifically the t-group movement, is based often on an examination of group process and on social change through exploring and modifying people's interpretations of situations. Students, blacks, and women are discovering that society and societal norms are continually formed and reformed, that human consciousness intervenes in social organization, and that persons rather than roles behave—this is happening not through books, but through action. Within sociology, too, a growing literature of empirical studies based on symbolic interactionist premises and recent developments such as the emerging field of ethnomethodology make Blumer's book an important orienting statement. It is a loss that Blumer did not comment on such developments in both society and sociology, because the perspective of symbolic interactionism as he presents it has so much potential as an integrating philosophy for all of them. And the perspective still stands as a valuable argument against narrowly-conceived structural-functionalism, against the reification of such concepts as status and role, against a static rather than a dynamic view of society, and against "mindless" empiricism in which the researchers take neither their own minds nor those of their subjects into account.

As a clear and well-written compilation of the basic thought of an influential sociologist, *Symbolic Interactionism* is interesting. As a foundation for sociology students who have not yet grasped the notion that men interpret and define their situations and act accordingly, it is valuable. As a conscience for those sociologists who may occasionally forget about con-

sciousness and its implications for their work, it is also potentially useful. But it must be viewed appropriately, as a historical document and an orienting statement, rather than a new contribution to the sociological literature.

*Marshall, Marx and Modern Times: The Multi-Dimensional Society*, by CLARK KERR. New York: Cambridge University Press, 1969. 138 pp. \$4.95.

ROBERT W. AVERY  
*University of Pittsburgh*

In this book, the Marshall Lectures of 1968, Clark Kerr considers the 19th century worlds of Marshall and Marx and then examines the modern industrial world against the interpretations and the predictions which these men made. Since neither foresaw very well what mid-20th century capitalism would look like, Kerr uses the second half of his book to develop his own characterization of contemporary American society. Typing it is "pluralistic industrialism," he depicts an inner society, anchored to the status quo and its possibilities, and an underclass, excluded from participation and its rewards.

Organizing his early chapters around four topics on which his subjects expressed themselves, Kerr finds (1) that both Marshall and Marx looked forward to a classless society and a more perfect man, though Marshall anticipated evolution as against Marx's revolution; (2) that Marshall was too certain that capitalism had a steadily improving future (he underestimated the effects of unemployment and the excesses of the business cycle), while Marx was too convinced that capitalism could not survive its own contradictions (he failed to see that exploitation is caused not by capitalism but by industrialism); (3) that Marshall mingled the hope and the expectation that class lines would dissolve in the face of the obvious advantages which awaited cooperation between capital and labor, in contrast to Marx's precisely opposite prediction; and (4) that Marshall foresaw with considerable accuracy how trade unions could both contribute to and inhibit the operation of the free market, while Marx regarded them mainly as temporary impediments. This summary skims across the surface of Kerr's succinct and pointed comparative study, which has the particular virtue of drawing out the respects in which Marshall was an analyst, not simply of the marketplace but of some of its institutional surroundings as well.

In the second half of the book Kerr portrays pluralistic industrialism and its American reali-



ant, managerial pluralism. It contains various public and private governments at its inner core, thus insuring constant friction and occasional conflict in the center; and it presents the problem of how to enlarge the inner society so as to include the underclass.

If Kerr's characterization of American society seems less convincing than his instructive comparisons of Marshall and Marx, it may be due to the abundance, perhaps even surfeit, of recent accounts of what American society is. To choose intelligently among them we need to have more information from authors about the theories which inspire their descriptions and the methods which lead them to their different conclusions. Kerr did not have the time in his lectures at Cambridge to undertake this part of the job, nor would it have been a suitable occasion. He prepared his words, as he says, for a heterogeneous audience. If one result was that technical questions were omitted, another was that he could speak and write in a style which is elegant in its simplicity.

*General Theory: Social, Political, Economic, and Regional*, by WALTER ISARD, in association with TONY E. SMITH, PETER ISARD, TZE HSIUNG TUNG, and MICHAEL DACEY. Cambridge, Mass.: The MIT Press, 1959. 1040 pp. \$10.00.

PHILLIP BONACICH  
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This book is an attempt by regional scientists to formulate a general theory of social systems. It contains the lectures of the senior author in a graduate course in the Department of Regional Science at the University of Pennsylvania.

Regional science is an integrative social science discipline concerned with all aspects of the role of location and space in human society ("Regional Science," *International Encyclopedia of the Social Sciences* (Vol. 13, pp. 382-390)). It is closely related to economics (regional scientists study location and spatial configuration of industry, land-use patterns, and interregional and international trade, among other things); but regional scientists are distinguished by a recognition that location and spatial relations have important non-economic political and social causes and consequences.

There are chapters on individual and group decision-making models that have been developed principally by economists, reviews of the empirical and theoretical and empirical work on mixed-motive games and coalition formation, a summary of some Parsonian theory, a dis-

cussion of the mathematical models of small groups developed by Herbert Simon, and a description of "a general social, political, and economic equilibrium theory." This is only a partial listing of the contents.

The general theory that is intended to unify these diverse topics is essentially an extension of economic ideas and models. A discussion of non-economic commodities could be of particular interest to sociologists (Chapter 12). Various qualities of groups and relationships (including solidarity, affection, and power) are analyzed in terms of how they resemble or do not resemble economic commodities. How transferable are they from one individual to another? Do they have supply and demand curves? This interesting path is not adequately explored in the book, perhaps because of the authors' limited knowledge of sociology. For example, despite the fact that the main theoretical position of the book is to see human behavior as exchange, they show no familiarity with the sociological brand of exchange theory developed by Homans and Blau.

Although the authors intended to provide an integrated theory of society, the quasi-economic model that is meant to unify the topics covered in the book is woefully inadequate to the demands that are put on it. Sociologists will probably find that the separate reviews of decision theories, bargaining models, coalition theory, and mathematical models of small groups are the most valuable parts of the book.

*The Discovery of Grounded Theory: Strategies for Qualitative Research*, Barney G. Glaser and Anselm L. Strauss. Chicago, Ill.: Aldine Publishing Co., 1967. 271 pp. \$6.75.

JOHN C. SCOTT  
*American University*

Glaser and Strauss maintain that the sociological mandate may be divided into two major concerns: the verification and the generation of theory. Although sociologists must attend both obligations to provide satisfactorily for the total enterprise, the authors have opted for the consideration of theory generation in this book, even as they have chosen to accent this orientation in their own research. More particularly, they are concerned with developing grounded theory, i.e., theory derived from an exploration of and an intimate familiarity with data. Grounded theory is distinguished, thus, from theory based merely on insight or caprice, from theory developed deductively, as well as

from theory established systematically through the testing of hypotheses.

It is in the process of generalizing their grounded theory that one sees more clearly Glaser and Strauss' distinction between their concern and that of some others. The generalization of their theory necessarily involves a purposeful process of inquiry in which comparative analysis has a crucial function. As outstanding examples of this approach, they offer the development of Weber's theory of bureaucracy and Durkheim's theory of suicide. Obviously this approach differs from the purely fanciful unfolding of theory or the creating of logico-deductive theory, "such as some well-known ones on the 'social system' and on 'social action.'" These, the authors suggest, "can lead their followers far astray in trying to advance sociology . . . and also in trying to advance their personal careers, for one cannot empirically dissociate the need to generate theory from the need to advance careers in sociology." (How's that for *grounded* theory?)

Although they grant that the formal testing of hypotheses may reveal some new theories, this is not the essential character of the process. The concern with verification discourages the generation of "a more rounded and more dense" theory. Furthermore, they insist, verification has misled and discourages many young and potential sociologists. The authors state that too much attention has been directed to the testing of theory. They assert that young sociologists have been contaminated (in the past at least) with the belief that they function as sociologists only to the degree that they are involved in verification. This, they feel, is very sad. Verification, while necessary, is not the only legitimate activity of sociologists, young or old. It is not of primary interest here nor is it generally their cup of tea.

Nevertheless, Strauss and Glaser are not "anti-quantitative." Quite to the contrary, they are delighted to have the quantitative researchers around to do the dirty work or the systematic testing of hypotheses while they get on with their "thing." If they are anti-anything, it is the "ungrounded" logico-deductive theorizing of the you-know-who type. The contribution of the later approach is minimized as compared to their own brand of theorizing; and, as for the verification of theory, they gladly leave that to the quantitatively orientated researchers who have not yet been caught up in the fascinating game of theory discovery.

One of the rewards of reading this book is the restatement of the persistent theme that research of the kind Glaser and Strauss advocate is a thrilling, creative thing. It is not the

least bit ignoble or tedious. The authors successfully transmit the sense of adventure, the air of excitement and of positive apprehension over what is discovered as one tracks down clues and sorts among attractive alternatives. The approach should appeal to many youngsters in the field as an alternative to some of the newer sociologies: "grounded theory" encourages field work, accents qualitative sensitivity, and implies that it has greater immediate practical application because it is closer to the data. Furthermore, its devotees do not have to worry too much about mathematics and statistics.

*Quantitative Ecological Analysis in the Social Sciences*, edited by Mattei Dogan and Stein Rokkan. Cambridge, Mass. The MIT Press, 1969. 607 pp. \$25.00.

ROBERT M. HAUSER  
*University of Wisconsin*

In 1966 the International Social Science Council sponsored a Symposium on Quantitative Ecological Analysis at Evian, France. This book is the end-product of that meeting, though about half the papers were not presented at Evian. Despite the term "ecological" in the title, none of the papers has any connection with the recent rediscovery of air pollution or, as one of my learned colleagues describes it, "gum-wrapper ecology." Nor, less fortunately, was the symposium informed by the concept of human ecology as a distinctive frame of reference for the study of social organization. The usage here derives from W. S. Robinson's unfortunate neologism of twenty years back. His characterization of correlations among aggregate data as "ecological" has been carried one logical step backward with the identification of any old areal datum as "ecological." In short, the volume focuses on the use of areal data; and, making the scope still narrower, most of the contributions deal with the explanation of political behavior.

Dogan and Rokkan identify four purposes of the symposium (p. 12): "(1) to provide an opportunity to discuss in some detail experiences in the planning, organization and operation of ecological data archives and to examine plans for joint international action to ensure accelerated development of such facilities; (2) to insure effective confrontations of differing substantive emphases and methodological traditions in the use of quantitative data for localities and to examine the pros and cons of alternative designs of analysis; (3) to review the possibilities of joint strategies at several levels of aggrega-

tion, particularly the possibilities of combining sample surveys of variations at the level of the individual with ecological analyses of the proximal contexts of such variation; and (4) to examine recent experiences in the development of ecological analyses in historical depth, particularly through the organization of data files for local and regional variations before the decisive breakthrough to economic growth and during the subsequent processes of urbanization, mobilization and modernization." The first and fourth objectives are given only cursory treatment in the published papers. Only a couple of efforts at time-series analysis are presented; and reports on archival work in Great Britain, Germany, Austria, Latin America, and the United States suggest that the payoff from such file acquisition will be long in coming. This may be all to the good if the remaining contributions, which deal mainly with the second and third objectives, are indicative of what will be done with ecological data archives.

The editors offer a bit of intellectual history which is suggestive of the spirit in which the volume was prepared. The introduction of third-generation computers, they say, has generated previously non-existent possibilities for inter-areal and inter-temporal linkage of data files and for sophisticated methods of data analysis: "correlation matrices, regression and covariance analysis, factor analysis, causal path analysis, calculation of systems of simultaneous equations in the econometric style" (p. viii). Despite the questionable empirical status of this claim, it may help to account for the quality of the papers, several of which display a pathological mixture of technological complexity with incredible methodological naivete and an almost perfect disregard for sociological substance. One might rationalize these pieces as "computer calisthenics," or "planned exercise using the available configurations to acquire new habits of inquiry," in the words of Marvick and Bayes ("Domains and Universes: Problems in Concerted Use of Multiple Data Files of Social Science Inquiries"), had not someone seen fit to preserve them in published form. Their paper is a case in point. After describing a data file for Los Angeles which merges census and electoral data aggregated to the tract level, they try to illustrate the utility of the merged files in conjunction with special purpose surveys. They classify a sample of 67 census tracts in which 700 interviews were also collected using differences between total rates of voter turnout in Presidential and Congressional elections. Lo and behold, the survey respondents in tracts with large turnout differences ("vacillating domains") are less active politically than those in tracts

with small turnout differences ("steady-state domain"). Do we need this sort of hand-waving to be convinced that people whose level of political interest is low are more likely to vote in Presidential than Congressional elections?

"Computer calisthenics" were also much in evidence in papers by Dögan, Capecchi and Galli, and Cox. Dögan throws away most of his data in order to produce an orthogonal design matrix for covariance analyses of effects of region, industrialization, social class, and "the religious factor" (an operationally undefined covariate) on left voting in the rural cantons of France. He seems to think F-ratios are measures of association. Capecchi and Galli produce no less than 49 diagrams of "causal linear circuits" of great complexity which purport to account for areal variation in left voting in Italy. Apparently their numerical entries are zero order correlation coefficients, and there is no substantive discussion of the structure of their models. All of this is presumably inspired by their reading of Boudon (p. 247): "With this type of analysis we can generally determine whether variables have a direct or indirect influence, and the causal relations by which these influences are determined. In other words, given a dependent variable, which in this case is the vote, we can identify the variables that influence it and determine whether these influences are direct or indirect." Similarly, after going through the motions of a factor analysis to choose independent variables, Cox produces two "causal models" of voter turnout and Conservative vote in the London conurbation and proceeds to "test" them by looking at inappropriate partial correlations without ever having discussed the structure of either model in substantive terms.

Several papers, both methodological and empirical, follow the Columbia tradition of ignoring the insight into statistical design and interpretation offered by covariance analysis. Instead, we have the usual unexamined assumptions. Levels of analysis are isomorphic with levels of aggregation. Selectivity among units is negligible, while association of individuals within units is random. Hence, aggregation effects are very sociological. Blessed are propositions relating aggregate to individual variates, for they cut across levels of analysis. The potential of these ideas for extensive verbalization and statistical artifact are demonstrated by Allardt ("Aggregate Analysis: the Problem of its Informative Value"), in a second paper by Cox ("The Spatial Structure of Information Flow and Partisan Attitudes"), and by Segal and Meyer ("The Social Context of Political Partisanship"), as well as by Marvick and Bayes. On the positive side, the awkward effort by Valkonen ("Individual

and Structural Effects in Ecological Research") makes some use of the linear model, while Alker ("A Typology of Ecological Fallacies") demonstrates the variety of covariance terms generated by a classification of pairs of individual variates by time and place. Scheuch ("Social Context and Individual Behavior") advocates measuring variables at the level of aggregation appropriate to our notions about social process, and Laponce, in a modest but competent comparison of Canadian census and survey data on ethnicity, religion, and party preference, was appropriately casual about the whole business (pp. 187-188): "Whatever difference there may be between the use of survey and of census data will come from the definition of our units of observation and analysis, of our definition of what is container and what is content, of where the fish begins and the fishbowl ends."

One fruitful "confrontation" of the sort presumably envisioned by the editors is the juxtaposition of papers by Philip Converse and by MacRae and Meldrum on the use of aggregate voting data to detect "critical elections." MacRae and Meldrum analyze residuals from expected percentages voting Democratic in Illinois counties in a two-way design of county by election year. Then, they extract a first principal component from the residual matrix and try to interpret the time series of unstandardized loadings. My suspicion is they are examining intertemporal heteroscedasticity in the residuals. In a far more straightforward effort, Converse fits a causal chain model with constant inter-period coefficients to electoral data for France and the U.S. and classifies as "critical" those years for which the inter-annual correlations produced by the model are consistently too large. His paper deserves to be read.

On the whole, the volume is a major disappointment, largely because of the failure to exploit the promised insights from regional science and econometrics. And the price is outrageous.

*Stress and Response in Fieldwork*, edited by FRANCIS HENRY AND SATISH SABERWAL. New York: Holt, Rinehart and Winston, 1969. 77 pp. Paperbound. \$1.95.

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This book consists of four short essays in which fieldworkers describe their responses in various settings, and a fifth essay by a psychiatrist who discusses these and other situations of reaction to stress. The first paper (Hans C. Buechler) considers a Bolivian peasant commu-

nity; the second (Peter C. W. Gutkind) treats unemployed Nigerian men in Lagos. Frances Henry contributes the third article, concentrating on three different field settings in Trinidad; and the essay by Satish Saberwal is set among the post-Mau Mau Embu of Kenya. As the editors point out, all these studies were carried out in contemporary field situations rather than in the remote tribal communities examined by anthropologists of an earlier time; thus, the works reflect some of the realities which young anthropologists can expect to encounter increasingly as isolated, unacculturated societies become a matter of history.

Fieldwork is difficult to do and even more difficult to teach to someone else in the classroom; the literature on process and problems of fieldwork is not vast, and the provision of a series of well-documented, well-analyzed detailed cases can be a valuable teaching tool. These studies are very brief and in general more discursive than analytic. The integration between them is rather meager, provided primarily by the Introduction. Some common problems recur in the various field settings: the establishment of rapport, identification of oneself and one's place in the society, location of critical informants, tactical complications of alliances with factions and attendant loss of access to others, and questions in regard to the extent and type of one's often conflicting commitments—to personal principles, professional goals, local elites, those sponsoring the investigation, and so forth.

But in spite of these common problems, the individual fieldworker's responses and solutions are quite distinct and unique, determined by the differing circumstances and personalities of the people involved. While it is comforting to know that everyone can expect these problems (and many more), and helpful to realize that others too have been uncertain and anxious and have blundered and guessed, the concrete and practical guidelines which students might look for are, and perhaps must be, missing from this book.

More than most social scientists, the anthropologist uses himself as an instrument, operating in the delicate areas of the unknown as he begins to explore matters which led him to identify ultimately the valid questions in his area of investigation. Such efforts generally precede the more rigorous and precisely defined studies which can be conducted only after the fundamental assumptions, categories, vocabulary, and variables have been identified. Usually this means that the anthropologist is a stranger in the field, working alone and without the precise guidelines and specific tools available to other

investigators. And certainly this produces stresses more peculiar and extreme than those encountered by scientists operating in a relatively known and familiar world. Self-awareness and fortitude are indispensable for effective field work, and if this volume is used to assist students in achieving an understanding of their own psychological responses, strengths, limitations, and talents in the beginning stages of fieldwork, it may indeed be a useful teaching aid.

*Discrete Distributions: Distributions in Statistics*, by NORMAN L. JOHNSON and SAMUEL KATZ. Boston, Mass.: Houghton Mifflin Co., 1969. 328 pp. \$12.50.

NEIL W. HENRY  
Cornell University

This book is the first of a three-volume series; the two succeeding volumes will deal with continuous distributions. Together they will surely provide a valuable reference source for statisticians and researchers in many fields. Here the distributions discussed at chapter length include the Binomial, the Poisson, the Negative Binomial, the Hypergeometric, and the Logarithmic Series. Other chapter headings are "Some Compound, Generalized and Modified Discrete Distributions," "Contagious Distributions," "Some Miscellaneous Discrete Distributions (Univariate)," and "Multivariate Discrete Distributions." The first two chapters introduce the formidable (but necessary) notation and briefly outline some of the basic concepts of probability and statistics.

The format of the presentation is particularly well chosen. Each chapter includes some historical remarks on the origins of the distribution and a concise discussion of its applications. More formal sections give the formulas for generating functions, moments, parameter estimation, and the like. Approximations to the distribution are also discussed, with tables and graphs included wherever appropriate for an understanding of the characteristics of the distributions. Extensive tables are not included, but the references to sources of tables are complete. The style is compact, necessary of course when packing so much information into 300-odd pages; and the density of formulas is overpowering at times. The division into short, clearly titled subsections, however, permits the reader to find specific information fairly easily. I suspect that most references to the book will be to check on methods of estimating parameters and finding approximations and tables for the better known distributions.

Probably very few sociologists will use this

book, but it is certainly a necessary reference for those mathematical sociologists and statisticians whose work involves probability models. The chapters on the negative binomial, the Poisson, and the contagious distributions, will be particularly useful in conjunction with standard textbooks such as Feller, Parzen, and Coleman's *Mathematical Sociology*.

*Diffusion Dynamics: A Review and Revision of the Quantitative Theory of the Spatial Diffusion of Innovation*, by LAWRENCE BROWN. Lund, Sweden: C.W.K. Gleerup, 1968. 94 pp. Paperbound. 20 Sw. Cr.

M. HAMIT FISEK  
Stanford University

I would like to commend this monograph to the attention not only of those people who are interested in the process of innovation diffusion, but also of those who are interested in questions of theory construction and philosophy of science. The quantitative theory of the diffusion of innovation has two features which are of special interest from the meta-theoretic point of view. In the first place, the substantive theory and the mathematical model have been developed simultaneously so that they can be viewed as a well-integrated whole. This, of course, is in direct contrast to most quantitative work in the social sciences, where the mathematical model is usually a superstructure imposed upon the substantive theory with little regard for its appropriateness. In the second place, the theory is one of the few instances where a social scientist has actually attempted to do something which I believe most social scientists would consider highly desirable but few actually attempt: to construct a theory for macro-phenomena based on assumptions about individual behavior.

The monograph itself is organized along lines which make the process of theory formulation easy to follow. It would serve as a good illustration in a course on theory construction or formal models. The first section reviews the existing work on the quantitative theory of diffusion of innovation, mainly Hägerstrand's work, and related topics such as the random net models of Rapoport and epidemiological models. The review is not eclectic, but presents the important points and problems. The next section analyzes the shortcomings of the existing theory, and the following section presents a revised conceptualization and develops a new mathematical model. This section is very well organized and illustrates the process of model construction quite well. The following sections investigate the good-

ness of fit of the model to a particular set of data and offer a critique of the model.

The basic shortcoming of the book is that it is rather hard to read. Although the mathematics involved is straightforward and should be accessible to all who have some knowledge of probability theory, the prose tends to be cryptic in spots. Of course, this is a research monograph and not a text book, and we cannot ask for too much in the way of elaborate presentation.

One further point needs to be made. Quantitative work on diffusion of innovation has been carried out mostly by geographers and economists (Brown is also a geographer). This work makes it clear, I think, that sociologists have things to contribute—as well as things to gain—from the study of diffusion models. On the one hand, Brown's model could be modified fairly conveniently to take into account complexities of social structure. On the other hand, it could also be applied, as Brown points out, to the study of cultural and ideological innovations.

*Transforming America: Patterns of Social Change*, by RAYMOND W. MACK, New York: Random House, 1967. 199 pp. \$4.95.

GERALD M. SHATTUCK  
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The author surveys several basic social problems in relation to changes in American society, in the context of sweeping social changes occurring around the world because of the scientific revolution. Mack examines the undue growth of population; dysfunctional elements of the development of educational, political, and industrial institutions; unbalanced aspects of the American value system; and the phenomenon of often-misinterpreted nonconformity. The text is structured in such a way as to present a basic set of problems in each area, delineate these problems in terms of appropriate data, and outline crucial questions raised by each set of problems.

Mack has purposely aimed at "educated people who would like a broad overview of what social scientists know about current social trends." While it is obvious that the sociological knowledge base regarding social change in modern society can hardly be presented within the covers of a single text of less than 200 pages, nevertheless, Mack has judiciously selected illustrative evidence to support his descriptions of some significant American problems.

A thoughtful reader may encounter three

problems with the book. First, he may find the text almost simplistic—always a risk when one opts for breadth rather than depth—even though helped by annotated references. Second, in moving from a sociological definition of a given problem to a "crucial question for Americans" based upon the problem, Mack appears to allow his values and imagery occasionally to overcome his objectivity. An example is at the end of his discussion of the population explosion where he states, "We cannot survive indefinitely as hostages on an island of plenty in a sea of poverty." Or, "Two powerful ideologies oppose birth control: Marxism and Roman Catholicism." (This perhaps neglects other ideological factors which contribute to the population problem such as Latin "machismo"). Third, not only does the reader hope for some hint at an answer to the author's crucial questions, but also he begins to wonder if these really are the most crucial questions. The problems of drugs, crime, or war are not treated in detail if at all. The generation gap, the disrupted university, the many varieties of backlash, environmental pollution, the problem of an isolated aging population, the increasingly publicized problem of conflict and violence, do not appear in the text in a substantial way. (The publication date of the book may have something to do with its curiously timeless viewpoint.)

Despite its weaknesses, the book does raise basic questions in a well-defined and sometimes challenging way. It is not a book that safely avoids issues. It is refreshingly devoid of the indirect and guarded terminology of the standard sociological statement. Its potential readership may extend beyond the author's modest intent and could appropriately include an advanced high school social science readership or a college social problems class.

*Technological Growth and Social Change: Achieving Modernisation*, by STANLEY A. HETZLER. New York: Praeger Publishers, 1969. 302 pp. \$7.50.

DARIO MENANTEAU-HORTA  
*University of Minnesota*

In the midst of a rapidly and sometimes dramatically changing world, a new book on technological development and social change usually offers a wide target for criticism. Contemporary works on the subject range from rather conservative and non-committed descriptions of social evolution to the most radical interpretations of the dialectic process of mankind. This book by

Hetzler, however, belongs to a new perspective which may prove fruitful in achieving a better understanding of the processes, problems, and prospects of what constitutes the hope and drama of our time: social and technological development.

Hetzler's major thesis is that "a more meaningful hypothesis for technological development can be derived by drawing together social and technological data into a single analytical framework." Hetzler begins his work by warning the reader that the basic approach used in his book is what he calls "socio-technics"—that is, "a study of the total range of relationships between man and machine." He stresses the need for more awareness and a better understanding of that pervasive interplay and the interdependency between technological advances and societal transformations.

Part One presents a panoramic view of some of the major problems facing highly industrialized societies as a direct consequence of rapid technological growth. Material devices created by man to facilitate the achievement and satisfaction of individual and social needs are turning against mankind, fostering a growing suspicion, hostility, and alienation of the individual and mankind. While technology, according to the author, "forms the root system of the whole complex known as Western Civilization," this same material progress is weakening institutional arrangements and challenging the most precious values, the "creed" and the fiber of an industrially advanced society. Moreover, another consequence of the massive confusion growing out of technological progress seems to be the increasing gap between materially advanced societies and developing societies. Hetzler briefly lists eight of the most common stereotypes applied by "Westerners" to members of developing nations. Although the list of clichés only describes the surface of the problem of international relations, it at least calls attention to the need for interdependency between nations if technological development is to be beneficial to the world community. This assertion, rather than being utopic, appears to be implicit in Hetzler's concluding remarks when he postulates that "the social class system, the pricing system or, indeed, even the enterprise system, all of which are seen to be highly divergent by cultural area, must, in the growth of technology, ultimately give way to common technical methodologies and uniform social practices, facts that make it possible to prescribe planning principles which are applicable to the promotion of technical growth in the undeveloped and the materially advancing countries, alike" (pp. 293-294).

Part Two reviews some of the traditional hypotheses on development. In spite of the fact that Hetzler lumps together in about 50 pages of text what he terms the "old concepts" derived from economics, sociology, social psychology, anthropology, and political science, his sketchy presentation contains valid criticism of these theories of social and economic development. For a reader with little or no exposure to the literature of social change, these few pages may serve as a good introduction to the area as well as a friendly invitation to sociological reasoning.

In Part Three Hetzler deals with the "new perspectives" or, rather, his approach to development. Once again, his main concern is to establish a conceptual framework within which the interplay between technological progress and social change can be better analyzed. From a historical perspective he distinguishes four phases of productive activity: basic production, factory production, elaborative production, and automation. This evolutionary process of social systems in their productive functions is related to five specific stages of technological progression: (1) man-machine organization, (2) power technology, (3) transportation technology, (4) agricultural technology, and (5) general automation.

Part Four offers an interesting discussion of the impact of modern technology upon patterns of economic and social organization which, even in the most "materially advanced" societies, are lagging. In the author's words, "the movement toward automation seems to be an evolutionary process . . . and in its movement, it is upsetting all the traditional economic precepts of competition, ownership, management, and fiscal policy."

Some of Hetzler's views are questionable, e.g., his assertion that the "main objective of development is that of converting a traditional or transitional society into a society of technical innovators." From the perspective of a developing society facing the dilemma of internal disorganization, injustice, and misery for a large segment of her population, the primary objective of development seems to be a rapid and radical transformation of the social structure, a drastic change in the relations of power, and substantial redistribution of resources and opportunities. Without these changes one could question the meaning of technological progress and the extent to which technological innovation would become a self-sustaining process.

Although Hetzler does not offer easy solutions to the problems that he describes, his contribution is certainly valuable in terms of questioning conventional social organizational patterns and meeting his own challenge to "develop and apply new ways of thinking."

*Theory and Practice of Social Planning*, by ALFRED J. KAHN. New York: Russell Sage Foundation, 1969. 348 pp. \$8.75.

*Studies in Social Policy and Planning*, by ALFRED J. KAHN. New York: Russell Sage Foundation, 1969. 326 pp. \$8.75.

JIRI NEHNEVAJSA  
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In *Theory and Practice*, Kahn develops an orientation to planning which he defines as "policy formulation and realization" (p. 17). The idea of "social planning" encompasses not merely planning in the domain of welfare activities and social services, but also "social aspects" of physical, fiscal, or economic plans, and constitutes one necessary axis in "comprehensive planning."

Following an introductory chapter, the author discusses social planning in America, stressing that Americans do plan, notwithstanding ideological predilections. Chapter 3 deals with the definition of the planning task in terms of facts, projections, and inventories as these form the baseline for planning. In the final chapter the author notes that planning is not always appropriate nor always possible even when appropriate. For example, societies "must respond rapidly to emergencies, must give scope to impulse and to intuition" (p. 328), presumably one type of circumstance when planning might not be appropriate. Yet, emergencies with non-zero probability of occurrence and with high disutilities attached to the consequences are precisely one of the situations for which planning is of paramount importance. In any event, the problems and conditions under which planning is "inappropriate" or "unfeasible" might have been better outlined at the beginning of the volume.

The author provides a worthwhile list of planning steps: define the problem in detail, diagnose the causes, seek relevant theories, and estimate consequences of various possible intervention. This listing of tasks might well have constituted the key organizing principle of the volume, but it does not. Instead the discussion proceeds to deal briefly with such topics as statistical series, special studies and surveys, and projections. The reader is not told how the planner might use surveys and when the use of survey techniques might not be warranted. Chapter 4 stresses that "opinions, beliefs, values and choices are very much in the forefront in planning" (p. 97), without really addressing the question of how the planner would actually put

to use information on values and preferences. The author merely suggests several sources of data (e.g., market analyses and polls) and offers anecdotal illustrations. His own conception of "a total approach" suggests political and organizational principles (e.g., maximum decentralization, maximum information for all participants, democratization in decision-making), but not procedures and techniques to determine the effects of variable distributions of values, preferences, and opinions. Thus, Kahn pays the cost of having adopted a definition which equates planning with policy formulation instead of considering alternative plans as inputs into policy-making, and policy formulation as selection from among the various options.

Chapter 10 ("Programming Problems in Social Service Delivery") belongs for all practical purposes in the companion volume of *Studies*. The chapters in this volume examine in detail several major social programs, including the war on poverty, juvenile delinquency, income security, urban renewal, community psychiatry, and delivery of social services.

Although Kahn repeatedly mentions the importance of feedback and evaluative information, he devotes only four pages (pp. 323-327) to an issue which I consider critical: monitoring the process of planning and plan implementation. Institutionalizing feedback and evaluative mechanisms is essential if one is to assess whether plans are attaining their objectives, whether the process of implementation is appropriate, whether the environment (human and physical) has been "behaving" in accordance with anticipations which went into the formulation of plans.

Kahn's books will aid the reader in thinking about planning more broadly than he might otherwise be tempted to; they will greatly enrich his knowledge, via brief illustrations and the case studies in the second volume, of some of the practices and experiences in American social planning (with occasional excursions into foreign examples); they will stimulate him perhaps to the point of becoming convinced that social dimensions of planning and plan implementation might well be one foundation on which the unity of social and behavioral sciences, basic and applied thrusts, could be based. These volumes deserve to be read and used, although no reader is likely to close them without concluding that more could have been expected from the author's erudition, great experience, insight, scholarship, and dedication.



*Modernization Among Peasants: The Impact of Communication*, by EVERETT M. ROGERS, in association with LYNNE SVENNING. New York: Holt, Rinehart and Winston, 1969. 429 pp. \$6.95.

GEORGE M. BEAL  
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The main theoretical and methodological theme of this book is made clear in the Preface: "the lack of utility contained in the abstract, speculative, and prolific writings of the grand theorists on social change and economic development has been abhorrent to me, as the purely speculative nature of these works precludes empirical testing of their central notions in the foreseeable future. Our attempt in the present investigation is to demonstrate the advantage of 'middle-range analysis' where theories containing general concepts are tested with empirical data from the 'real world.'" Accordingly, Rogers seeks to determine commonly occurring patterns of behavior among peasants that illuminate their paths toward modernization, and to demonstrate theoretical and methodological approaches to the study of modernization among peasants. Modernization, approached from the individual level, is defined as the process by which individuals change from a traditional way of life to a more complex, technologically advanced, and rapidly changing style of life. Rogers' main data base is a field study of 255 peasants in Colombia. He attempts to determine the cross-cultural validity of his findings by comparing his Colombian findings to research in a number of other less-developed countries, emphasizing India and Kenya.

Rogers approaches his task with three chapters dealing with "Social Change, Development, and Modernization"; "The Subculture of Peasantry"; and "Middle-Range Theory." The chapter on middle-range theory provides the basis for most of the remainder of the book. The essence of middle-range theory is taken to mean a set of postulated relationships which are testable but which deal only with a rather limited, particular type of behavior. Rogers believes modernization is essentially a communication process. He orders his limited number of concepts related to communication and modernization as follows: main antecedents—literacy, mass media exposure and cosmopolitanism; intervening variables—empathy, achievement motivation, fatalism; and main consequences—innovativeness, political knowledge, and aspirations. In general, one or more chapters are devoted to each of these, or closely related, concepts. The final

chapters deal with the interrelationships among conceptual variables, computer simulation of innovation, and next steps in modernization research.

There are a number of positive things that can and should be said about this book. It explores in depth and presents in detail the relationship between and among a limited number of variables assumed to be the essence of modernization. Concepts are clearly defined; operational process and measures are clearly presented. Rogers' work represents an excellent codification of the findings from a wide range of studies. His data retrievable system is apparently operative. His multivariate analysis, prediction, graphic presentation of data and findings, and the precise summaries at the end of each chapter are highly commendable.

However, the study suffers from a number of shortcomings. The apparent attempt to combine a series of papers done at different points in time and by different students and colleagues into an integrated book does not quite come off. Though Rogers proposes a modernization process "model" with antecedent, intervening, and consequence variable categories in a limited time-dimension or "causal model," the internal chapter analyses are often done without regard to the proposed model. While the detailed codification of findings from a wide variety of sources is highly commendable and valuable, this codification is often a listing without sufficient conceptual clarity and theoretical rationale. Rogers' theorizing (even middle-range) is often missing. Though he posits the position that middle-range theory will close the gap between grand theory and raw empiricism, little attention is given to relating middle-range theory upward toward grand theory. The demonstration of different quantitative analytical techniques is valuable. However, in some cases this appears to be the primary objective, rather than bringing conceptual clarity and understanding to the modernization process. The data base is limited, not so much a fault of Rogers as the state of the art.

Despite the criticisms, this book is highly recommended to those interested in the micro-approach to understanding the modernization process in less-developed countries. Though it certainly does not provide the final answers, it does provide an excellent codification of the present state of knowledge, with suggested and tested refinements by Rogers and his colleagues. It can be a significant base for truly innovative research and application.

*Agents of Change: Professionals in Developing Countries*, edited by GUY BENVENISTE and WARREN F. ILCHMAN. New York: Praeger Publishers, 1969. 252 pp. \$13.50.

*Planned Change in Agrarian Countries*, by ARTHUR H. NIEHOFF. Technical Report 69-21. Alexandria, Va.: Human Resources Research Organization, 1969. 147 pp. Paperbound. No price indicated.

JOHN C. BELCHER  
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Both of these volumes are concerned with the roles of the professional as he attempts to induce technological change in an underdeveloped country. The study by Niehoff presents a formula considered to be effective in bringing about the acceptance of innovations in all change situations. The other study presents rather solid evidence as to why no single theoretical approach nor educational background can guarantee the effectiveness of a change agent.

In *Planned Change in Agrarian Countries*, Niehoff analyzes 203 projects from developing countries, from which he develops a taxonomy of the factors influencing innovative efforts. (This taxonomy is virtually the same one presented in his earlier work, *A Casebook of Social Change*.) The list of factors will be familiar to anyone who has read the various community development guides published through the years; for example, it is hardly surprising to read that innovations are introduced through local leaders, must be adapted to the existing social structure, and are to be initiated in such a way that they are compatible with the culture of recipient groups and of maximum benefit to them.

An examination of the appendix shows the source of most of the 203 cases to be two- to four-page summaries from mimeographed reports, a few professional journals such as *Human Organization*, text books, and popular magazines including *The Reader's Digest*. Several cases come from one report. For example, 13 of the cases are from a few pages of a bulletin by Luz Einsiedel published by the University of the Philippines. There is no solid evidence that these various cases made any contribution other than to authenticate conclusions previously reached. The main portions of the book appear to be lecture notes for an undergraduate course on social change. Professionals may dismiss the work as an over-generalized reification of concepts nearly as old as the social science disciplines.

The Benveniste-Ilchman volume is based on the assumption that professionals abroad tend to behave in direct contrast to what is expected of them at home, where they tend to be objective, politically neutral researchers employed by a conservative establishment. Abroad the professional develops a radical stance as he becomes involved in action programs in different environments and must adapt to many roles outside his area of expertise. This volume contains the essays of seventeen professionals with experience abroad in such diverse fields as criminology, forestry, and education—all of whom participated in a conference held at Berkeley. Each addresses himself in his own way to the problems and role-conflicts the professional encounters abroad, and examines possible modifications of the training programs of professional schools to better prepare Americans for increasing participation in the technological development of other countries. In general, there seems to be considerable pessimism that such professionals can be developed within these training programs; there does seem to be a feeling that general social science training prior to entering the professional schools would have considerable promise. It is impossible to find many specific areas of agreement, since each writer presents his own interpretation of the roles of change agents in cultures other than his own—some in short and seemingly extemporaneous essays, and others in lengthy and carefully prepared expositions. Those with overseas experience will be able to benefit from the ideas expressed, e.g., in revamping training programs; students desiring international opportunities probably will learn little from the volume.

Certainly the conference volume casts great doubt upon the usefulness of Niehoff's simplistic approach to change, which may serve as a model to be followed in giving lectures to civic luncheon clubs wanting positive answers to everything.

*Africa in Social Change: West African Societies in Transition*, by P. C. LLOYD. New York: Frederick A. Praeger, 1968. 363 pp. \$7.50.

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In recent years we have witnessed a steady increase in the number of books about Africa, but the field of sociology has not been conspicuous in its contribution to that output. Lloyd's book was, therefore, especially welcome when it first appeared as a paperback in 1967. This clothbound revision is essentially the same

as the original with minor alterations in the text and in the statistical appendix and a few recent titles added to the bibliography.

The subtitle is more accurately descriptive of the contents than the title, for the book is focused completely on West Africa. Drawing on the research of other scholars and his own fifteen years of professional experience in Nigeria, Lloyd has combined a theoretical discussion of the processes of social change with detailed illustrative descriptions of institutional change in West African societies. While the book speaks clearly to the special interests of economists and political scientists, the approach is distinctly sociological and as such is an important addition to the literature.

Part One sets the scene with generalizations about the traditional social and political organization of West African societies (and Lloyd is careful here and elsewhere to note the necessity for caution in generalizing). Part Two traces the history of contact with the West, and considers some of the effects on rural and urban life and the rise of a Western-educated elite. Part Three contrasts functional and conflict theories of society as they relate to change, and then describes changes in West African family structure, the development of new urban associations, the growth of political parties and the trend towards one-party government, and the types of strain experienced by individuals in a rapidly changing society. Part Four considers the future of West African independent states in the context of developing ideologies, tribalism, and emerging types of conflict.

Two related themes recur throughout the book. First, Lloyd emphasizes the importance of changes within the societies rather than changes effected from without. Contact with the West has not led to a collapse of the traditional institutional structures; on the contrary, considerable associational cohesion has been maintained. In this time of transition from tribal society to modern society, then, the crucial factor is not the rate of borrowing from the West, but rather the speed and smoothness with which indigenous institutions can be modified. Second, Lloyd stresses the importance in this transitional period of the relatively small Western-educated elites who assumed political leadership in every West African territory as Independence came. Much will depend on the abilities of these elites to understand the problems their nations face and to make effective use of the opportunities open to them, to mediate successfully between European and traditional African values, and to obtain support from the masses in their endeavors. To Lloyd the future of the West African states rests largely with these elites.

Lloyd has generalized courageously, and his detailed descriptions effectively support the generalizations and increase their clarity. Particularly interesting is the discussion of tribalism as an urban phenomenon. On the other hand, the book is less impressive theoretically than descriptively. The discussion of functional and conflict models, for example, is not followed up to the extent that one would expect. Lloyd has not broken any theoretical ground in this volume, but he has made a signal contribution to our understanding of the sociology of change in West Africa. Finally, the five maps are good, the annotated bibliography is helpful, and the index is thorough.

*Conflict and Continuity in Brazilian Society*, edited by HENRY H. KEITH and S. F. EDWARDS. Columbia, S. C.: University of South Carolina Press, 1969. 312 pp. \$10.00.

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This book is a compendium of historical essays and commentaries on them, presented by 17 outstanding Brazilianists at the Brazilian History Seminar held at the University of South Carolina in October of 1967. The editors explain that they selected the title "Conflict and Continuity" because "it seemed that the 'unopposite' conflict-continuity dichotomy held much more promise of yielding meaningful interpretations of Brazilian history than the more conventional opposites of continuity and change." The index of Portuguese and Brazilian names, terms, and places is appreciated; however, the cross-referencing is not as complete as one would like it. For example, the entry "Absentee landlords, influence of on rural labor, 297" does not reappear under "Labor, *rural*" but under "landowners, impact of absence of on rural labor." Some cross-references, because of duplication, were unnecessary: "Afonso V (1438-81), concessions to Flemings, 7" "Flemish merchants, privileges granted to (1457), 7." Also: "Foreign Merchants," "Foreigners in Brazil," and "Foreigners in Seville." Finally, the names of some lesser figures are listed, while the Marquis de Pombal and Euclides da Cunha are not.

The essays are divided into four general categories ("The Economy," "The Church," "The Polity," "Synthesis and Interpretation") and are arranged on an institutional basis, which results in an uneven distribution of emphasis.

Bailey Diffie discusses foreigners and their activities in Brazil, thereby challenging the traditional position that Portugal "followed a policy

of exclusivism and monopoly." He shows that foreigners, e.g., merchants and bankers, received privileges in Portugal going back as far as the 12th century. Dauril Alden makes an "assessment of the extent to which the Crown's decision to expel the Jesuits from its domains was economically motivated and the economic consequences of that action." He considers (1) the sources of Jesuit wealth in Brazil, (2) conflicts between the Jesuits and their rivals before 1722, (3) the campaign of Paulo da Silva Nunes against the Jesuits 1722-1746, and (4) the climax of Portuguese Jesuitphobia of the fateful fifties. Manoel Cardozo presents a study of Brazil's Enlightenment and Bishop Azeredo Coutinho who represented it. Azeredo Coutinho, who studied at Coimbra, was an enthusiast of the Enlightenment. "On the surface . . . Brazil was free of the horrendous Church-State problems which afflicted Spanish America," states George Boehrer. This freedom he explains in a consideration of regalism and reformism within the Church in the latter half of the nineteenth century. Alan Manchester asserts that "the transfer of the Portuguese court from Lisbon to Rio de Janeiro, 1807-1808, precipitated developments of long-term consequence. It modified radically the relationship between the Mother Country and its most prosperous colony; it shaped the course of Brazilian Independence; and it contributed materially to the preservation of the union." Harry Bernstein's paper pertains to the *juiz do povo* and the independence of Brazil during the period of change from Pombal to the Peninsular Wars. In his study he demonstrates that a public figure, the *juiz do povo*, helped halt the disintegration of Portuguese political life despite the departure of the royal court. Henry Keith, questioning the nonviolent tradition in Brazilian history, states that it was first "an apologia for the eventual victory by the monarchy over liberal and separatist [then later] it become official dogma." Republican governments used the tradition to defend the *status quo* "in the interest of maintaining themselves in power." Jordan Young states that "the course of events of October, 1930, demonstrates a peculiar ability of the nation's leaders to propose compromise political solutions to crises and of the people to accept them, thus avoiding disastrous civil disturbances. The avoidance of conflict by compromise is a definite fact of historical life in Brazil and one that provides an effective pattern of continuity from the colonial period to the present." Lahneyer Lôbo stresses the theme of continuity which is, in her opinion, "the dominant feature of Brazilian history." She examines the evolution of the social and economic foundations of Brazilian society, ana-

lyzes the developments in Brazil from 1929 to the present, and examines the different prescriptions for future developments in Brazil.

*Family and Kinship in Chinese Society*, edited by MAURICE FREEDMAN. Stanford, Calif.: Stanford University Press, 1970. 269 pp. \$7.95.

YUNG-TEH CHOW  
Northern Michigan University

This book is a collection of ten papers written by ten authors; yet it does present a comprehensive picture of the Chinese family, marriage, and kinship. Although only one of the authors is Chinese, all of them have had firsthand acquaintance with the Chinese scene.

Since such work is hampered by the inaccessibility of the People's Republic, we should be satisfied at present with the material drawn from studies made in Taiwan, Hong Kong, and Singapore. However, Freedman himself is conscious of the "insecure foundation for general statements about Chinese society—except, of course, in regard to the changes brought about on the mainland since 1949" (p. vii). Nevertheless, at least one of the authors assumes "that we may usefully generalize about key modes of behavior in Chinese society, huge though it is"; and he ventures to speak as if he could support his "general statements with data from all parts of China and recorded at many points in time during last hundred years" (p. 163). Anyway, this volume does illuminate many features of Chinese social life and institutions by a close study of them in these atypically Chinese areas.

The authors and subjects of the essays are Myron Cohen on the nature of the *chia*, Margery Wolf on child training and the Chinese family, Irene Taeuber on the families of Chinese farmers, Ai-Li S. Chin on family relations in modern Chinese fiction, Jack Potter on land and lineage in traditional China, Johanna Meskill on the Chinese genealogy as a research source, Maurice Freedman on ritual aspects of Chinese kinships and marriage, Arthur Wolf on Chinese kinship and mourning dress, John McCoy on Chinese kin terms of reference and address, and John Pelzel on comparison with Japanese kinship. Though all of these papers are substantial and informative, three essays are especially significant.

Chin presents an analysis of two sets of short stories and a comparative study of family relationships in Taiwan and the mainland. Her findings will surprise most readers: even if they are not taken aback by the desperate sad-

ness of the stories from Taiwan, they will be jolted by the traditionalism—one might almost say conservatism—of the Communist stories.

Meskill, in a splendid research on Chinese genealogy carried out in Taiwan, found that the Chinese genealogy is the product of a particular ideological environment. When the genealogical labors of Chinese editors can be combined with the investigative resources of the modern state and the inquiries of contemporary social science, the value and limitations of the genealogy will become fully clear.

Pelzel's investigation of the Japanese kinship system demonstrates that the Japanese *ie* belongs to a different world of structure from the Chinese *chia*, even though the two terms are written with the same ideograph, and that the larger assemblages of kin in Japan lack the form and significance we have learned to associate with Chinese lineage. In the strictest sense the *ie* is a corporate group, and it never (in principle) dies. But the *chia* is in a perpetual state of dissolution, for when the partition of a joint family takes place, none of the resulting units retains the precise identity of the unit from which it springs. Furthermore, the estate of the *chia* is unlikely to be maintained as an entity over the generations unless the family has only one son in each generation. Therefore, it is obvious that the Japanese single-heir or unigeniture system of inheritance must have a tremendous effect on the economic and social mobility patterns of the country.

*Peace on the March: Transnational Participation*, by ROBERT COOLEY ANGELL. New York: Van Nostrand Reinhold Co., 1969. 205 pp. Paperbound. \$2.45.

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University of Pittsburgh

Angell has tackled one of the most puzzling empirical problems of contemporary social science: whether the increase in the crossing of national boundaries involves also a breaking-down of those boundaries. Does transnational activity yield a significant attenuation of loyalty to the nation-state and, thereby, the nation-state form of international system? Those who have worked in this area, (e.g., Karl Deutsch, Johan Galtung, and David Singer) have not arrived at definitive conclusions.

Angell sets out to coordinate the relevant findings, adding some of his own and presenting previously unpublished data from older surveys. Eight forms of transnational participation are

delineated: study, teaching, and research abroad; settlement; international assistance; service in foreign missions; residence for business reasons; residence in military service; participation in international non-governmental organizations; and membership of United Nations secretariats. Angell's major hypothesis is "that policy-makers are being subjected increasingly to a stream of influence from elites toward accommodation among nations, a stream that derives in part from the growing amount of transnational participation with positive effects from those elites." A secondary hypothesis is that "the transnational participation of those engaged in it is not decreasing relative to their domestic participation." As Angell is quick to admit, this secondary hypothesis cannot really be tested in reference to available data. However, he claims that what he has discovered to be a broadening stream of transnational participation is not, at least in the more "developed" societies, being matched by increasing rates of domestic participation. How such a conclusion could be reached without either lengthy argument or a substantial amount of documentation is difficult to see. It appears that what Angell has in mind in talking of domestic participation is an increase in participation in organizations whose growth would *ideologically* undermine his findings about the positive, accommodative effects of increasing transnational involvement. But this is far from being the whole story. To use as the major example the membership figures for the Daughters of the American Revolution is perhaps indicative of the extent to which Angell fails to consider the problem comprehensively. It is not simply a matter of whether "nationalistic" tendencies are outmatching transnational tendencies, but also whether intra-societal developments are leading to societal introversion and/or the proliferation of sub-national preoccupations. The recent history of the USA would seem to provide an excellent example of just this possibility.

Angell argues with admirable persistence in the face of inadequate data that there is an increasing web of transnational participation—a web which, it should be emphasized, has two large areas of weakness. The first of these is centered on China and includes a number of other societies in that vicinity; the second is sub-Saharan Africa. To the question about whether these mainly less-developed societies are likely to be drawn into the transnational web Angell understandably has no firm answer. Of the eight forms of transnational participation which he examines, Angell finds that only two clearly have negative effects on relations between nations. Of these, missionary activity is rel-

atively unimportant as far as size and rate of expansion are concerned. It is business penetration which is the most seriously negative in its consequences.

Angell has performed a valuable service, even though there do appear to be some inconsistencies between some of his conclusions and the data which he presents (undoubtedly brought about by his tendency to adhere to a form of social-science utopianism frequently found among students of international affairs). Other problems are that Angell attributes excessive importance to the Bolshevik revolution as the source of the present divisions on the international scene, and that surprisingly little mention is made of the contributions of Deutsch to the debate about the relationship between communication across and communication inside national boundaries. Nevertheless, Angell's book is a modest and helpful contribution to a vitally important social-scientific problem area.

*We Wish To Be Looked Upon: A Study of the Aspirations of Youth in a Developing Society*, by VERA RUBIN and MARISA ZAVALLONI. New York: Teachers College Press, 1969. 257 pp. No price indicated.

RICHARD G. BRAUNGART  
*University of Maryland*

This monograph examines ethnic and social class differences in value systems and the impact of a changing society on the attitudes and aspirations of secondary school youth. The authors, an anthropologist and a social psychologist, undertook an interdisciplinary case study of 960 upper form secondary school students from Trinidad, using data obtained in 1957 and 1961 from some 30 secondary schools randomly distributed throughout the island. The research instrument, adopted from the cross-national study by Gillespie and Allport, included two protocols: (1) an autobiography of the future in which respondents were asked to write about their hopes and expectations up to the year 2000, and (2) a self-administered questionnaire covering a variety of demographic and attitude questions. The main control variables were ethnic group identification, social class, religious denomination, sex, and type of secondary school attended. The dependent variables included educational and occupational aspirations, long-range goals, attitudes toward marriage and family, expectations of styles of living, and orientation toward social, political and economic goals.

In Chapters I through III, the authors pre-

sent their research strategy and provide a brief scenario tracing the social history of Trinidad from a plantation-based British Crown colony to national independence, with its accompanying structural differentiation and newly diffused "modernistic" values. The major ethnic groups are Whites and Colored (comprising the high-status student groups) and East Indian and Negro (comprising the low-status student groups). The upper-class is made up of White planters and proprietors; the middle class consists of a second rank of White overseers and officials, including Colored landowners and professionals; the lower class is composed primarily of unskilled East Indians and Negroes who comprise the peasantry and proletariat. Ethnic *status* and social *class* characteristics are fused into a "pigmentocracy" where whiteness places the individual at the top of the social and economic order and blackness at the bottom. The educational system in Trinidad does little more than reinforce the existing ethnic status and class positions.

Rubin and Zavalloni find that low-status student groups, unlike their high-status counterparts, exhibit less personal fear of failure, higher striving potential, stronger idealized aspirations, and a tenacious drive for mobility. Lower-class students within the lower-status ethnic groupings exhibit these characteristics in more pronounced fashion. Lower-status students exhibit higher achievement motivation, a striving orientation, a preference for prestigious or "status-linked" occupations, nurturing attitudes toward others, greater expectations, and more proneness to mythologize and fantasize future goals. High-status students favor modernistic occupations, privatized and economic goals, and are much less ambitious.

The ethos of mobility and the desire for material life-styles are interpreted as the leitmotif of lower-status and lower-class students. What are perceived as luxuries for lower-status students are accepted as necessities by higher-status student groups. Status and class characteristics are found to influence the convergence of personal aspirations with perception of social requirements in altruistic fashion. When compared on an Index of Social Orientation, lower-status students rank highest on psychological, social, and national commitments; high status students rank high on status maintenance and privatized economic commitments. Again lower-class youth exhibit more pronounced commitments.

The authors conclude by suggesting that, while the students in their study represent a "mobile elite" (only one-third of Trinidadian youth attend secondary school) and the majority per-

ceive education as the *rite de passage* for success and happiness, little or nothing in Trinidadian society suggests that these aspirations will be fulfilled. And, of course, this is the dilemma and tragedy facing all youth in a developing society.

Since the authors interject verbatim essay material throughout the monograph, it is unclear whether the essays stimulate or initiate contextual analyses or whether essay materials are used merely as supportive or illustrative data. In either case, we do not know how representative—both qualitatively and quantitatively—the student essays are. Content and simple marginal analyses could help provide the answer. Granted, multi-ethnic status-group differences exist (a plus for Weber). However, in a polarized and rapidly changing society such as Trinidad, where is the class consciousness (a minus for Marx)? Essays expressing opposing or radical points of view are either nonexistent or are not selected for inclusion in this monograph. This raises some questions as to the representativeness and validity of the student sample itself. With these caveats, this study is highly recommended to social scientists interested in Caribbean society and survey research methodology.

*Readings on the Sociology of Small Groups*, edited by THEODORE M. MILLS with STAN ROSENBERG. Englewood Cliffs, N. J.: Prentice-Hall, 1970. 247 pp. Paperbound. \$3.95.

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This book of readings, a companion volume to Mills' *The Sociology of Small Groups*, consists of 18 articles designed to "cast new light on current thought" in the field of small-groups analysis. The editors state that their primary purpose is to emphasize the relevance of studying groups and to demonstrate "the quality of work through which the field is likely to advance." The theme emphasized throughout the book is that the small group represents larger social systems in microcosm. Interaction among personality, the group's social system, and its culture are fully examined. Emphasis on observing small groups in their "natural" setting indicates the editors' strong desire to generalize from the small-groups level to macro-systems.

Part I ("The Relevance of Small Groups") consists of one article by Sidney Verba focusing on the relationship between the small group and political behavior. As good a selection as this is, it is likely to leave the reader with a rather

narrow sense of small-group relevance and applicability. Part II ("Theoretical Perspectives") consists of five articles that demonstrate rather well the interaction among personality, social system, and culture. Parsons' system theory, Bales' Interaction Process Analysis, Deutsch's Cybernetic Model, and two articles on unconscious processes in groups are included. Though consistent with the general theme of the book, these articles cannot be considered representative of the theoretical approaches currently employed in the field. Ignored are such perspectives as field theory; the exchange theory of Homans, Thibaut and Kelley, and Blau; and symbolic interactionism as represented by such authors as Goffman and Blumer.

Part III ("Observing and Experimenting with Groups") is the weakest section of the book. Included is one article by Sherif and Sherif on observing groups in their natural setting, and one article by Kintz, *et al.* on the experimenter effect in laboratory studies. The editors are too willing to set aside some of the more crucial methodological hang-ups in small-groups research, e.g., problems of causal inference, validity and reliability of measures, and accuracy of observations. Part IV consists of eight articles which provide a good sample of research conducted primarily in natural settings. Here are observations of small groups such as the family, the street-corner gang, therapy groups, army basic-training squads, and work groups. Part V ("Change and Growth in Groups") consists of two articles dealing with human-relations training groups. Selection of training groups as a context within which to discuss group growth and change greatly reduces the generalizability for which the editors seem to be calling throughout the rest of the book.

Mills and Rosenberg in introducing each article do a fairly good job linking each to their unifying theme. What would have been welcomed, however, is more elaboration and interpretation, discussion of avenues of further research, suggestions for further readings, and perhaps some original—at least more recent—works (the most current having been written in 1966).

Although this book does not present a representative sampling of small-group theory and research, it does make a laudable attempt to tie together a number of small group studies around a common theme or theoretical perspective. Moreover, it provides needed encouragement to small group researchers to get out of the laboratory and attempt to generalize their findings to larger action systems.

*Persons and Positions: Individuals and Their Social Locations*, by LEILA CALHOUN DEASY. Washington, D. C.: Catholic University of America Press, 1969. 301 pp. Paperbound. \$3.50.

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This volume is composed of eight papers apparently written between 1955 and 1966, two of them undated. For the most part they were originally prepared for oral presentations. The book was evidently prepared from a typed manuscript by using a photographic process. The result is that footnotes occupy an inordinate amount of space since the type and spacing in the footnotes is the same as that in the text.

According to Deasy's introduction, the first paper provides the theoretical framework for the others. Four aspects of role are pointed up, i.e., "individuals (1) in social locations (2) behave (3) with reference to expectations" (p. 8); and (4) emotional values and sentiments are implied (p. 9). Beyond that, the author offers no definition of role. This may be an advantage insofar as it does not add to the endless proliferation of definitions, but role relationships are analyzed without ever specifying exactly what is meant by role or indicating how to determine where one role ends and another begins. The theme of the paper is the built-in adjustment difficulties attendant to occupying several roles. Specifically, role is related to age and sex, family structure, occupation, social class, ethnic origin, and conflict.

In the second paper life cycle is treated from a role perspective. In lieu of a definition of role the author again falls back on the "key components" enumerated in the first paper. This chapter represents an augmentation of part of the first paper, using life cycle as an organizing principle for a discussion of age and sex roles.

The third paper is a case study of a working class family which for a number of years virtually kept secret the fact that it contained unstable quadruplets ultimately diagnosed as schizophrenic. The paper focuses on the means whereby the family was able to keep the illness a secret despite that as children the young women had achieved some notoriety as the only living monozygotic quadruplets. The fourth paper, "Psychiatric Patients as Nursing Personnel See Them," deals with patterns of interaction between hospital personnel and patients on a closed psychiatric ward. Extensive attention is paid to the reaction of nurses to patients.

The fifth paper treats the relationship between wives of first-admissions mental patients and

hospital psychiatrists, focussing upon the kind of contacts with the psychiatrists sought by the wives and the psychiatrists' interpretations, related to the "family doctor" image, of their responsibilities to the patients' families. The sixth paper reflects the author's problems in conducting research on the families of hospitalized psychiatric patients. The seventh paper is a review of some of the literature on role relationships within the hospital; it is directed primarily toward roles in medicine, nursing, social work, and clinical psychology. The final paper is restricted to an analysis of elements of the student-professor role relationship in the doctoral program in a graduate school of social work.

As a whole, this book is difficult to characterize. It is not particularly well integrated; indeed, many of the articles are related only in that they deal with "persons and positions." It is by no means a theoretical book, nor was it intended to be; it does, however, contain a number of insights and is at times provocative. It should not be ignored.

*Birth Order and Life Roles*, by LUCILLE K. FORER. Springfield, Ill.: Charles C Thomas, 1969. 168 pp. \$ 8.95.

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This book by a clinical psychologist has been written for professional and lay persons to describe the influence of the sibling order on the development of "child and adult characteristics." Forer seeks to integrate the various research findings in this field, both quantitative and qualitative, with her own clinical observations. Independent variables discussed are family size, sex of the children, birth order, and the respective sibling roles. The birth order affects such dependent variables as self-identity, choice of friends and spouses, intellectual aspirations and academic achievement, the extent of gentleness and anger, kindness and anxiety, and the occurrence of psychopathological features and deviant behavior—to mention only a few. According to the author, these effects occur in any culture in which families exist as separate and stable units. Under these conditions, birth-order effects are also observed on all socio-economic levels.

The discussion of "the family" in all its possible numerical and sex role (e.g., one-boy and one-girl families; boy-boy, boy-girl, and girl-girl families; more-child families) is overly abstract because socio-economic and other factors



are often not considered. At the same time, however, the analysis is limited to the "usual" (white, middle-class) family in the United States and similar societies. Conditions such as absence of the father and adoption are mentioned briefly and then dismissed as atypical.

Although Forer is aware of the fact that socialization is a very complex process, she sometimes tends to end up with an almost monocausal explanation of personality development. It seems that this tendency is mainly a result of the fact that she neither presents a comprehensive theory of the socialization process nor indicates the relative explanatory power of the variable "birth order." Thus, the book is predominantly a collection of empirical findings and empirical generalizations. Forer fails to show or even to discuss how findings obtained by such divergent approaches as psychoanalysis and survey methods can be theoretically and methodologically integrated, beyond fragmentary references to role theory and the psychoanalytic theories of Freud and Alder. She reports, e.g., that boy-boy families "contribute more cases to behavior clinics" than any other family type. This is "explained" by the fact that these siblings show "learning disabilities" and "language problems" more frequently than any other combination of siblings. Why these occur, however, is not explained (p. 56).

Forer believes that her book could contribute to a better understanding of "why other people think, feel, and behave as they do." And in the best tradition of positivism she thinks that people can and will use her information "for conscious direction and control" of their behavior. To make sure that people draw the right conclusions, she offers 28 pages of suggestions to parents as well as siblings. Although her practical aim of enlightening people is certainly praiseworthy, it seems unlikely that suggestions written in such a general, horoscope-like language will be taken seriously or that the lay reader will be attracted by the book's cover, much less its price.

*Adjustment to Retirement: A Cross-National Study*, by R. J. HAVIGHURST, J. M. A. MURNICHS, B. NEUGARTEN, and H. THOMAE. New York: Humanities Press, 1969. 195 pp. Paperbound. \$6.25.

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*Adjustment to Retirement* is a too-modest title for such a capstone work as this cross-national study of some 300 retired Caucasian males (ages 69-76) in 12 socio-cultural contexts

(public school teachers and steel workers in six major urban centers—Vienna, Austria; Milano, Italy; Bonn-Ruhr, Germany; Nijmegen Holland; Warsaw, Poland; and Chicago, U.S.A.). The research extends the socio-psychological parameters of "successful aging" so representative of retirement research cited in Chapter One.

The writers forthrightly and with methodological rigor become their own critics of the study's limitations in Chapters Two and Three. With equal enthusiasm they generate an encouraging holistic approach to aging centered in an exhaustive examination of 12 distinct roles. In their within-group, between-group, and between-culture comparisons, they do not leave the retiree a "nothing but" determined archetype. Rather, the adjustment is presumed to embrace at least three dimensions: the cultural, the societal, and the individual. Using an overlooked concept that has been conspicuously buried in gerontological literature, they view the aged's behavior through a "reference group" orientation that avoids a doctrinaire perspective of either engagement or disengagement.

Defining theirs as a non-theoretical pilot study of the socio-psychological dimensions of adjustment of occupational similars in culturally dissimilar settings, the authors use a Malinowskian frame of reference; that is, they do not posit a Freudian universalistic model to the retiree phenomenon. The research evidences a healthy merger of a macro structural-functional stance (Merton—similars emerging from dissimilars and dissimilars evolving from similars) with a micro interactionist tilt (Maddox—self is contextually anchored within reciprocal relationships).

For the serious student of aging, a thorough examination of this volume provides many hypotheses for further theoretical and empirical elaborations. The study indicates the need for further probing into the priorities of meaningful aging. For example, what weights may be assigned to the inputs (*individual socio-psychological attitudes, group supports* from the occupational or professional socialization and continued relational involvements, and *cultural values* that provide the milieu in which the previous are operative) leading to a highly positive "Global Life Outlook"? Or again, at what point does this "sustaining system" break before the three axiomatic characteristics (see O'Dea's *Sociology of Religion*) of human existence: contingency or the uncertainty context and the ensuing human injury, powerlessness of the impossibility context and the vulnerabilities to which all flesh is heir, and scarcity or the insatiability context and subsequent felt deprivation?

Years in planning and execution (1957-1968), the research is an adroit probing and dramatization of the differential roles and coping norms of retirees of various cultures and occupational subcultures. The truism of cultural relativity is tested against another truism of the dynamic and developmental nature of the life process. Addressing itself to ontologically significant issues of policy-making import, the study warrants serious reading by any student, teacher, researcher, or practitioner in social gerontology.

*An Inventory of Research Findings*, by MATILDA WHITE RILEY, ANNE FONER, and ASSOCIATES. Volume 1 of *Aging and Society*. New York: Russell Sage Foundation, 1968. 636 pp. \$35.00.

*Aging and the Professions*, edited by MATILDA WHITE RILEY, JOHN W. RILEY, JR., MARILYN E. JOHNSON, and ASSOCIATES. Volume II of *Aging and Society*. New York: Russell Sage Foundation, 1969. 410 pp. \$9.50.

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Publication of the *Handbook of Social Gerontology* in 1960 sparked a burgeoning interest among sociologists in the advanced stages of the life cycle. Recent monographs have examined the status and roles of older persons in the United States and in other societies. However, these two volumes of what is eventually to be a three-volume set represent one of the more ambitious efforts to date in gerontology. Volume I provides a number of generalizations on aging abstracted from census materials and research literature. Volume II draws out implications of this research for practitioners working with aged clientele. A third volume in preparation will place the findings in a sociological context.

Volume I will be of greatest interest to the sociologist. Following the tradition of Berelson and Steiner (*Human Behavior*), it represents a massive effort to develop some basic generalizations that have received empirical support. These generalizations are stated succinctly in propositional form with appropriate reference to supportive studies and, in some cases, with cautions from the authors as to proper interpretations. A major interpretational problem lies in the fact that gerontological studies commonly have employed cross-sectional research designs, whereas the theories being tested focus on aging as a developmental process.

The material is diverse, being cast in 25

chapters ranging from considerations of physical changes in aging through personality development and psychological adjustment to the integration of older persons in the social and economic structure of society. References largely are to studies conducted in the United States, although some cross-cultural material is included. Many tables and graphs are used to enrich the discussion. Individual chapters examine such topics as population and residential patterns; physical changes in aging; personality needs; life attitudes; mental disorders and deviance; work and retirement; economic, political, religious, and leisure roles; and community participation.

Illustrative of the hundreds of generalizations contained in this volume are the following on the morale of older persons: "Morale tends to decline by age beyond 60+"; "Morale is generally higher among men than women"; and, "Although positive satisfactions (happiness or morale) may decrease by age, worrying (quite a different dimension of the individual's general feeling about his life) does not show a complementary increase." In addition to providing research generalizations, each chapter contains a bibliography of recent materials as well as a brief statement of future research needs. The listing of research materials that have appeared in books and journals not normally perused by sociologists is valuable.

This volume is a necessary acquisition for sociologists interested in gerontology and will be a prized sourcebook for persons wishing to give greater attention in their teaching to the later stages of the life cycle. Hopefully the price will not prevent its being acquired for personal libraries.

The volume has several faults, even though it is clearly a landmark contribution. One shortcoming is the sense of closure that pervades the volume. The methodical listing of propositions often suggests that particular issues are no longer viable. However, many of the propositions have tenuous empirical support, with documentation frequently resting on only one or two studies (though it must be recognized that the authors have exercised care in selecting their supportive studies, many of which are based on national samples). Thus, the reader tends not to be altered to the many current controversies, such as the debate over the desirability of age-integrated versus segregated housing and the continuing debate among advocates of life-continuity, activity, and disengagement theories. The absence of theoretical models often leaves the reader grasping for frameworks by which to organize the mass of facts. Theoretical matters, however, appear to

be a central focus of the projected third volume.

Volume II will be of lesser value to the sociologist. The objective was to draw out implications for practice of some research generalizations reported in Volume I. Unfortunately the individual chapters, each written by an expert in the field, fall short of this objective, many making only token reference to the earlier materials or using them more to establish the importance of a problem than as a source of guidance. A central theme is that research findings often conflict with stereotypes of aging held by the public, and that older persons have a greater potential for change and adaptation than is commonly believed. A secondary theme is that intervention programs to assist the aged often must be started before the onset of old age. Greater emphasis on anticipatory socialization for future life roles is suggested as a critical component of programs addressed to ameliorating negative aspects of old age.

Application of gerontological research to several fields is explored (e.g., medicine, public health, law, architecture, education, mass communication). The chapter by Phillip Hammond on the implications of an aging population for the content of the ministerial role will be of particular interest to the sociologist. Also of value is Robert Morris' discussion of needed changes in social work education suggested by recent data on the aged. Juanita Krups and Harold Sheppard present rich background data and valuable observations on the economic status and employment prospects of older persons. Many of the chapters, however, tend to be overly descriptive and lengthy, and provide few insights into practice that have not already been aired in the literature.

What Volume II illustrates best is the difficulty of developing prescriptive solutions to problems of the aged from extant research findings. While existing data is valuable in pointing up the nature and complexity of problems confronting the aged, it seldom provides a very adequate base for assessing the merits of alternative remedial actions. It needs to be better recognized that concern with research application must be reflected in the initial formulation of research rather than in awaiting the publication of results.

*Deviance and Identity*, by JOHN LOFLAND, with the assistance of LYN H. LOFLAND. Englewood Cliffs, N. J.: Prentice-Hall, 1969. 330 pp. Clothbound. \$4.95.

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*Deviance and Identity* is organized in reference to "the deviant act," "the assumption

of deviant identity," and "the assumption of normal identity." In two introductory chapters Lofland presents a brief historical discussion of the concept of deviance and a statement of his social conflict perspective. He proposes that the social differentiation of deviant acts and persons is based on societal reactions of strongly sensed threat and fear. This conception of deviance poses a problem, to which a solution is suggested:

Given the wide range of groups and perspectives in American civilization, it is probably the case that almost any act or person is strongly feared by some social category or organized group. If it is to be possible to isolate a category of deviance rather than simply a multitude of conflicts, it is essential that there be involved at least a powerful minority—even if not a majority—who feel a strong sense of threat and fear (p. 16).

The social conflict perspective on deviance is clearly one of the interesting developments in the field, but Lofland's commitment to it is ambiguous. Having presented the conceptual framework for the analysis of deviance as a product of social conflict, he proceeds to ignore it in the major body of his treatise. The only "conflict" element which remains as Lofland moves to the analysis of "the deviant act" is "fear"; and that analysis is not systematically related to the dimensions of the conflict he earlier states to be important: "the proportion of the society which feels threatened . . . how strongly the party feels threatened . . . the extent to which the party is organized . . . how much power, relative to the feared party, is possessed by those threatened" (p. 14).

Lofland also proposes that "the system focus for defining and analyzing deviance is the total society, most typically the nation-state," and that deviant acts "that can only be of concern within less inclusive or 'lower-level' systems such as formal organizations and face-to-face groups seem best left as topics of analysis within their respective formal areas" (p. 16). We find, however, that his analysis of deviance and deviants is micro-sociological in the social interactionist tradition, and focused on situations and conditions which by Lofland's own statement would be of only "local interest." His work is, in fact, an attempt to systematize, elaborate, and extend—occasionally with insight and imagination—the concepts generated by Goffman's sociology of everyday life, which is frequently cited and from which many illustrations are drawn.

There is still another ambiguity in Lofland's theoretical posture. He writes:

It is perhaps obvious but should nonetheless be

said that deviance is here defined with reference to public definitions embodied in civil rulings and detection and apprehension procedures—not with reference to any human bodies that actually get detected and apprehended. Definition hinges upon the *possibility* of detection and apprehension, not upon *actual* detection and apprehension (p. 23).

Lofland's formulation of the process of deviant identity formation, however, applies only to those who are at least publicly known if not "detected" by those who impute deviant character and impose deviant identities. This ambiguity in Lofland's conception of the "domain of deviance" obscures a fundamental issue contained in the interactionist approach to deviance, presented in its most controversial form by Howard Becker's definition in *Outsiders*: "The deviant is one to whom that label has successfully been applied; deviant behavior is that behavior that people so label." This definition underscores the view that "actual detection," in the sense of public knowledge, is a necessary condition for the definition of behavior and persons as deviant. Lofland's detailed analyses of the contingencies of what he calls the "facilitants" of deviant acts and identities presupposes such public knowledge.

The ambiguities noted above distract and confuse the reader, leading him to expect and look for analyses which do not materialize. Also unfortunate is Lofland's tendency to indulge in neologisms and phrase-making—e.g., *facilitating hardware*, *strangership*, *prosaic intermediacy*, *imputational specialists*, *residential encompassment*, *deviant-smith*, *normal-smiths*. The last of these words leads to the truly barbaric "non-normal-smith Other." The introduction of these words and phrases does not add to the discussion of, as it turns out, familiar topics, but it does add unnecessarily to its length.

*Narcotic Addicts in Kentucky*, by JOHN A. O'DONNELL. Chevy Chase, Md.: National Institute of Mental Health, 1969. 297 pp. \$3.00.

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This study presents a picture of narcotic addiction which has been neglected. While most current investigations focus on the metropolitan, nonwhite drug user, O'Donnell's research deals primarily with the rural, white Anglo-Saxon addict. The universe was defined as all white narcotic addicts who had been hospitalized at

Lexington and who had been Kentucky residents at the time of first admission, but a stratified random sampling procedure was employed in such a way as to overrepresent the more rural Kentucky counties. It seems well worth mention that, of the total sample of 266 subjects, information was obtained for 265; and no subjects refused to be interviewed. If the subject was still living, he was interviewed; if he was deceased, a death certificate was obtained. Interviews were also conducted with subjects' spouses, parents, siblings, children, police, health department officials and physicians. In addition, five other sources of information were checked for all subjects: Lexington Hospital records, Federal Bureau of Narcotics, Kentucky State Hospitals, Kentucky Department of Corrections, and County Court records from the subjects' home counties. Information was obtained for some subjects from twelve or more different sources.

Two dependent variables of major importance were "pattern of posthospital drug use" and "involvement in drug subculture." "Pattern of posthospital drug use" refers to the percentage of non-institutional time that a subject spent in drug use or abstinence. By computing the number of man-years a subject spent in the various addiction statuses, each case could be classified into one of the following categories: addicted to narcotics, addicted to substitutes (i.e., alcohol or barbituates), some abstinence, much abstinence, or complete abstinence. This measure appears to be superior to the traditional relapsed-abstinent dichotomy used in earlier studies, since it can more fully take account of those subjects who shift from one pattern to another. Thus, the data from this sample indicated that about 30% of the subjects were continuously addicted to narcotics during their posthospital time and that 10% were completely abstinent, while the remaining 60% showed a sporadic pattern of narcotic addiction alternating with periods of abstinence or shifts to substitutes.

Seven indicators of "involvement in drug subculture" were: (1) had sold narcotics; (2) obtained narcotics from non-medical sources; (3) had history of heroin use; (4) used intravenous route; (5) had had one or more "cures"; (6) was known to Bureau of Narcotics; (7) had used more than one narcotic. On the basis of these criteria, subjects were categorized as having no involvement, some involvement, or much involvement. The research revealed that involvement was positively correlated with length of addiction. Also, involvement increased for those who were younger at the beginning of addiction, for those who were addicted in

earlier decades, and for those whose first use of drugs was less legitimate (i.e., first use for pleasure or for treatment of alcoholism, rather than for medical treatment).

For these and other variables used in the study, clear operational definitions are given, and coding criteria are explicitly stated. Further, case material is often presented to illustrate the rationale for the assignment of cases to particular categories. A major aim of the investigation was methodological, to develop sound follow-up measures. Thus, considerable attention is devoted to definition of variables and to assessments of their reliability and validity. Percentage tables are used in data analysis.

Another purpose of the study was an examination of mortality rates among the sample members. The subjects' death rates were high, with observed deaths exceeding expected deaths by a ratio of 2.5 to 1 for males and about 3 to 1 for females. Much of the loss of expected years of life was accounted for by two factors: pre-admission illnesses, and deaths from unnatural causes such as accidents, suicide, homicide and behavior disorders.

The book systematically presents descriptive data on the natural history of narcotic addiction, covering subjects' family backgrounds, education, employment, sources of narcotics, self-concepts as addicts, criminal records, marital history, and hospital treatment. These variables are examined to determine to what degree they are predictive of pattern of post-hospital drug use. Later chapters focus on the social context of addiction, assessing the significance of rural-urban differences, examining the importance of contacts with other addicts, and analyzing the relationships between medical practice and addiction. The book concludes with a summary of how addiction patterns in Kentucky have changed since the early 1900's, and suggests what implications such changes have for public policy.

The book is clearly written and well organized. It deserves to be read, not only by those who have a special interest in the topic of drug use but also by students, who will find it an excellent model for sociological research.

*On the Nature of Suicide*, edited by EDWIN S. SHNEIDMAN. San Francisco, Calif.: Jossey-Bass, 1969. 146 pp. \$6.00.

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This little volume begins with the concept of *gematria*, a mystical interpretive system

based on the use of numbers and combinations of numbers to analyze texts. The number of particular interest to the editor is 58, the number of years which passed between the 1910 psychoanalytic symposium on suicide in Vienna and the first annual conference of the American Association of Suicidology in Chicago in 1968. This book, a collection of papers from the 1968 conference, is aimed at the explication of the nature of the changes in suicide theory in the 58-year period between 1910 and 1968. Shneidman has assembled for this task a group of twelve scholars—four from psychiatry and psychoanalysis; two from medicine; two from psychology; and one each from sociology, education, philosophy, and actuarial science. The list includes such respected authors as Karl Menninger, Paul Friedman, and Robert Havighurst; but many of the best-known names in suicide theory in *sociology* are missing, both from the list of contributors and from the index. Perhaps one reason for this is the editor's stipulation that all authors included had to be "well alive in 1910"; but what seems more likely is that the selection of contributors was influenced by an implicit bias against sociological theory of the type produced by Henry and Short, Gibbs and Martin, and E. H. Powell. Jack Douglas, the one sociologist among the contributors, has declared in essence that attempts to isolate some common denominator in the etiology of suicide are all but useless. Thus, neglect of sociological contributions considerably reduces the value of the book as an inventory of changes in suicide theory since 1910.

The volume suffers further by its attempt to do too much in too little space. Included in its 146 pages are six pages of section headings and blank pages, four pages of inadequate index, and thirteen chapter headings, each of which takes up more than half a page. Shneidman's prologue runs to 30 pages, and an unfocused round-table discussion to 13, leaving only some 86 pages for the 11 contributed papers. Louis Dublin gets less than four pages, Menninger less than five, and the others are similarly hampered by lack of space. The result is a jerky volume of articles too short to more than whet the reader's appetite for more. *Gematria*, in short, reveals the volume to be somewhat lacking.

Despite this basically negative reaction to the volume as a whole, several of the individual articles rise above the space limitations placed upon them. Havighurst's paper, for example, presents a number of new and testable hypotheses on cross-national variations in suicide rates and some interesting speculations

on Negro-White differences in the United States. Shneidman's prologue suggests a new typology of suicide that may prove useful to researchers. Erwin Stengel gives a brief but cogent look at the suicide which fails—its effect on the inner life of the unsuccessful victim, its effect on those close to the attempter, and the process of communication involved.

I do not know whether Shneidman intends to publish the proceedings of the annual conferences of the American Association of Suicidology on a continuing basis. Hopefully he will since there is a definite need for a periodic reassessment of the current state of theory and research on suicide. However, future volumes, if they appear, would be improved by either an increase in length or a reduction of the number of contributors. The inclusion of more clearly sociological contributions would also be in order.

*Black Suicide*, by HERBERT HENDIN. New York: Basic Books, 1969. 176 pp. \$5.95.

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This is an account of various mechanisms whereby black existence in the U. S. is psychologically transformed into racial- and self-hatred, which in turn give rise to black suicide. Each of the major chapters illustrates a mechanism which Hendin found in his study of 25 attempted suicides by blacks. The first chapter deals with the main theme of violence and frustration stemming from everyday ghetto life, and lays the base for the other manifestations in following chapters. Black male homosexuality with preference for white partners is dealt with as a failed attempt to deny the existence of racial prejudice and self-hatred. The suicide of older men is seen as resulting from failed attempts at complete submission and accommodation. The suicides of women are attributed to the inability to form stable marital unions and families, those of adolescents to general inability to adapt to ghetto life. Two fairly elaborate appendices detail statistical information, call attention to the high suicide rate among young urban black people, and report findings of a battery of psychological tests.

A secondary theme is a critique of the five sociological works on suicide cited, which Hendin argues are foolish efforts "to formularize" suicide and its causes. He is particularly critical of Gibbs and Martin and of Henry and Short. While clearly disapproving of sociological at-

tempts to generalize and frame universal statements, Hendin substitutes his own peculiar combination of cultural relativism and psychoanalytic universalism.

Critically, the book suffers from the traditional faults most sociologists find with psychoanalytic works. The first concerns the sample size and representiveness. Only two statements are made regarding the sample: the first states that the subjects appeared at a hospital following a suicide attempt and were interviewed, and in the acknowledgements the author thanks two of his colleagues for keeping him informed of suicidal admissions. There is also very little comparative material, either black with white, black suicide with black non-suicide, or even among various types of black suicide. Hendin's attention is mostly devoted to an elaboration of the universal psychoanalytic mechanisms of repression, sexuality, Oedipus and Electra complications, dream and free association interpretation, and early childhood experiences. Black homosexuality with preference for white partners, for example, is treated as an attempt to incorporate symbolically the more favored whiteness and to intensify via degradation the self-hatred of black the homosexual.

In summary, the dominant impression is that of an edited version of 25 psychoanalytic interviews with black people, strung together with simplistic psychoanalytic observations and a few comments on the state of sociology.

*Among the People: Encounters with the Poor*, edited by IRWIN DEUTSCHER and ELIZABETH J. THOMPSON. New York: Basic Books, 1968. 408 pp. \$10.00.

R. DAVID CORWIN  
New York University

This anthology focuses on the interaction between practicing professionals or research social scientists and the poor. The authors rely heavily upon non-survey and non-statistical research methods; and, in some cases, description and analysis are coupled with policy advocacy. A great deal of the research was done in Syracuse, New York, but the book embodies much of the spirit of American social sciences in general and sociology in particular during the 1960's—it is active, concerned, intense, and somewhat disorganized. It describes the field as it is.

In the Prologue, Charles Slack writes of life for "Big Chino," gang leader, man of the streets, "a human relations consultant to dying spic children." In the process, Slack talks of Big

Chino's and his own reactions to conferences, symposia and the like, and argues that professionals involved in the War on Poverty are inadequate to the task—they get rich and the poor stay poor.

Arnold Rose discusses "The Unemployables" who represent several categories of individuals—disabled, minorities, displaced—each of which requires different public policies to enhance its employability. Deutscher analyzes the role and values of "The Gatekeepers in Public Housing." Although his discussion is restricted to public housing, by extension he implies that middle-class bureaucrats control access to many government agencies, especially those servicing the poor. The role of the middle class also dominates the following article by Charles Willie, who argues that the poor need the help of "intercessors," middle-class individuals who advocate the causes and take the cases of the poor. On the other hand, Warren Haggstrom, in the book's longest essay, makes the case for the poor interceding on their own behalf through their own organizations. Anything else, he points out in a classic argument for community organization and political action on the part of the poor themselves, will result in failure and frustration.

S. M. Miller outlines the credential barriers which confront today's youth, especially the poor. Credentials are not the only obstacles poor youth face; police attitudes are another. In the most challenging article in the book, Robert Hardt examines "the extent to which apparent differences in rates of delinquency between poor kids and those who are better off reflect the behavior of youngsters or the behavior of the police." In a finding which is now predictable, the differential delinquency rate seems more a function of differential police response to identical behavior of poor and non-poor youth. "The Everyday Life of Delinquent Boys" is the focus of a two-part article in which David Cummings describes his encounters as an 18-year-old participant-observer of delinquent youth; and his mother, Elaine Cummings, comments upon her son's observations. Taken together the Cummings' work demonstrates a point made by ethnomethodologists: when we describe others we describe ourselves.

The theme of poor and non-poor is continued in an article by Jerome Beker, James Victor, and Linda Seidel which compares two schools, one for the poor and one for the middle class. They find that the middle-class school is oriented to the future while the school for the poor is custodial in outlook. Moreover, differences from one school to the other in pupil performance and attitudes increase as students go on in school. Arthur Pearl argues swiftly and

emphatically that the school is an "atavistic monstrosity" and that the dropout does not really drop out; he is forced out by an arbitrary and authoritative system. Others, especially the middle class, can adapt to the system, "Learn the Ropes." Blanche Geer *et al.* describe such a situation in the training of a variety of professionals and explore the complicated, subtle, and informal manner in which one learns to survive and even thrive in new roles.

Howard Osofsky, a physician, using anecdotal evidence, argues that attitudes of the poor who tend to ignore symptoms and physicians who are less interested in poor patients mitigate against adequate medical treatment. Taking a broader view, Lee Rainwater notes that the entire health system in the United States is threatened not only by lower-class and physicians' attitudes toward health care, but also by centralization of the health system which fails to account for the health needs and life style of the lower and working-class population. The policy Rainwater recommends is extensive decentralization. A. C. Higgins turns these arguments around, however, and in an insightful article notes that medical problems can be explained not by description of the poor but by analysis of the institutions of medical care and medical organizations. His work in Alamance County, North Carolina, shows how "idiosyncratic specialization," legalisms, and administrative confusion and bickering reduce the effectiveness of medical care.

The final section, entitled "It's the Same Everywhere," has several excellent articles. Linton Freeman and Morris Sunshine present an imaginative paper in which computer simulation is used to examine the relationships between property values and racial composition of neighborhoods undergoing racial transition. They find that there is no single relationship between these two variables which covers all situations. Whether property values rise or fall after blacks move into a previously all-white neighborhood depends upon a variety of other factors. Helen Icker Safa describes life in a shanty town of Puerto Rico which is characterized by social isolation from the outside world but strong social cohesion within the community. According to Safa, the belief that social bonds break down within the urban setting may more accurately describe relations between social classes rather than within them. The unemployed in Syracuse, New York, and Shields, Great Britain, are the subject of a cross-cultural study by Dorothy and Adrian Sirfield. The position of the unskilled and unemployed in the two countries is remarkably similar. The key situational and life chance

differences are between the skilled and unskilled within each country rather than between the unskilled in either.

The final essay is a wide-ranging philosophical discussion of mass society, technology, culture, and free will by Paul Meadows, who pleads for less formality and quantitateness and for a more impressionistic, intuitive style of life, research, and thought. In this context we can examine the article by Rhonda Cassetta, in which the effectiveness of participant-observation as a research technique is examined. The author concludes that the method can be used to gather accurate data if details are recorded soon after observation. Moreover, the participant-observer—in this case Melvin Weiss (to whom the book is dedicated in memoriam), as the observer of schizophrenic families—experiences as much discomfort as the families. This point summarizes the entire book, for the work is about social scientists more than it is about the poor. It answers the question of what social scientists were doing in the 1960's somewhat better than the question of what the poor were like. This is what the editors intended.

*Social Inequality: Selected Readings*, edited by ANDRÉ BÉTEILLE. Baltimore, Md.: Penguin Books, 1969. 397 pp. Paperbound. \$2.25.

TONY BELL  
*California State College, Fullerton*

Béteille's aim is to present stratification not as a distributive but as a relational phenomenon. The approach purports to examine life styles and status systems governing interaction as epitomized in one of the few exciting articles in the collection, Béteille's own contribution entitled "The Decline of Social Inequality?" In this previously unpublished paper, Béteille argues for the increased worldwide importance of racism as a status system. He suggests that biological race is supplanting more traditional systems based on purity and pollution, as in caste society.

The 18 selections, two of which are appearing for the first time in English, are organized into nine parts. Parts 1, 2, and 9 focus on theoretical issues of differentiation and stratification, while also dealing with terminological and conceptual problems of class and caste. The most interesting of these articles is by Dahrendorf, who concludes that power and power structures logically precede other structures of social stratification. Sections 3 through 8 examine

stratification in industrial, agrarian, colonial, and "simpler" societies, with special emphasis on caste and race. The latter selections are, with a few notable exceptions, written in the wholistic impressionistic case study form which is usually associated with cultural anthropological studies. The exceptions include Dahrendorf's summary of changes in industrial class structure since Marx, Worsley's discussion of middle classes and emergent leadership in developing countries in Africa, and Wertheim's article on changing power and status structures in Indonesia. Despite the impressive array of authors including, in addition to the aforementioned names, Runciman, Aron, Ossowski, Wesolowski, Dore, Wolf, Sahlins, Leach, Srinivas, St. Clair Drake, van den Berghe, and Dumont, the book fails as a reader for the following reasons:

1. A paltry six introductory pages and nine additional separate paragraphs unsuccessfully attempt to carry the entire burden of presenting the editor's main scheme, justifying his selections, and sensitizing and guiding the reader through the more than 350 pages of material. There is, however, both a subject and author index.

2. The articles, eight years old on the average in 1969, do not acquaint the reader with either newer data or newer directions of thought; this is particularly obvious in the selections dealing with black-white relations in the United States.

3. In general, there is an overweight of conceptual-terminological problems. Conspicuous by its absence is the problem of methodology, which apparently is a dirty word in Béteille's vocabulary. He states about some American contributors: "The poverty of their approach derives from an obsessive concern for 'methodology' and scientific precision at the expense of sociologically significant problems" (p. 10). Although I agree that many stratificational studies conforming rigidly to strict survey design and precise mathematical data manipulation have had picayune impact on the field, I see no sense in a forced choice between "methodology" and "significance." I agree that the relational approach to stratification is more difficult to operationalize than the distributional approach. However, a study such as Laumann's *Prestige and Association in an Urban community* illustrates that it can be done. Methodology, whether overtly recognized or not, is inevitably part of any scientific inquiry. The lack of sufficient organization and breadth probably limits this book's appeal as a supplemental undergraduate reader.



*Castes, Old and New: Essays in Social Structure and Social Stratification*, by ANDRÉ BÉTEILLE. New York: Asia Publishing House, 1969. 254 pp. \$10.00.

RONALD A. KLOCKE  
Mankato State College

This book is an overview of the sociological, anthropological, historical, geographical, and political consequences of changes in status and power in India since independence (1947). Of the various approaches used, the discussion of the political consequences is the most thoroughly and effectively handled.

In the Introduction, Bêteille develops a rationale for his selection of specific concepts to be emphasized. Chapter I considers the effect of race and descent on status categories in India; Bêteille asserts that these "traditional" variables produce some of the major cleavages in Indian society. He cautions against facile generalizing, stating that "variations between castes are often overshadowed by sharp differences within them" (p. 39)—which he demonstrates in a detailed analysis. Chapter II examines the institutional changes that have occurred during the past century and the effects of these changes on social differentiation. Bêteille argues "that the relations between caste on the one hand and power and alternative forms of status on the other are becoming increasingly indeterminate" (p. 5), primarily due to changes in landownership, occupation, income, education and political power. Chapter III describes the demographic, educational, political, economic, and social plight of the Harijans or Backward Classes. While Bêteille states that "things are getting better all the time" for these people, he pessimistically asserts that economically and educationally they continue to remain backward, and that 35% of them still remain agricultural workers (p. 99). Chapter IV discusses the future of the Harijans and suggest that this future holds a variety of positive choices and opportunities: e.g., the avenues of 'Western education, the expansion of caste-free occupations and the possibilities of geographical mobility all enable the individual to change his style of life on his own" (p. 132). It appears that political action is used as a viable alternative to achieve social acceptance when these other avenues are blocked. Chapter V examines the effect of caste on the political process; and it provides the basis for the discussion in Chapter VI of an extensive case study of the effects of caste on politics in Tamilnad. The description here becomes somewhat tedious to the non-Indian; perhaps some of it could have been

shortened. Chapter VII contrasts the concepts of status group and class by emphasizing the role of endogamy in bringing about status change. Caste endogamy was very strong but appears to be weakening considerably, thus producing a very heterogeneous social structure and a blurring of caste lines. Chapter VIII describes the life styles, social activities, and recruitment of military, civil service, professional and political elites. The final summary chapter suggests avenues for further research.

The book provides an inside view of contemporary caste orientations in India which will be helpful to the student of caste. Bêteille's conclusions regarding the effects of endogamy and the possibility of the emergence of a dual (old-new) stratification system warrant further empirical investigation.

*The Old Colony Mennonites: Dilemmas of Ethnic Minority Life*, by CALVIN WALL REDEKOP. Baltimore, Md.: The Johns Hopkins Press, 1969. 302 pp. \$10.00.

CLARK S. KNOWLTON  
University of Utah

Famous for its turbulent history celebrated in border ballads and folk stories, the Mexican state of Chihuahua has provided a haven for several groups escaping religious persecution in the United States and social and government pressures in Canada. From Utah came the Mormons toward the end of the 19th century, in search of a potential area of settlement if they decided to migrate from the United States. Settling in a small cluster of villages near the town of Nuevas Casas Grandes, the Mormons have flourished as fruit farmers and ranchers. The German-speaking Old Colony Mennonites came in the 1920's to Chihuahua and its neighboring state, Durango, seeking physical and cultural isolation and tolerance for their unique religious ways of life.

In this comprehensive study of the Old Colony Mennonites in Mexico, Redekop begins by presenting an inadequate sketch of their history. He traces their origin to the massive Mennonite migration from Russia to Manitoba, Canada, in the 1870's to escape increasing Russian pressures to acculturate and assimilate. The Old Colony Mennonites evolved from the conservative Mennonites who recreated in Manitoba the same type of autonomous farm village communities, integrated around religious values and governed by religious leaders, that they had known in Russia. Other Mennonites settled among diverse ethnic and religious groupings.

These gradually purchased new farm equipment, modernized their farming operations, and, influenced by their neighbors, permitted the provincial authorities to improve the educational level of their schools by using English rather than German as the medium of instruction. The more conservative Mennonites, regarding these trends as wayward and backsliding, gradually drew apart from the more liberal groupings and in time formed a specific Mennonite sect. In brief, the Old Colony Mennonites migrated to Mexico to escape secularization, nationalism, and increasing government encroachment upon their school systems.

A detailed analysis of the value systems of the Old Colony Mennonite communities is followed by a discussion of the social structure of the village communities and the role of the Church, the forces of social control, the drive toward conformity, village autonomy, equalitarianism, the concentration upon minor traits such as rubber-tired tractors to measure conformity to traditional values, and the gradual subtle differentiation of different villages in minor points of doctrine and practice. The last half of the book is devoted to an interesting analysis of the impact of the growing social and economic interaction with the surrounding Mexicans, the gradual ending of physical and social isolation, and the increasing influence of Mexican government agencies upon the life of the settlements. The author would have strengthened his presentation had he compared the Old Colony Mennonite beliefs and practices with those of other Mennonite groups that evolved out of the Canadian migration.

The Old Colony Mennonites rest lightly in the various countries in which they live. They seem always willing to migrate, often at considerable financial sacrifice, to escape secularization, nationalism, and mass conformity. However, sympathetic governments with large amounts of sparsely populated, good farming land to settle are growing fewer; and Redekop questions with regret whether the Old Colony Mennonites can continue long to resist social pressures toward conformity. He argues that the Old Colony Mennonites do not fit the traditional typology of church and sect, nor Wirth's definition of a minority group. On the other hand, he states that Gordon's schema of cultural versus structural assimilation does cover the situation in which the Old Colony Mennonites find themselves. On the basis of a religious principle an ethnic group struggles to preserve a unique way of life. The ethnic group inevitably comes under pressures toward conformity from the larger society in which it lives. A minority situation is created which carries with it connotations of

inferiority, social distance, and cultural differentiation. As minorities disappear, new ones inevitably come into existence, given the propensity of men to follow a different voice from that which inspires their neighbors.

Redekop's excellent study points up the need to pay additional attention to the many varied minority groups in the United States and neighboring countries, in addition to Blacks, Mexican Americans, and American Indians, before an adequate theoretical structure can develop about the formation, characteristics, and social evolution of minority groups.

*Racial Violence in the United States*, edited by ALLEN D. GRIMSHAW. Chicago, Ill.: Aldine Publishing Co., 1969. 553 pp. \$12.75.

ARNOLD A. SIO  
*University of the West Indies, Jamaica*

Among the large number of books on violence that have appeared in recent years, many of them of indifferent composition, this one is excellent. Grimshaw had several reasons for editing this book. He was concerned to place the recent expressions of racial violence into historical perspective in order to correct the ahistorical nature of much of the current interpretation. He also thought it important to present explanations of violence that are representative of the various perspectives offered by scholars. His third reason was in part professional and in part personal; that is, the need for Grimshaw, whose own work on racial violence began more than a decade ago, to review both the history and the interpretations of racial violence in the light of more recent events, especially since 1964. Finally, the book is intended to provide the policy-makers and scholars with a basis for an informed and meaningful response by government and the citizenry to the problem and issues of racial violence in America.

It is difficult to do justice to an anthology, but a summary of the major sections and some of the papers included should at least give a general idea of the contents. The criteria employed in the selection of the materials follow from Grimshaw's reasons for doing the book. The anthology consists of four sections divided into twelve chapters that encompass the history of Negro-white violence, patterns of racial violence, cause of racial violence, and the changing meaning of violence.

The historical section makes up about half the book and contains descriptions of slave rebellions and racial violence during the Civil

War and Reconstruction, the Second Reconstruction and the initial years of the Great Migration, World War I, the Depression, World War II, and the post-war period to 1963. It is made abundantly clear that racial violence in particular and lawlessness in general have long been part of American life and have involved many groups in the society. These materials reveal, as Grimshaw indicates in his own paper on lawlessness and violence, that over the years Negro-white relations, especially in the South, have been of the accommodative pattern of superordination-subordination with the whites as the dominant group, and that intense conflict occurred when the subordinate minority disrupted the pattern or was perceived by the whites as attempting to do so. Negro-white conflict has been generated by the breakdown of the accommodative pattern, a pattern that was essentially unstable because the subordinate black group refused to accept its status and had the power to challenge it. Racial violence, then, is the result of attempts to alter the accommodative structure. The papers dealing with the assault upon this structure in the 1960's make up the final chapter in this section. These papers become increasingly analytic and interpretive, serving as a transition to the next section of the book.

The second part of the book deals with the efforts to delineate repeated patterns in American racial violence. Included are papers concerned with comparisons with Great Britain, characteristics of rioters, ecology of rioting, the riot process, the riff-raff theory, and patterns in the disorders of the 1960's. There are, among other papers, selections from the report of the National Advisory Commission on Civil Disorders and one of the supplemental studies for the Commission by Fogelson and Hill.

The third section contains overlapping papers on the question of causation, organized into two chapters on empirical generalizations and theory. The first chapter contains the early work of Hovland and Sears on the frustration-aggression hypothesis and the correlation between economic variables and lynching, as well as the later attempt by Mintz to refine these findings. Also included is the more recent research of Lieberman and Silverman on the "immediate precipitants" and "underlying conditions" of race riots in urban areas that have and have not experienced these disorders. The final paper is a comparative one by Gurr on the relationship between persisting deprivation and the magnitude of turmoil. The papers in this chapter represent increasingly sophisticated approaches to the problem of causation.

The second chapter in this part of the book

contains an excellent selection of theoretical papers on violence. The two perspectives in terms of which explanations have been pursued, one centering on the individual and the other on the social structure, guided the choice of papers. The initial paper by Grimshaw treats the individual's location in the social structure in relation to his choice of one of these perspectives. The papers that follow range from the psychological to the sociological and include the psychoanalytic work of Sterba, the social psychological approach of Clark, the more sociological social psychology represented by Ransford, and the selections by Grimshaw, Waskow, and Williams in which the theoretical perspective focuses on the social structure within which the violent behavior of individuals takes place.

The final section of the book represents Grimshaw's view that the professional student of society has obligations as a citizen as well as a scholar. Here are selections by Grimshaw and Janowitz in which the scholar as citizen seeks to bring his knowledge and skill to bear on the social problems of a society, suggesting ways of responding to legitimate demands for change and maintaining viable democratic institutions.

An unusual feature of this anthology are the several contributions by Grimshaw himself. The quality of these contributions not only explains the excellence of this volume but also makes clear Grimshaw's own perspective and reveals his maturity and originality as a student of racial violence. His personal contributions to this anthology leave us with the keen desire for a book from him on racial violence that is entirely his own.

*Political Violence: The Behavioral Process*, by H. L. NIEBURG. New York: St. Martin's Press, 1969. 184 pp. Clothbound, \$5.95. Paperbound, \$2.50.

*Violence in the City*, by BLAIR JUSTICE. Fort Worth, Tex.: Texas Christian University Press, 1969. 289 pp. \$8.00.

ROBERT BLAUNER  
*University of California, Berkeley*

The interest in violence which these books share is pursued from very different angles. Nieburg, a political scientist, presents a general and fairly abstract synthesis of behavioral science theories. Justice, a psychologist, journalist, and former human relations deputy to the mayor of Houston, gives more empirical and pragmatic interpretations that befit his day-to-

day involvements in the strategies of conflict resolution. Interestingly the authors clash in their appraisals of the value of violence in social change—a point I will develop later.

*Political Violence* is impressive. It is a rare social scientist today who can develop a theoretical model of societal process in a style that can be grasped by undergraduates while being enjoyable. Nieburg is also a good critic of the many theories that purport to explain the apparent upsurge of violence in American society: the frontier theory, the gun proliferation view, the McLuhan perspective, the killer-instinct theory (Lorenz, Ardey, *et al.*), and many others. His own view stresses the political dimensions of group violence which he places in a context of bargaining theory. Since shifting relations between human groups is a universal fact of societal process, the power relations within a nation-state are always more-or-less out of kilter with the emerging balance of social forces. New constituencies kept out of the power system use violence as a resource to force political changes. The black rebellion is the most important contemporary example. There are dangers in the escalation of violence, but Nieburg sees reintegration of the society on more inclusive bases as the most likely outcome of urban conflict. This summary cannot do justice to the subtlety of the author's analysis of the dynamic interaction involved in much racial conflict today, and the sharp insights offered on the situation and tactics of other contending groups such as youth.

*Violence in the City* contains much material of interest on race and social conflict in Houston, Texas. The research team interviewed 7,000 people in the black community, and the book also includes demographic and documentary data on conditions in "Space City U.S.A." The attitudinal tables suggest that Houston Negroes are predominantly "moderate," oriented toward entry into the American mainstream. Perhaps the most illuminating findings document the effect of dramatic conflict on social attitudes. In the course of the study, confrontation and violence broke out between the police and students at Texas Southern University. Contrasting comparable samples interviewed before and after this crisis, Justice found that negative appraisals of the police treatment of blacks increased from 37% to 67%, support for Muhammad Ali's religious deferment rose from 39% to 61%, and support for disruption, violence, and black power increased, though not as drastically.

Justice's book distills much of the Houston race relations experience; it is filled with programs and suggestions for reform in police-com-

munity relations, jobs, housing, and education. There are good ideas here and experiences that other cities might consider. However, the author is somewhat uncritical of his secondary sources, particularly those dealing theoretically with such internal "Negro problems" as crime, family structure, personality, and identity. Though the author explicitly warns against such a tendency, the burden of the books seems to be the pathology of the ghetto at the expense of black community and cultural strengths. Though an avowed pluralist, Justice is generally negative, sometimes even condescending and contemptuous, toward black nationalism and black power. Whereas Nieburg sees these dynamics as positive forces which help unify Afro-Americans, Justice is apprehensive of unity around militant positions: he wants blacks and whites to see one another as individuals who happen to belong to racial groups that are more remarkable for inner diversity than for uniformity.

In his introduction, Justice states, "I cannot ever accept the notion that violence brings good." Here the author must be talking about the violence of the oppressed because his emphatic chapter on "The Blue Minority" certainly accepts the necessity and value of the legitimate violence of state representatives. Nieburg, on the other hand, sees violence and particularly the threat of its use by relatively powerless, though rising groups, as one of the main instruments for restoring long-run social stability on a more democratic basis. He feels that the extremism of the current black movement has opened more doors in the past few years than the nonviolent strategy of the previous decade. Justice sees such extremism as threatening all the progress for which Houston's white liberals and black moderates have been working so hard. Perspectives differ, and this vital question may not be settled in our day. In the meantime each author could read the other's book.

*The Politics of the Southern Negro: From Exclusion to Big City Organization*, by HARRY HOLLOWAY. New York: Random House, 1969. 374 pp. \$8.95.

CHARLES E. GARTH  
Morehouse College

It is refreshing indeed when one discovers, among that surfeit of "publications" catering to the current vogue for Negro or Black topics, a book which actually makes a cogent contribution to knowledge. In the first two chapters, the author presents the basic question he seeks to answer and the rationale for his particular ap-

proach to that answer. The question is, "Was it not possible to see the political emergence of the Southern Negro as a part of some overall pattern?" The approach to an answer is based upon two assumptions: "that the South may be regarded as a traditional society that has been modernizing for several decades, and that the Southern Negro may be viewed as an ethnic group comparable to ethnic groups in the North."

The second part of the book consists of case studies of Negro political action in various Southern communities, including Mississippi ("Traditionalism and the Freedom Democratic Party"); Fayette County, Tennessee ("The Quest for a Negro Majority"); Macon County, Alabama ("Emergence of a Negro Majority"); Marion County, Texas ("The Manipulated Vote"); Birmingham, Alabama ("Urbanism and a Politics of Race"); Atlanta, Georgia ("Conservative Coalition"); Houston, Texas ("Liberal Coalition"); and Memphis, Tennessee ("Independent Power Politics"). It is in these case studies that Holloway makes his real contribution, even though they vary in quality. With commendable skill and precision, he weaves together information from interviews, mass media, documents, and observation. The result is a convincing narrative of Negro political activity in each community. His distinction between "ethnic politics" and "the politics of Race" may help to clarify his assumption regarding the comparability of Southern Negroes and northern ethnic groups in the political arena. However, certain nagging questions remain. For example, he posits the poverty-hate syndrome to explain difficulties in Atlanta, but does not explain the "liberal" coalition in Houston between poor persons of diverse ethnicity.

In the final chapter Holloway not only presents an analytical summary of the political emergence of the Negro as the South shifts from "old" to "new," but also attempts to vindicate the image of the Negro in America. He alludes to the seldom-recognized fact that, apart from his viability in politics, the mere presence of the Negro in the United States today—indicating Trojan survival capacity—is sufficient testament to the Negro's relative worth vis-à-vis any other ethnic group in this land. Unfortunately, some passages are not sufficiently explained and therefore may be misleading—for example, the assertion that "In general one can conclude that much is possible in arousing the Negro masses if the churches help and little is possible if they oppose" (p. 331). Hopefully, Holloway does not intend to imply that Negroes are natural religious

zealots; rather, he recognizes that these have been the only institutions Negroes really controlled. Another example is a very unfortunate passage on page 338: "Among such possible shocks to the system the most extreme would be a global race war in which the world's whites would be pitted against the world's dark skinned people. . . . One has only to recall what happened to the West Coast Japanese in World War II to appreciate the hardships that a global race conflict might inflict on American Racial minorities." Holloway disregards three basic facts: (1) the Japanese were less than 1% of the population; the Negroes are more than 10% (a more formidable fifth column if they wanted to be); (2) the degree of "hardship" imposed would depend to a certain degree, at least, on which side wins in the event of a global race war; and (3) according to studies employing the Bogardus social distance scale, the acceptance of Japanese in America increased after the bloody battles of World War II.

Despite its few defects, this book has tremendous value on the basis of sheer information. It could well serve as a primary text for anyone interested in local politics. In addition to Holloway's skillful treatment of his particular problem, there is some serendipity value in the book: the case studies provide suggestions for further examination of Oscar Lewis' notions on community organization and poverty, Scott Greer's on the spiral radiation of political interest from neighborhood to state, and Davis' on rising expectations as a force in social change. Finally, the book serves as one of the more salient indicators that Regionalism is not dead.

*The American Right Wing: Readings in Political Behavior*, edited by ROBERT A. SCHOENBERGER. New York: Holt, Rinehart and Winston, 1969. 308 pp. Paperbound. \$4.95.

JOHN C. POCK  
*The Johns Hopkins University*

Were it not for a lot of speculative nonsense propounded by some rather prolific members of the sociological brotherhood, this collection of papers would not exist. More importantly, the dissertations on which six of these nine papers are based would not have been written. But social scientific folk wisdom ordains that political behavior departing from some unstated kind of wishy-washy, muddled golden mean is "extremist." Moreover, since extremists have some strange ideas about their opponents and the body politic, it follows that such people

must be cripples suffering from profound personal disorders and/or expressing a variety of sociological strains, stresses, and other dysfunctions. Therefore, it becomes necessary to describe, study, and psychoanalyze this intellectual artifact and even, perhaps, to find out whether it exists as more than a fiction.

Taken as a whole and ignoring the authors' separate purposes, this collection has the virtue of asking a simple question that social research ought to pose about generalizations, scientific or otherwise: Is it really so? Each of these studies (by five political scientists, three sociologists, and one psychologist), contains a rather simple, low-key description of people who, because of some overt voluntary act such as organizational membership, meeting attendance, or letter-writing, have been designated as right-wing, conservative, super-patriot, or radical rightist. None of the papers is sufficiently definitive to dispel the usual imagery of such people; each study suffers from very severe, usually unavoidable, defects in sampling and research design. Nonetheless, despite this, the incomparability of data, and the small numbers, the over-all picture one obtains of the Christian Crusade or John Birch activist, the Goldwater supporter or the Conservative Party member, is that of politically involved, dedicated citizens operating within the political system. Regardless of how much one might disagree with them—yes, they are rather fundamentalist in more ways than one—these people are walking around loose doing their socio-political thing without freaking out.

Most of the studies are at their best when they describe findings within the framework of their methodological limitations. The lone psychologist stays with the tools of his trade and restricts his interpretation to the variables of his discipline. Typically the others, especially the sociologists, are disposed to move beyond their data into psychologistic flights of fancy aided by rather tortuous reasoning. But the psychologist, after depth interviews with some Dallas volunteers, provides a good, if irreverent, summary of this whole fad in political sociology: "Why then did these rightists choose the John Birch Society rather than numismatics? [Because it] involved all the advantages and few of the disadvantages of coin collecting: there appear to be many more rightists than numismatists in Dallas; rightist social events are more frequent; the hobby is not very expensive; the relevant literature is easy to come by, and eliminates the necessity to spend time on biased national newscasts or mass publications; and certainly radical rightism deals with more important issues than numismatics. Few

people have a pleasant hobby which can also save the world from disaster."

As several of the authors point out but others ignore, the subjects of all these studies had been selected because they were active in the political arena. So why is it so surprising that such people have strongly held opinions, deeply felt convictions and beliefs? Why is it necessary for the social scientist, apparently oblivious to the facts of political life, to interpret this pejoratively as dogmatism or stereotypy? Why must social research take what people say so seriously as to transform political rhetoric into, at best, an "ideology"—or worse, as indicative of "underlying psychopathology"? Thus, in one of the better-executed studies reported here, the Christian Crusaders' "obsession" with the Communist Conspiracy is used as the index for "trust in government." One cannot help wondering about the conceptual status of this 1962 indicator in the light of the current Agnew-Mitchell hegemony.

This volume might be useful in undergraduate courses as a suggestive antidote to the more popular essays written by pundits in the social scientific community. But it is unlikely that anyone will let himself be influenced by the empirical evidence these readings contain.

*Sociology of Law: Selected Readings*, edited by VILHELM AUBERT. Baltimore, Md.: Penguin Books, 1969. 367 pp. Paperbound. \$2.25.

MURRAY MILNER, JR.  
New York University

This book contains 28 previously published selections, preceded by a brief Introduction by Aubert. The selections are grouped into five parts, each of which has an introduction of 100–150 words. At the end of the book there is a useful list of further readings, an adequate author index, and the usual not-so-adequate subject index.

Part I ("Law and the Social Structure") contains selections from Durkheim's *Division of Labor*, Maine on status and contract, the Marxist Karl Renner on legal institutions and property, Thurman Arnold on law as an ideological symbol contributing to stability, and Harry Bredemeier's analysis of the legal system using Parsons' AGIL scheme. Part II focuses primarily on the extent to which traditional norms determine the shape of a society's formal laws, versus the degree to which legal innovation can shape popular sentiments. It includes articles by A. V. Dicey, O. Kahn-Freund,

Yehezkel Dror, Jan Gorecki, Aubert, Michael Banton, and Adam Podgorecki. Part III deals with law and the process of conflict resolution, and includes an especially useful article by Torstein Eckhoff on the difference between mediators and judges. Part IV ("Judicial Behavior") includes selections by Glendon Schubert, Roger Hood, Harry Kalven and Hans Zeisel, and Julius Stone. The concern here is with the factors which influence the outcomes of judicial decision-making. Part V has six articles on the legal profession, including the excellent article by Dietrich Rueschmeyer contrasting the legal and the medical professions.

This is a useful collection of readings, many of which are fairly inaccessible except to the persistent researcher. Their quality is uneven; and, with notable exceptions such as the Kalven and Zeisel article, the level of methodological and theoretical sophistication tends to be on the low side. This, however, is probably an accurate reflection of the fact that sociology of law has been a relatively neglected sub-discipline.

This book suffers from the lack of coherence and integration characteristic of readers, but in this case the problem is accentuated by at least two factors. Apparently the editor was restricted by the publisher's format for the general series (their reader on social inequality also has five-and-one-half pages of general introduction and 100-150 word introductions to each part). This is very unfortunate, since the literature from a field like the sociology of law is much less central to the main body of sociological literature than a field like stratification. Consequently, more introductory material is required in order to place things in context adequately. The problem of integration is further aggravated because the selection is deliberately international and draws on studies from a variety of societies. From a comparative perspective this adds to the book's value, but it also increases the need for more adequate introductory material.

There is a minor but annoying editorial flaw. In the Table of Contents the date of each selection is given. But instead of setting the historical context by indicating the date of original publication, the date given is that of the English edition from which the selection was taken. This means that the student new to the field is easily misled into thinking that Weber, Maine, and Durkheim were writing in 1954, 1960, and 1964 respectively. Finally, the binding is of an exceptionally poor quality; my review edition came apart before I was one-fourth into it.

*Law in Culture and Society*, edited by LAURA NADER. Chicago, Ill.: Aldine Publishing Co., 1969. 454 pp. \$10.75.

F. JAMES DAVIS  
*California State College, Fullerton*

Most of these papers were presented at the second Wenner-Gren Conference held in Austria in 1966. Thirteen of them are by anthropologists, two by sociologists, and one by a law professor. Ten of the contributors are American, three are English, and one each are Norwegian, French, and Dutch. Ten of the anthropological papers are discussions of cases of dispute-settlement in tribal settings in Africa, Oceania, and the Americas: two are essays, and one is a description of different styles of court procedure in a Mexican town.

Part I, "Case Studies of Law in Non-Western Societies," includes papers by P. H. Gulliver, Laura Nader, E. Adamson Hoebel, André J. F. Köbben, Raymond Verdier, and Charles O. Frake. In the introduction to this section, Gulliver notes an early agreement at the Conference not to waste time trying to agree on a definition of law. He contends that anthropological studies of law must include the pre-history of disputes and the consequences of settlements, not just the dispute-settling process. He also says most field research has been limited to adjudication, while he includes negotiation.

Part II, "Law and Innovation in Non-Western Societies," is concerned with the effects of personality factors on law. Papers by Leopold Pospisil, Isaac Schapera, and Richard P. Werbner involve instances in which individuals brought about changes in the processes of dispute-settlement. The contribution by James L. Gibbs, Jr., stressing the effects on legal processes of personality patterns that are common in the society, is more explicitly socio-psychological.

In Part III, "Case Studies of Law in Western Societies," Herma H. Kay uses the "trouble-case" approach to describe a California experience in administrative law. Gresham M. Sykes summarizes some of the evidence concerning congestion and delay in American courts. Vilhelm Aubert discusses law as conflict-resolution in a small industrial society (Norway). Aubert's introduction to Part III is a valuable commentary on some of the problems of developing the sociology of law, especially the division of labor between legal scholars and sociologists.

In Part IV, "Comparative," Sally Falk Moore's paper treats the law of descent in one primitive society. Essays by Max Gluckman

and Paul Bohannon, and comments on them in Moore's introductory statement, provide the most explicit discussions in the book of different basic assumptions and modes of conceptualization. Gluckman contends that jurisprudence provides categories and concepts that are useful in anthropological studies of law. Bohannon argues for the use of native terms rather than those of developed Western legal systems. Underlying these positions are basic differences in theory and in the philosophy of social science.

Judging by Laura Nader's comments, there was more awareness at the conference of divergences in approach than of convergences. Some new questions were raised and some working assumptions became more explicit. The major agreement seems to be that anthropological studies of law should continue to focus on what Llewellyn and Hoebel taught us to call the "trouble-case."

*Law in a Changing America*, edited by GEOFFREY C. HAZARD, JR. Englewood Cliffs, N. J.: Prentice-Hall, 1968. 207 pp. \$5.95.

RICHARD LEMPERT  
*University of Michigan Law School*

This collection of essays, prepared as background reading for a conference sponsored by the American Assembly and the American Bar Foundation on the goals of the legal profession in the years ahead, begins and ends with a bow toward changing America. The first chapter is an attempt by sociologist Wilbert Moore, the only non-lawyer among the essayists, to sketch generally the patterns of social and political structure likely to pertain in the near future. This chapter, while perhaps valuable for the conferees, contains nothing new to the sociologist. A measure of the change occurring since the essay was presented (March 1968) is that the continued endurance of certain American values (e.g., national patriotism) and procedures (e.g., the democratic vote, bargaining and negotiation, and the judicial process) whose staying power was then understandably assumed, now seems at least somewhat problematic. The Appendix by Roger L. Price, then a law student, reviews several standard works containing population or economic projections and discusses briefly some implications of predicted developments. This too contains nothing new to the sociologist.

The remaining chapters cover such diverse topics as the problem of protecting the individual from big government, the relevance

of economic analysis to the antitrust laws, empirical inquiry and legal policy, serving the legal needs of the poor, regulating professional qualifications, and legal education before, during, and after law school. While these chapters may discuss material new to the sociologist, most of them are not sociologically interesting. No essay presents new empirical research or attempts to tie discussion to established sociological theory. In addition, many of the essays, which average sixteen pages in length, deal with topics that deserve to be and have been the subjects of books at least as long as this one. None of this is surprising, given the authorship of the essays and the purpose for which they were prepared.

The most which one can ask from a volume such as this is that the chapters stimulate the reader to think more deeply about a particular subject matter, suggest a fresh approach for thinking about a particular topic, or provide one or two new insights into a problem area. The quality of the contributors is reflected by the fact that many of the essays do at least one of these. Louis Pollak's comments on the role courts play in protecting individual rights to privacy in mass society, and David Caver's call for specialization in and among law schools, are two contributions which are particularly stimulating.

The chapter most obviously relevant to sociology, Harry Kalven's essay "The Quest for the Middle Range: Empirical Inquiry and Legal Policy," is one of the best in the book. Kalven makes the point that the application of social science to legal problems bears on legal decision-making only where facts are in a middle range. At one extreme are legal decisions, such as the decision outlawing *de jure* segregation in the public schools, which are rooted in such fundamental values that the empirical balance of good and bad effects should not motivate the decision maker. At the other extreme are cases where relevant factual premises are clear enough so that the precision which social science can bring to the inquiry is not needed. Three further points apply to "middle range" facts. First, even when norms and values are not deeply held, facts do not *per se* resolve value issues. Thus, empirical science may be able to tell us that jury trials are 40% longer than bench trials, but it does not tell us whether juries should be retained in civil cases. Second, legal rules and institutions are multifunctional. Social science evaluation must take into account the variety of ends served. And, third, legal decision makers do not wish to delegate policy choices to scientific experts. For social science learning to have an impact on



the living law it may (at least in certain areas) have to become popular learning and enter law via normal political processes. Many of the other chapters illustrate, without intending to do so, one or more of Kalven's points.

This book seems well-suited to its intended purpose, as a starting point for a discussion of legal institutions and the legal profession. Its value to the social scientist is, however, limited. Unless one is looking for a broad overview of some of the issues currently concerning lawyers and law professors, even the sociologist interested in the law would be well-advised to spend his reading time with other volumes.

*Kids and Cops: A Study in Mutual Hostility*, by DONALD H. BOUMA. Grand Rapids, Mich.: William B. Eerdmans Publishing Co. 1969. 168 pp. Paperbound. \$2.95.

BRIAN VARGUS  
*University of Pittsburgh*

This book draws on interviews with "some 10,000 students in ten Michigan cities and over 300 police officers in three cities" in order to ascertain attitudes concerning police held by junior high school students and to compare them with perceptions police have of young people. According to the author's Preface, the discussion is focused on the "three cities where both police and youth were questioned." The first chapter indicates the goals of the work—to gain, in light of recent riots, new information about police community relations and particularly police-youth relations in the inner city. The rest of the book demonstrates that the author's goals are rarely achieved.

The promise of the study is unfulfilled due to inadequate attention to basic issues in the study design and to shoddy data analysis. A few of the more glaring errors are evident in the reports of simple percentage distributions with no indication of the total number of cases involved, the omission of all but the most simple of cross-tabulations, and lengthy discussions of spurious relationships or relationships whose significance is unclear. The failure to report the number of cases involved in the percentage distributions makes it impossible for the reader to know the character or composition of the subject population. The author indicates he has interviewed 10,000 students in grades 3 through 12, but he claims his primary focus is junior-high-age youngsters. In the second chapter, the figures concerning "Youth Attitudes" presumably do not refer to all 10,000 students. When the sample is divided into racial groups,

no indication of number of cases is given. This is crucial in a work that consistently asserts the significance of race for youth-police attitudes. Later, in a chapter on police perceptions of young peoples' attitudes about police, the author claims to compare police attitudes with attitudes of inner-city youth. A comparison of some of these figures (e.g., pp. 121, 125) with those reported earlier seems to confirm that the author has classified all Negroes in his sample as "inner city." It is somewhat staggering to see in print what appears to be the naive assumption that all Negroes are "inner city" residents and all "inner city" residents are black!

The simple cross tabulation technique using percentages frequently obscures the spurious nature of the apparent relationships. For example, the author reports that parochial school students are more favorable to the police than students in public schools, and discusses this pattern at some length. Later he notes (pp. 80–82) that this is all spurious since, when other things associated with favorableness toward the police (e.g., SES) are controlled, the differences "disappear or become statistically insignificant." He mentions also that the differences are due to non-comparable samples since the "publics" included the 9th grade, while the parochial school students were all 7th and 8th graders. The author indicated earlier that the older a student became the less favorable he was likely to be toward police. The data clearly demanded more complex techniques of analysis such as regression analysis. Computation of significance tests for those few percentage tables where it is possible to ascertain the number of cases involved establishes that most of the reported differences are significant but that the strength of association is almost always quite low.

Overall, the continuing pattern of "methodological disasters" renders the results uninterpretable and the book suspect. Beyond this, there are no results, conclusions, or recommendations that are surprising or unique. When graduate students complain about "abstracted empiricism" and its dangers—particularly its conservative bias—I am usually unimpressed. This book has strengthened their position. At first I thought it might be salvaged as a reader for police training. However, all the book would serve to do is reinforce the conservative biases of the police. For example, the author points out that the police were "accurate in their assessment" (p. 123) that regular church-attenders are more favorable to the police. Earlier (pp. 65–66) he indicated that church attendance had "slight" or no influence on perceptions of police when other important variables like race and

SES were controlled. In one spot this book "writes" its own epitaph. Discussing police-community relations Bouma writes: "Simplistic stereotyping illumines nothing and serves only the purpose of demagoguery."

*Judging Delinquents: Context and Process in Juvenile Court*, by ROBERT M. EMERSON. Chicago, Ill.: Aldine Publishing Co., 1969. 293 pp. \$8.95.

JON E. SIMPSON  
*University of Massachusetts*

As criminologists have become disenchanted with concepts primarily derived from the structural-functional perspective and/or based on "official" data pertaining to youth known to legal agencies, research endeavors of the past decade increasingly have favored the societal-reaction approach and have depended on evidence obtained by self-reporting and participant-observation techniques. This study of basic procedures and functions of the juvenile court draws from both of these distinctive theoretical heritages. Parenthetically, the former is termed "institutional analysis," which seems inappropriate in view of the recurring references to structural and functional determinants of the court's operations, responses to juveniles, and relations with a limited number of specific community agencies. The substantive materials reflect greater reliance on the societal reaction approach, and Emerson clearly excludes detailed examination of the labeling processes prior or subsequent to court contacts. Given the court's dual concerns with treatment and control, the central theme is that case management and processing depend heavily on assessments of delinquents' moral character, essentially established during confrontations within the court setting.

The analysis is based on a case study of an unnamed juvenile court located in the central district of a large northern city. As a participant observer, the researcher stresses the interaction between delinquents and court officials at formal hearings and probation interviews. Direct observations are supplemented by interpretive reactions from the professional agents; but the juveniles' reconstructions, if solicited, are not reflected in this report. Moreover, the volume lacks summary data about the offenders beyond three tables compiled from annual court statistics on disposition of complaints and types of offenses. The distributions are fairly typical, except that only 66 of almost 1,500 complaints

resulted in commitments to correctional institutions.

Part I reviews the social forces that set the parameters for the actual processing of cases. After a cursory discussion of the setting, powers, procedures, and internal organization of the court, Emerson details the extent to which the court's relations with enforcement and welfare agencies impede achievement of a treatment orientation. Although the court is fairly successful in resisting pressures from the political system and enforcers, treatment goals are displaced in the course of meeting demands for emergency, sanctioning, and "dumping" services from child-care organizations. On balance the court is portrayed as a "back-up institution" fundamentally concerned with controlling juvenile behavior and preventing serious delinquent activity.

Part II explicates the processes by which the court evaluates and manages troublesome youths. In sifting out those juveniles thought to require special handling, the court relies primarily on its judgments and inferences regarding the moral character—i.e., normal, criminal, or disturbed—of the delinquent actor. The three classes supposedly correspond, in turn, to three dispositional alternatives: release or routine probation, incarceration, and special care and treatment. Several chapters examine "the ways in which agents can present, discredit, or build up a delinquent's character, in which the delinquent can defend character against the discrediting implications of misconduct and denunciation, and in which the delinquent reveals character within the ceremonial structure of the courtroom event."

Throughout encounters with the probation or clinic staffs, considered in Part III, the juvenile's moral character is particularly vulnerable to disparagement. By emphasizing treatment as well as control in these programs, court personnel impose conflicting expectations that are adequately handled by relatively few delinquents.

Whether measured against the author's modest exploratory objectives or the seminal contributions of Cicourel, Lemert, and Matza, *Judging Delinquents* is likely to disappoint both students and practitioners dealing with delinquency. Various shortcomings override Emerson's notable achievements in identifying key dimensions of the labeling process and in describing the salient character of interaction between court officials and delinquents. Little effort is made to inform the reader about the nature and adequacy of the field work. The conceptualization of moral character and its three classes is unclear, despite the reference to "overall behavior, personality, and family and social circumstances." Although the importance of social class and family situation

is occasionally acknowledged, basic background characteristics are not systematically explored in relation to interactional patterns and ultimate outcomes. (My examination of the selected cases committed to the Youth Correction Authority suggest that sex, race, offense, and prior involvement with legal agencies, rather than moral character, account for most of the variance.) In view of the stress on the dynamics and consequences of the labeling processes, it is difficult to understand why Emerson neglects the juveniles' perceptions of their court experiences. Finally, the complete dependence on anecdotal excerpts destroys any sense of continuity in the processing of individual cases through sequential stages and obscures the real meaning and significance of specific episodes.

*Theft of the Nation: The Structure and Operations of Organized Crime in America*, by DONALD R. CRESSEY. New York: Harper and Row, 1969. 367 pp. \$6.95.

RICHARD R. MYERS  
Oberlin College

This book is a description and analysis of the Mafia or Cosa Nostra in this country as a nationwide, organized criminal cartel and confederation which is increasingly moving in to "own" and corrupt legitimate business and government. The book represents a revision and an extensive addition to the author's section in the Task Force Report on Organized Crime prepared for the President's Commission on Law Enforcement and Administration of Justice. Cressey writes in the Preface, "Upon being invited to work for the Commission, I was not at all sure that a nationwide organization of criminals exists. Discussions with my friends and colleagues indicated, and continue to indicate, that this skepticism is widely shared. I changed my mind."

Focusing on organizational aspects of Cosa Nostra as the key to its dominance and power, Cressey analyzes the functionality of particular systems in the division of labor, norms, positions, and positional relationships. After looking closely at organizational origin and development, he concludes that Sicilian-Italian cultural background, particularly with regard to strength of family ties, secrecy, and traditional acceptance of illegal activities and underground authoritarian controls, provided a most favorable context for a very small minority of Sicilian-Italians adapting old country patterns of organized crime to the American situation, and also constituted perhaps the most important

basis for achieving dominance in the gangland wars with other groups in the 1920's and early 1930's.

The author identifies a number of large systems characteristic of the division of labor in each Cosa Nostra "family." The systems are authority, production, sales, corruption, judicial, and law-enforcement. Each contributes to the whole, but some appear to vary in functional importance as conditions change. Cressey directs attention to what he believes to be major changes in patterns and emphasis in Cosa Nostra authority and law-enforcement systems:

We are now witnessing the passing of the days when the rules of organized crime had to devote most of their time and intelligence to insuring that their members were not bad criminals. Either the rulers are securing such a degree of conformity that defection is no longer much of a problem, or they are becoming so respectable and thereby so insulated from law-enforcement processes, that strict conformity is no longer essential. I believe the latter is true (p. 232).

There is an extended discussion of the trend toward acquiring and rewarding technical and professional business skills and competence by Cosa Nostra leaders as they make huge investments in larger and larger businesses. The increasing numbers of professionals particularly appears to produce some modification in methods of control. The "membership books" of Cosa Nostra families have been closed to new members for a number of years. Lawyers, accountants, and other more recently acquired administrative experts are close associates but to some extent are "outsiders." Thus, as is suggested, as ethnic traditions wane and the defensive strength of the inner membership declines, aggressive leadership may be shown by the employe sector with significant effects on policy.

Cressey deals with the several systems of Cosa Nostra's organization as five, six, or seven level bureaucratic hierarchies, with a concentration of decision-making at the top and communication through channels. He notes the survival value of self-perpetuation through continuous replacement in positions and bureaucratic protection from threat both from within and without, particularly for leaders, through insulation and remote control. One of the most stimulating and rewarding features of this book is the comparison of several kinds of organizations, both criminal and non-criminal, in terms of common or relatively unique structures and functions. Cressey proposes that a criminal organization be distinguished as that which has an established division of labor with positions designed for the commission of crime, such division of labor to provide at least one position for a corrupter, one position for a corruptee, and

one position for an enforcer. Positions and positional relationships which are discussed and which appear to be highly functional to a criminal organization of the type of Cosa Nostra are those of "money mover" and "buffer."

Cressey is seriously limited by a lack of detailed information. He indicates that he is able to provide only a "structural skeleton" of the operating organization, and throughout the book reminds the reader of the dearth of detailed information. There is heavy reliance on the Valachi testimony and on Federal wire taps and bugs, with some information gained from the confidential reports of law-enforcement and investigative agencies and from interviews with "knowledgeable policemen and investigators." Nevertheless, Cressey has assembled most of what is known about Cosa Nostra at the present time and has presented it in a very useful book.

*Small Town and the Nation: The Conflict of Local and Translocal Forces*, by DON MARTINDALE and R. GALEN HANSON. Westport, Conn.: Greenwood Publishing Corp., 1969. 211 pp. \$10.00.

RONALD JOHNSON  
*University of North Dakota*

This work essentially focuses on the relationship between a small town in the Midwest and the mass society. This study of Benson, Minnesota, a more quantified version of Vidich and Bensman's *Small Town in Mass Society*, reviews earlier studies of rural communities and provides an analysis of physical and cultural characteristics as well as a historical sketch.

The authors advance three specific hypotheses: (1) "The lines of tension between local and nonlocal forces are discernible in the economy of Benson." (2) "Benson's structure of power and influence is polarized in terms of local and nonlocal interests." (3) "The major lines of stress in Benson's institutions of socialization arise from the conflict of local and nonlocal interests."

Demographic and other social data as well as social psychological measures were gathered from three samples: 38 Bensonites who had lived in the community for several years, 37 new style Bensonites, and 35 ex-Bensonites (people who have left the community). The conclusion of this study is that, as stated in the Preface, "with some minor exceptions the working hypotheses were verified. The general hypotheses were confirmed beyond question."

The best part of the book is the first chapter,

which reviews the literature, provides an excellent theoretical framework for discussing contemporary community theory, and analyzes the situation in which Benson finds itself in the 1960's.

A number of chapters will interest primarily individuals who wish to consider the background material on a community in the central lowlands, while scholars interested in this region and local citizens may find valuable the chapters on the physical and cultural setting and the history of the community. The same is true regarding the significance of chapters describing the social structure of Benson, as well as differences among the three samples. The reader becomes exhausted when he finally gets to a discussion of how Benson's economy has polarized local and translocal forces. The empirical evidence is adequately presented, although in extensive tables. A more interesting presentation of the differences between the three samples is found in Chapter 7, "Conflicting Styles of Socialization." While some tables, particularly those (41 and 42) comparing old and new-style Bensonites on various attitudinal items, are justified, the space given to such detail as the church budget of a local congregation is excessive. The final chapter provides an interesting discussion of the extent to which power and influence in Benson are influenced by the greater society. A brief overview of social control in Benson concludes the book.

Although the book is well documented, both as a history and as a current empirical study, it is unfortunate that to a great extent this is "just another community study." True, we could increase indefinitely our sample of communities studied; but it is now time to give more consideration to discovery of the common generalizations based on community studies (many are in graduate theses) and use their conclusions to build a better body of theory about the community.

*Toward a Theory of Popular Culture: The Sociology and History of American Music and Dance, 1920-1968*, by D. DUANE BRAUN. Ann Arbor, Mich.: Ann Arbor Publishers, 1969. 165 pp. Paperbound. \$3.00.

DORIS Y. WILKINSON  
*Macalester College*

The functionalist mode of sociological explanation has not yielded fruitful systematic analysis of aesthetic institutions and artistic creations. For those with an interest in this facet of man's socio-cultural structure, Braun's

little book, although descriptive in nature, should be welcome. Tracing the evolution, inter-relationships, and uniqueness of song and dance in America, Braun employs three conceptual variables: cultural preconditions, prevailing attitudes in society, and individual creativity. He states that "the introduction of new styles in popular culture . . . is a result of these three elements interacting together" (p. 158), maintaining that his work attempts to construct a theory of "popular culture" while at the same time begin "a direct refutation of the societal reflectionists who hold that the incidence of dancing and music increases in periods of stress" (p. iii). However, occasional contradictions to this specified purpose appear.

Beginning with an emphasis on the development of jazz as a Black and an American phenomenon, this musical style is viewed as forming "a synthesis and interpretation of several folk strains existing in America as far back as pre-revolutionary Puritan New England" (p. 2). Following a description of the jazz era, the "swing" movement of the 30's is interpreted as having had a structural and serendipitous consequence wherein "Integration became a reality . . . prior to this period there had never been any racial mixing—a band was either all white or entirely black" (p. 45).

After the post war period "rock and roll" was conceived, legitimized, and fractionalized as American art styles and the political structure were drastically altered. Multiple innovations in song and dance emerged: folk songs with a moral message, go-go girls, and the disco-hèque diffused as the civil rights movement and the Viet Nam war permeated the normative and structural fabric of American society. White singers imitating Black American musicians became almost a modal stylistic pattern. LSD and marijuana were linked with the behavior of the new artists and in the lyrics of the 60's.

In summary, three common elements are established for dance and music innovation: "(1) the beginning in humble origins, usually among rural Negroes, (2) the gradual changes in the course of its 'legitimatization' process by the white middle and upper classes, and (3) its institutionalization into the culture" (pp. 139-140). Overall, this little volume contains an interesting array of descriptive data on artistic creations. Although this book may never reach major publishing firms, hopefully it will reach social scientists who postulate a theoretical orientation which integrates aesthetic innovations with areas like the sociology of knowledge or with more general explanations of the creative process. In addition, "there is no reason that change or development in popular music

and dance cannot be used as an indicator of broader social and cultural trends" (p. 159). It is also hoped that the expression "popular culture" will be dropped from the existing proliferated labyrinth of meaningless sociological concepts.

*Rural Sociology: Its Origin and Growth in the United States*, by LOWRY NELSON. Minneapolis, Minn.: University of Minnesota Press, 1969. 221 pp. \$6.75.

J. H. JONES, JR.  
Louisiana State University

This is an attempt to account for the origin of rural sociology in the United States and to chart its growth, in light of the Nelson proposition that "any field of knowledge . . . comes into being because of the convergence at the appropriate time, of conditions in the social environment together with the presence of imaginative and forceful men who are stimulated by those conditions to respond creatively in new intellectual formations."

Chapter 1 treats the "conditions in the social environment"—general rural depression reflecting slim farmer credit, insufficient transportation, poor markets, inadequate schools, social isolation, and lack of organization. Most of chapters two through six describe the "imaginative and forceful men" who responded to these conditions. More than a bibliography of the founders of rural sociology, these pages weave some three dozen names (e.g., Bailey, Butterfield, Galpin, Ely, Taylor, Small, and Giddings) into a blow-by-blow account of the struggles of the new discipline. Nelson includes important support from church and lay groups as well as academic institutions in the process. Chapter 7, "The Growth and Spread," treats the second generation of rural sociologists as the land grant colleges get into the act during the 20's and 30's. Rural sociology, nurtured by these forces and important federal subsidies (Chapter 8), is declared by Nelson to be of age, a field whose scope and content is indicated largely by an analysis of rural sociology textbooks (Chapter 9) and an informative account of professional growth (Chapter 10). After a brief report on rural sociology abroad (Chapter 11), the volume closes on a note sounding "progress, problems, and perspectives" (Chapter 12).

The time sequence of events is a bit jumpy as the author treats first one phase of development, e.g., men in the field, and then in a different chapter turns back to develop another

phase. This is a decision of organization which confronts any writer presenting different types of events over the same period of time. Some effort might have been made to chart the chronology of events so as to give the reader a better overall view.

Anybody who is anybody in rural sociology is mentioned in the works and, of course, some, such as Zimmerman, Galpin, and Kolb, show more than a score of references. The book will be read by "old timers" in the field with nostalgia. It should be read by every scholar of sociology and related fields as an eminent treatment of the birth and growth of an imposing area of academic achievement.

This effort rightly contains only the sociological subject matter necessary to chart the advancement of the discipline as it grew from an initial practical approach for improving rural life to a state of relative scientific sophistication. This development included such problems as what to call this new field, where it should fit into the college structure, what was its scope, and how it related to similar disciplines. Such a contribution is surely more important to rural sociology than another mediocre textbook and will long stand as the bible of its origin and growth.

*The Sociology of Industry*, by S. R. PARKER, R. K. BROWN, J. CHILD, and M. A. SMITH. New York: Frederick A. Praeger, 1967. 182 pp. \$5.00.

CHARLES V. MERCER  
*North Carolina State University*

This small and readable book is intended as an introduction to industrial sociology for readers who have some familiarity with sociological analysis. The authors propose "to synthesize the growing body of relevant empirical materials and to show how sociological theory at different levels of analysis treats the three interrelated aspects of the subject matter."

Part I ("Institutions and Systems") "examines the relationship between industry and other sub-systems of society"; Part II ("Organizations and Roles") "is concerned with the internal structure of industry and the roles which individuals play in that structure"; and Part III ("Aspects of Occupations") deals with the "work and nonwork roles of individuals." Nine of the fifteen chapters were written by Parker and two chapters each by the other three authors.

The book consists mainly of summaries of research findings, mostly from Great Britain and the United States, which should be useful for those whose primary source has been the sociology of work and work organizations in the United States. While there is some interpretation and some synthesizing, the theoretical considerations tend to be more implicit in the organization of materials than explicit in the analysis.

In a brief introduction to any comprehensive body of knowledge one must choose between depth and breadth. Here the authors' emphasis is on breadth: they cover a surprisingly large number of studies from a very wide range of research situations. Those with interests in specific areas within industrial sociology will no doubt be disappointed by the brief and, at points, superficial coverage of most subjects. For example, "The Motivation to Work" and "Work Satisfaction" are covered in one page and one-and-a-half pages, respectively.

The book should be useful to anyone who wishes a brief overview of the subject. People in other disciplines whose interests are related to industrial sociology should find this book helpful in identifying common concerns and in acquiring familiarity with some of the work of industrial sociology. Finally, the book will be useful in advanced general survey courses and as a general introduction to in-depth study of specific areas.

*Policies, Decisions, and Organization*, edited by FREMONT J. LYDEN, GEORGE A. SHIPMAN, and MORTON KROLL. New York: Appleton-Century-Crofts, 1969. 387 pp. Paperbound. \$4.95.

ROBERT A. ROTHMAN  
*University of Delaware*

In the three decades since Barnard first characterized decision-making as the essential process in organizational action, countless students of administration have focused on the dynamics of decision-making in attempts to reconstruct administrative actions and, in turn, develop principles of administration. This perspective generated specialized bodies of literature in sociology and political science as well as in the traditional areas of business administration. It is only more recently that these diverse perspectives are beginning to converge in the study of the recurring sequence of problems confronted at different stages of organizational problem-solving.

The editors of this volume bring together a

collection of previously published papers from these three disciplines which examine the formation and implementation of public policy within such an organizational framework. Readings are organized around a process model comprising four analytically and temporally separate stages in the policy cycle of governmental organizations: establishing a mandate, defining objectives, implementation, and evaluation. Conceptualization of stages emphasizes that any particular administrative act is imbedded in the total policy sequence and that decisions at each point involve different objectives, present different problems, and are limited by different sets of constraints.

Comparative research and case studies are included, but primary emphasis is on papers providing analytic and conceptual understanding of the policy process. The bulk of the readings focus on four aspects of the cycle—participants in the policy process, organization goals, administrative operations, and organizational effectiveness. These sections are complemented by sections which place organizations in the total policy process and which examine decision-making as an analytic process.

In every section the authors have selected a combination of papers which provide broad and comprehensive coverage of the topic. This is true, for example, of the four papers on administrative operations which explore the crucial role of operational problem-solving in the context of the total program. A few individual selections are weak. For example, a study of public opinion is included under participants in decision-making; but it is a descriptive piece which fails to contribute much to the question of the interaction between the public and policy actions. On the whole, however, the papers selected are of good quality and fit nicely into the basic framework.

The book also exhibits a well-executed format which maintains the organizational life-cycle perspective. Introductory comments to the sections place the readings in context, emphasize points, and clarify the criteria for inclusion of each paper. Continuity with the underlying framework is also maintained by very brief introductions to each paper and by retitling articles to make them consistent with the point they illustrate. These simple techniques will probably make it more useful to students.

Considered as a unit, this is a well-integrated and coherent volume which succeeds very well in illustrating both the complexity of the decision-making process and the intricate network of considerations which impinge upon that process. Since it represents a welcome codification of the contributions of a number of disciplines,

it should prove useful in courses in political sociology and complex organizations, as well as in courses in public administration. The broad analytic coverage will permit the instructor maximum flexibility in the substantive areas to be emphasized. It should also find an audience among those concerned with applied decision-making.

*Form and Content in Industrial Democracy: Some Experiences from Norway and Other European Countries*, by F. E. EMERY and EINAR THORSRUD. London: Tavistock Publications (Distributed in the U.S.A. by Barnes and Noble, New York), 1969. 116 pp. \$5.00.

MARTIN MEISSNER

*University of British Columbia, Canada*

Despite its title, this book deals with what the authors do *not* regard as a viable form of industrial democracy, namely representation on the boards of companies. The scene is Norway and four or five of its more or less government-owned enterprises. The authors are applied psychologists—at the time, one of them with the Institute for Industrial Social Research at the technical university at Trondheim, the other at the Tavistock Institute of London. Their major interest was that of well-known previous Tavistock work, the development of autonomous work groups; but this book is not directed to that interest. Instead, it has the appearance of a preliminary report intended to get rid of the wider European concern with representational machinery in order to get on with the main job.

Of the 109 text pages, 22 are devoted to six appendices. One of these contains the gist of an opinion survey done elsewhere in Norway by someone else; three more set out the issues of the work-group autonomy phase of the project; and the remaining two describe briefly the questions and circumstances for thirty interviews with employee and employer representatives.

Chapter 1 consists of verbatim excerpts from ten speeches by government ministers, trade unionists, and businessmen intended to describe the "objectives" of industrial democracy in Norway. In Chapter 2 are excerpts from some interviews with employee representatives on company boards, suggesting what they think they are supposed to do. One of the striking answers is "to create the right conditions for the best possible production." The five pages of Chapter 3 sort out three groups whose interests the representatives might serve, namely employees as a social class, as an occupational group, and in a particular company.

Chapter 4 takes up a third of the text and is a review of information about representational systems in Yugoslavia, Germany, and Britain. The first of these is a summary of the work of Kolaja on the operation of workers' councils in two Yugoslav companies. The second summarizes information from two books about co-determination in West Germany. The third goes over some of the material from the Glacier Metal Company studies previously described by Jaques and Brown.

The remaining twenty pages contain chapters 5 to 7 and the interesting core of this grab bag of a book. We find here a brief description of what happens when a representative first gets on a board and is told that he must be a board member first and not a trade-union man. Implicitly the new representative comes away with the impression that the breadth and level of his education and experience do not give him much weight in board discussions. The authors emphasize that the function of company boards, regardless of the political circumstances, is to maintain and expand the capital of the company and nothing else. They offer some interesting ideas about the relative importance of management versus the board, depending on the extent to which specific uses of company capital are predetermined (through family tradition or government ownership) or can be more freely shifted.

The general conclusion is that boards of companies are the wrong places to seek industrial democracy and that it will have to be sought in the sharing of managerial power through expansion of the areas of negotiation and in greater autonomy of worker groups on the job. Issues in industrial democracy make demands for clarification of the underlying ideology and create fascinating questions for extant theories of organization. This book is not in the neighborhood of an analytical formulation or treatment of either problem.

*Laboratory Simulation of a Police Communications System Under Stress*, by THOMAS E. DRABEK. Columbus, O.: The Ohio State University College of Administrative Science, 1969. 148 pp. Paperbound. \$4.50.

OTOMAR J. BARTOS  
*University of Pittsburgh*

Attempts at simulating complex systems are rare, attempts under laboratory conditions rarer yet; thus Drabek's monograph represents a welcome addition to this literature. Four police officers were observed in a laboratory in most

aspects like a police radio room: telephone calls reporting incidents were processed by the officers as during their working hours, primarily by dispatching police cruisers to the scene of the incident. While the officers knew that the calls were not real, every effort was made to simulate real-life occurrences. Two types of situations were considered: a "normal" Friday evening, and a "stressful" situation stemming from a purported jet airliner crash with 151 passengers aboard.

Perhaps the most valuable aspect of the monograph lies in its description of the painstaking procedures adopted to maintain the realism of the simulation: some of the assistants on the project were trained in police jargon so as to sound like officers manning the police cruisers; others, instructed to make reports about "normal" incidents, were given detailed information about the nature of the incident; the "normal" calls were carefully planned to resemble in their frequency and type the usual Friday evening calls; the "crisis" calls followed a detailed scenario designed to resemble closely a real airliner crash; and so on. To read this discussion is highly useful for those contemplating simulations of complex organizations; others will find here an alternative to the prevailing type of laboratory research with its tendency to remove the richness of the stimuli usually found in real life.

The book is less successful in its treatment of theory and findings. Consider, for example, the finding that the man expected to supervise the four-man radioroom and to "assist if things get busy" (p. 51) (the radio room sergeant) was found to initiate many more communications during the stressful situation than under the normal conditions. Drabek concludes from this that the performance structure of the group has changed (p. 112). This conclusion raises some problems. If one maintains that *any* change in interaction, even that expected and built into the system, constitutes structural change, then this conclusion is true but idiosyncratic and not very helpful. If, on the other hand, one reserves that term for changes violating organizational expectations—as seems more appropriate—then the conclusion is clearly false. Moreover, several hypotheses postulated by the author are of limited interest for the same reason. Is it really so surprising to find that organizational stress leads to an increased task performance, a shift to the most urgent task, and to more frequent decision-making?

In view of the fact that such obviously true hypotheses are quite common in the literature on social organizations, the above criticism of Drabek's work may seem unfair. Yet, precisely



because he succeeded in developing a better-than-usual design, one expects him to accomplish more than others. For example, he has a unique opportunity to adopt a *normative* stance in analyzing his data. How should the training and the communication system of the officers be changed to increase their capacity to handle stressful situations? Pursuing this question would not only be helpful to the Columbus P. D., whose cooperation made the research possible, but might lead to an important theoretical contribution. It is not unusual to find that one has to have a fairly explicit model of the organization before such questions can be answered. In building such a model, one has to consider many relationships that are not intuitively obvious. The stress created by *this* need may indeed change the structure of organizational theory.

*Men in Crisis: A Study of a Mine Disaster*, by  
 REX A. LUCAS. New York: Basic Books,  
 1969. 335 pp. \$10.00.

GEORGE W. BAKER  
*National Science Foundation*

Lucas has intensively reconsidered a segment of the interview data from the same mine disaster (Nova Scotia, 1958) which H. C. Beach and Lucas examined in their 1960 NAS-NRC volume, *Individual and Group Behavior in a Coal Mine Disaster*. His present analytical approach and objectives partly stem from that earlier experience. The 1960 Beach and Lucas report employed the skills and research orientations of psychiatry, psychology, and sociology. The research staff gathered and analyzed post-rescue data from 19 trapped miners, a matched sample of 12 non-trapped miners, wives of the trapped and non-trapped miners, and assorted service and professional personnel in the disaster-struck Nova Scotia community. In his 1969 study Lucas worked from his perspective as a sociologist and attempted to examine in greater detail the trapped miner interview data, collected in 1958 by a psychiatrist, which focused on the miners' experiences from impact to rescue.

Being familiar with the Beach and Lucas study of the Nova Scotia disaster and having earlier found its interdisciplinary approach satisfying, my examination of the current study may be unduly critical. In any case, many readers of Lucas (1969) will want to explore and compare it with Beach and Lucas (1960). Such an effort will be rewarding, and it may clarify some substantive and methodological details.

Lucas' present study was designed to answer such questions as how the miners maintained social control during entrapment, how they perceived various situations during this extremely threatening period, and how they were able to perform such abhorrent acts as drinking their own urine. For these purposes Lucas considered that "the wider context of disaster research and the specific personality attributes of individuals are largely irrelevant to the types of sociological problems considered in this study." Maybe. In any case, Lucas wisely utilized the miners' community norms and the miners' subculture as major factors in his examination and explanation of their entrapment behavior. He also profitably employed concepts from anthropology, psychology, and physiology. Confidence in his findings was strengthened by the availability of two groups of survivors rather than one. In his judgement this provided a measure of replication.

Lucas has had ample opportunity to identify and utilize a much larger body of relevant literature than Beach and he was able to draw on for their 1960 report. He has also developed extensive and sometimes intriguing chapter notes that should not be neglected by the reader with a special interest in disaster behavior. Such scholars, in assessing the rich bibliography, may be disappointed to discover relatively few major works on disaster behavior with a publication date later than 1966. For this Lucas cannot be faulted. Except for the more recent Sealab studies and R. J. Lifton's continuing work, this reviewer is not aware of many new major publications on human behavior in disaster, even though numerous disasters have been investigated during the past few years.

*Sociology of Religion: Selected Readings*, edited  
 by ROLAND ROBERTSON. Baltimore, Md.:  
 Penguin Books, 1969. 473 pp. Paperbound.  
 \$2.25.

MYRON R. UTECH  
*Alma College*

This collection ranges from the classic works of Weber and Durkheim to the contemporary works of Peter Berger and P. Worsley. Methodologically, the studies range from the most abstract and analytical to the most concrete and subjective based on participant observation. In time-perspective they vary from the most general historical context to the most particularistic current issues.

Robertson indicates that the readings are intended to "provide a core beginning to the study in the field of sociology of religion," and uses

the following compilation criteria: the analysis should be up to date and should establish "a pattern of concerns which appear to be central to the field as a sociological discipline." The readings should "indicate analytic issues and provide some empirical grounds for the student" to stimulate his interest in an interrelated set of problems and should attempt to give coverage to a variety of religions and types of societies. There is, of course, the perennial problem of having to choose a general framework from which to study religious phenomena. There is the sociology of religion which sees religion as merely one of the institutions in society, in contrast with the sociological position that religion be interpreted as that set of beliefs which is fundamental to all other beliefs and is basic to the ultimate problems of human existence. Robertson takes the latter position—for which he provides no rationale other than to note that some sociologists are now including more anthropological materials in their studies. He then states, however, that "the sociologist of religion has no over-riding commitment—at least not in his professional publications—to the view that religious commitment of any kind ought to influence economic or political behavior, whereas the person grounding his analysis in a the person grounding his analysis in a religious perspective obviously does (by definition) have some such view." Thus, while he sets forth the objectivist position, at the same time he asserts the contradictory view that religion is central to all belief systems. In addition, he laments the lack of an adequate theoretical scheme but propounds "a general perspective" and "a general style of analysis." It is unclear, then, what Robertson means by *theory* and whether it is synonymous with *general perspective* or *styles of analysis*.

Part One ("General Approaches and Perspectives") is composed of excerpts taken from Weber, Durkheim, Parsons, and an article by Berger and Luckman. Robertson indicates that the Parsons article suggests that Weber and Durkheim were interested in the same general problem, i.e., the meaning of social life. On this basis he uses the Berger and Luckman article to introduce the sociology of knowledge as an area which should be "joined" with the sociology of religion—an example of the editor's broad perspective on the sociology of religion.

Part Two ("Major Patterns of Religion") treats "aspects of primitive religion," "religion in historical perspective," "religion in modern industrial societies," and "mixed patterns of religion in the modern world." Included are excerpts from Evan-Pritchard's study of the Nuer, Troeltsch on medieval Christianity, and

excerpts from B. R. Wilson's *Religion in Secular Society*. One receives the impression that the article by Douglas at the beginning of Part Two on the "primitive" religions, in which he discusses the processes of differentiation and specialization as the core of religious evolutionism, was used by Robertson implicitly if not explicitly to introduce another "perspective" on the sociology of religion.

Part Three ("Problems of Analysis and Interpretation"), including articles by Bellah, Worsley, and Glock and Stark, deals with the ubiquitous problem of the definitions of religion and religious experience.

Part Four, reminiscent of the "perspective" implicitly developed in Part Two, is entitled "Religion and Social and Cultural Differentiation." Here the book deals with a variety of topics such as religion and social change, politics, stratification, religious movements, religious authority, and specialization. Two of the articles, "A Typology of Sects" by B. R. Wilson and "African Religious Movements" by J. W. Fernandez, are detailed discussions and extensions of the church-sect typology and might have been more appropriately included in Part Three.

The articles taken singly are excellent, but the book is poorly organized because the editor seems to be unclear as to the proper scope of sociology of religion. His introductions are unclear and misleading to the extent that the sections of the book seem to have inappropriate titles. The analysis is up to date, but lacks clarity in demonstrating how Robertson's "pattern of concerns" is central to the field as a sociological discipline. The analytic issues seem to be obfuscated and the empirical data sparse, though he does cover a variety of religions and types of societies.

*Innovation in Mass Education*, edited by DAVID STREET. New York: John Wiley and Sons, 1969. 342 pp. \$9.95.

LEO C. RIGSBY  
Vanderbilt University

This is a collection of essays in search of an audience. Most of the articles are macrosociological evaluation studies, though also included are an introductory essay by the editor which sets the problem arena for macrosociological studies of education, an autobiographical article by Martin Mayer relating his experience with the educational bureaucracy of New York City (he was chairman of a local school board there for five years), and a discursive essay by Morris

Janowitz on strategies for systematic changes in the structure and process of education.

Street's essay, though short and aimed primarily at giving coherence to the volume, provides some interesting and useful insights into research issues in the structure and organization of education. He rightfully castigates the bulk of evaluation research, especially that conducted in field settings, for concentrating on only one independent variable—exposure to 'the program,' the no-difference results of many unanswered of whether or not any real change in staff behavior is induced by the existence of the program. He comments: "With the question evaluative studies tell us very little" (p. 8).

Martin Mayer's essay, adapted from an article appearing in a popular magazine, seems strangely out of place in this volume. It is a descriptive account of the rigidity of educational bureaucracy, but it serves to inflame the spirit rather than to raise research questions. Perhaps what makes the article seem out of place here is that it is so readable and interesting.

The empirical papers in this volume are exploratory evaluation studies (if one may coin such an illogical label for them). Because of their exploratory nature and because of a number of methodological problems, they fail to answer (and in some cases, even to inform) policy questions. In fact, the redeeming value in some of the papers is in the new questions they suggest rather than in "answering" those questions which purportedly guided the research. The topics covered in these articles include (1) a summer compensatory education program for inner-city high school students (Roberta Ash); (2) nongraded elementary education (Mary Queeley); (3) a group work intervention program (Rosemary Sarri and Robert Vinter); (4) the impact of goals, curriculum, and recruitment base of three teacher-training institutions on their trainees (Bryan Roberts); (5) the relations between OEO Community Action Programs and school systems (Raphael Nystrand); (6) use of nonprofessionals in city school systems (Timothy Leggett); (7) community conflict generated over the structure and functioning of a school system (Robert Parelius); and (8) the impact of residential mobility and changing school-of-attendance on changes in the IQ scores of pupils (Thomas Smita, C. T. Husbands, and David Street). One problem with several of these studies is that they are condensations of longer reports or, in several cases, of dissertations. Much (hopefully!) was lost in the condensation process.

Two examples of problems in the empirical analyses will be given here as typical of the sorts of methodological problems encountered in these

articles. First, in the study by Sarri and Vinter, much is made of the differences which appear in the distribution of grades of students in different curriculum tracks (controlling for students' IQ level). They conclude that teachers use different standards to evaluate comparable performance (inferring comparable performance on the basis of the IQ control) of students in college prep versus non-college prep curricula. Perhaps teachers do use different standards, but not on the basis of the data presented. (See their Table 10, p. 114.)

A second example where poor methodology undermines the usefulness of results occurs in the article by Roberts. He uses a pseudo-panel (a cross-sectional sample of freshmen compared with a cross-sectional sample of junior-seniors) to assess *change* in professional orientations due to exposure to the education curriculum, incautiously interpreting the results as actual change and with no assurance that such obvious variables as background and previous educational experience were controlled. In fact, data presented for another purpose (see Table 5, p. 136) raises serious question as to whether the two samples from one school are comparable.

In "Institution Building in Urban Education," Janowitz reviews change and innovation in urban education and develops what he calls a "conceptual model" of how change ought to be carried out. His review and model are based on some implicit and explicit assumptions that many policy makers, educational researchers, and educators would not accept (e.g., that intelligence testing and achievement testing are of doubtful validity and are, on the whole, dysfunctional, p. 292). For the sake of completeness it should be mentioned that he does explicitly acknowledge that the school cannot do everything and that a minority of children, unable to succeed in school, will have to rely on other institutions (military, work, and so forth) for essential socialization experience. Janowitz argues that a strategic model of change is needed to guide the restructuring of urban education. The substance of the model proposed is that much flexibility needs to be introduced into urban educational systems so that they can deal in a variety of ways with the problems they face. Unfortunately, Janowitz offers few useful ideas of how to bring about his utopia. The essay does serve the purpose of causing one to examine his own assumptions regarding the social functions of education and the nature of the learning process. It should be mentioned that this essay is a condensation of a short book of the same title published by the Russell Sage Foundation. I checked a few pages in the book version and found much elaboration on, and

qualification of, ideas which were simply asserted in the condensed version. Based on this quick check, I would suggest that readers who wish to consider Janowitz's ideas on institution building would be better served to read the book than the condensation of it in the present volume.

I remarked at the beginning of this review that this was a collection of articles in search of an audience. Policy makers will want to look elsewhere for hard answers to questions regarding the effectiveness of the innovations studied herein. Educational researchers will not find these essays useful as research models. Those engaged in research and development may find a few ideas in this book, but they are ideas which can be found in other places.

*Education and Jobs: The Great Training Robbery* by IVAR BERG, with the assistance of SHERRY GORELICK. New York: Praeger Publishers, 1970. 200 pp. \$7.50.

LEONARD GORDON  
*Arizona State University*

Social scientists (e.g., the Coleman Report) and policy makers (e.g., Congress' passage of the Elementary and Secondary Education Act) tend to emphasize the positive correlation between education and economic advancement. It now becomes increasingly important nonetheless, for critical evaluations of the presumed correlation model are sorely needed. Berg provides such a critique by intensively re-evaluating available data. The almost muck-raking intensity of Berg's attack on the conventional wisdom provides much of the book's high readability, interest, insights, and analytical defects. Berg sometimes brilliantly re-evaluates U. S. census and other occupational and educational data, demonstrating a number of ways the same data can reasonably lead to different conclusions. Yet, as Eli Ginzberg notes in the Foreword, Berg does not confront what is a central issue raised by his challenge to education-jobs data, that of how enlarged efforts in formal educational training are specifically connected to increasing productivity of the economy.

Although no clear alternatives are advanced in either the educational or economic fields, there are clear insights into some current dysfunctional aspects of the educational system vis-à-vis the occupational structure. Berg begins by focussing on the present cultural assumptions regarding the education-employment nexus. His thesis and working hypothesis is that it is invalid to hold that an expansion of

current educational training to more people will necessarily result in more and better-paying jobs for them. This thesis is tested against data on educational training levels correlated with income, job satisfaction, promotional rates, and other job-related data.

Chapter III ("Job Requirements and Educational Achievement") is particularly strong. Here Berg considers five versions of data matching achieved education against education required for available jobs in the occupational structure. Striking discrepancies become evident. For example, it is noted in "Version 1" that, if jobs in "category 5" that call for 1-3 years of college training and if jobs in "category 6" calling for a collegiate degree are separated, then in 1960 there was a 7.2% oversupply of college graduates. Yet, "Version 2" of the same data noted that when categories 5 and 6 are combined so that all the jobs in these two categories call for a college degree, i.e. the prevailing trend, then instead of an oversupply of college graduates it appears as though there is a 10.3% deficit of available college graduates for the jobs available. The current job market for Ph.D.'s and others with advanced training supports Berg's re-evaluation here. For example, the New York Department of Education estimates that in that state in 1970 that are 780,000 men and women looking for unskilled jobs that do not exist while 243,000 jobs are unfilled for lack of college graduates and 60,000 unfilled for lack of high school graduates. The problem is that, if the educational system effectively took up the training slack entirely, there would then be 477,000 educationally advanced people for whom jobs would be available in the present job market.

Berg holds that there is a poor match of educational training and available jobs. His major point is that there are often unrealistic designations of "educational requirements" for many job categories at all levels of blue collar, white collar, and managerial type jobs. Moreover, the schools presently do an inadequate job of training the poor or near-poor, especially in urban ghetto areas irrespective of schooling level reached.

There are some major gaps in Berg's analysis. He concentrates on the existing job structure and not enough on the emergent trends. For example, it is noted in relation to "education achieved" and "education required" designations for available jobs in 1950 and in 1960 that in neither year was there a good match of these two characteristics. The same data show that jobs requiring less than 8 years of schooling fell from 3.9% in 1950 to less than 1% in 1960, a drop of over 400%, whereas

jobs requiring a college degree rose from 1.1% in 1950 to 1.4% in 1960, an increase of over 25%. Given the high employment rates throughout the 1960's while the educational training trend continued upward suggests that to a considerable degree there is a correlation, if not fully understood, between educational training and jobs available.

Another gap is not related to education and jobs *per se*, but to the other functions of education in American society: e.g., since Thomas Jefferson's and Horace Mann's days one of the main tasks of education has been to socialize people into the democratic citizenship role. Certainly the complexity of modern urban-industrial American society makes that a viable economic structure not the only relevant issue in discussing education and jobs. The focus of the book is legitimately limited; but the critical conclusion should perhaps have been stated with greater passion. Although Berg has raised the right questions about the presumed relationship between educational achievement and job status, his goal "to be liberated from a sense of 'task incompleteness'" has been attained. This book is a good beginning.

*The English Public Schools: The Sociology of Elite Education*, by IAN WEINBERG. New York: Atherton Press, 1967. 225 pp. \$2.95.

*The Cloistered Elite: A Sociological Analysis of the English Public Boarding School*, by JOHN WAKEFORD. London: Macmillan & Co., 1969. 269 pp. 45s.

LLOYD BARENBLATT  
New York University

The grip of the English elite school on British life and social structure persists and even shows signs of intensifying in some ways, despite the obvious efforts to democratize education and opportunity in Great Britain. The "public" schools define themselves informally but definitely by their tacit mutual recognition, exchanges of social accords, reciprocations of power and favor, and more explicitly and formally, by membership in the Headmaster's Conference. They are recognized as a legal entity by statute, official mention, and concern of government commissions. Numbering only eighty-four, the public schools go back historically to late feudal times; but their present nature and place in the social system is a Victorian product. A notion of the functional importance of the conference schools can be seen by such facts as their contribution of 60% of the cadets at the Royal Military Academy and their dominating influence in the staffing of

the foreign service and the home civil service. Despite the periods of tenure of the Labour Party and the growth of the state-supported free grammar schools, they continue to exert their influence on the social and governmental structures of the country. The very model of being "very British" is set by these schools, and that term is used colloquially to describe the personal mannerisms and character associated with the few "old boys" who are the graduates of the public schools. Gaitskell was a product and advocate of them, and Wilson sent his son to one of them.

Of course, there is a gossip interest in the goings-on of the elite as well as a more valid sociological interest in the public schools as organizations and as an institution. Of more importance is the complex nature of the functional relationships of this institution to the working system of British society. Although these books are invaluable sources, the more precise mechanics of the interdependence of elite class influence and the differential British educational structure remains mostly opaque; Weinberg's book is particularly suggestive regarding the functional requirements of British society and government which the public schools fulfill.

More puzzling is the position and social consequences of elite private education in the United States. The model in America is largely that of the English public school, and the proportion of students involved is also quite small. While the English public school is highly visible and its relation to the national life is acknowledged without question, the American elite private school is only dimly visible, even to the intellectual segment, and unknown or dismissed by the larger public. In the United States the elite school tends to be stereotyped in terms of its snob functions; and hard, clear quality of its education is neither openly acknowledged nor bruted about by the schools themselves. The relationship between the American elite private school and national leadership is obscure to the extent that its possible sociological importance remains unclear even as a question. Herein lies one of the important reasons why American social scientists would do well to scrutinize the institution of the English public school. This area of inquiry offers an entry point to the complex and poorly illuminated system of class and power controls in the United States.

Wakeford's book is especially strong on the content of the English public schools' internal social system. The social and psychological input, process, and output in terms of the boys themselves is even more interesting if the reader

allows himself to speculate on their relevance to broader sociological antecedents and consequences. For instance, what are the societal implications of the "total institution" qualities of the public schools? There is a certain cuteness in finding a general similarity between the schools for the elite and reformatories; but the equation does have important meaning in suggesting that deliberate and careful socialization processes, shielded from outside contamination and the prevention of premature impact of the inmates on the outer world, are functional in both Chillicothe and Cheltenham. Weinberg's book complements Wakeford's nicely in that there is more material connecting the elite school to the larger social order, especially in the chapters on occupational positions desired and filled by the boys and on the boys' parents.

*Sociology in Britain: A Survey of Research*, by ERNEST KRAUSZ. New York: Columbia University Press, 1969. 222 pp. \$7.50.

EARL HOPPER

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The book's explicit intentions are to describe the scope of contemporary British sociology and to review some of its main trends; its implicit aims are to outline the social structure of modern Britain and to introduce students to the discipline of sociology. All four are worthy tasks. But here they are undertaken simultaneously. This is presumably why the book lacks focus, and also explains its general ambiguity and superficiality.

In connection with the two implicit aims, the book does not warrant a serious critique. It is inevitable that a discussion of "sociology" should involve comment on the structure of that society and on the nature of sociology itself. In this case, however, the discussion is so limited as to mislead and to confuse. It could be condensed without loss into an introductory chapter.

In connection with its explicit aims, the book provides detailed information about some aspects of sociology in Britain today. Its summaries of much-quoted and well-known studies will help undergraduates. Brief consideration is given to the contributions which sociologists in Britain have made in the problem areas of theory and methods, as well as in the philosophy of the social sciences. The author considers more fully such topics as community studies, education, deviance, social stratification, and mobility.

Perhaps the author's main contribution is to stimulate thought about the development of

sociology in Britain. I was reminded that the emergence of modern social science in an industrialized yet still sacred society is an enormously complex and fascinating source of sociological problems. This is one reason why I was disappointed that so little attention was given to the sociology of sociology in Britain. A number of general but critical questions have to be asked. For example, how is the age structure of sociologists related to their changing interests and styles? Further, what has happened to the philosophical-quasi-theoretical school and to the empiricists? Is an overriding concern with social problems still the main source of intellectual energy? What about the lack of funds for empirical research and its effects on procedures for training graduate students? And what of the inhibiting effects of Oxford and Cambridge whose suspicions concerning the status of the discipline have not only retarded the development of sociology there but also throughout the country and perhaps in Western Europe? None of these points is covered. So, in effect, after reading the book, one knows about certain studies of social phenomena in England and Wales but hardly anything about sociology in Britain.

Anyone who is really in touch with the British academic scene knows that most of the sociological enterprise is still in draft. It takes the form of unpublished papers and discussions at seminars, meetings, and conferences. Although this arrangement encourages young sociologists as well as those who are scattered around the country in dozens of small institutions, it also retards the publication of more permanent written contributions. This is especially so in the case of empirical research. One reason for this is a widespread sense of inferiority compared to American sociologists in knowledge of research methods and techniques. Another is the oppressive cynicism and hyper-criticism by some very senior sociologists who, for the most part, were not trained in sociology and have little experience with empirical research. Their guiding perfectionism is often disabling. And a third reason is the discovery of phenomenology and symbolic interactionism, which, except in the hands of well trained professionals, lead only to the kind of discursive papers suitable for conferences. In brief, despite (and perhaps because of) the boom in the number of departments, chairs, students, and staff, a contemporary sociology is still incipient.

In 1950 the *British Journal of Sociology* was born. Although the initial issues contained many more articles by foreigners than by natives, this was a symbol that modern sociology in England was possible. In 1967 *Sociology*, the

official journal of the British Sociological Association, was first published. It was started mainly by a group of young research-oriented sociologists who were trained as sociologists. A cursory inspection suggests that it reflects the best empirical research in Britain today, and that its theoretical articles are always pertinent to further research. *Sociology in Britain* is a contribution in this respect, but a small one.

*Clients Come Last: Volunteers and Welfare Organizations*, by ESTHER STANTON. Beverly Hills, Calif.: Sage Publications, 1970. 192 pp. \$7.50.

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The distinguishing feature of this book is the forthrightness with which its author dissects the public relations techniques that organizations use in developing community support. Though Stanton centers her participant-observer analysis on the Pangloss (note the suggestiveness of the name) Mental Health Association, a United Fund agency with which she was connected over a period of seven years, she relates her observations to private philanthropic organizations. Moreover, Etzioni, in his enthusiastic foreword to the book, links them to the "all-encompassing phenomenon of our times," i.e., the fashioning of facades that make society seem more appealing, humane, and participatory than it is (pp. 9, 10).

The multiplicity of devices employed in the image-making process are presented in a dramaturgical framework adopted from Goffman's *The Presentation of Self in Everyday Life*. This process begins as a temporary measure on the part of the agency to bridge the gap between its altruistic goals of social change and its practical need to mobilize the support of the community on which it is dependent. In the case of the Pangloss Mental Health Association, the agency created and sustained a glamorized image of itself through prime coverage in the mass media and recruitment ("seduction") of socially prominent ("name brand") board members (p. 115), as well as through such day-to-day performance routines of staff members as assuming a dedicated look (p. 100), giving each volunteer the "VIP treatment" (p. 112), engineering spontaneity by "planting" the audience with pledgers (p. 120), and bolstering the performance of a presiding officer with an "idiot sheet" complete to the detail of spelling the volunteers' names phonetically (p. 62).

The outcome, according to Stanton, is that so much effort goes into image manipulation that "honest, decent, and idealistic people—both paid staff and volunteers—submit their own humanitarianism in activities which are mutually manipulative" (p. 13). Clients and their needs go largely untended. The whole Association program becomes hopelessly involved in staging a drama of effectiveness that will conceal its ineffectiveness for creating a society in which the mentally ill receive understanding and expert professional treatment (p. 132).

Though the colorful, semi-serious, impressionistic language in which this book is written does not meet the rigorous demands of precision and objectivity currently expected in sociological writing, it does produce an exciting emotional impact. The reader winds up persuaded that the bold statements Stanton has made, like the overstatements of which Rachel Carson was accused, will turn out to be truer than many of us like to think. But perplexing questions remain: Is an elaborate system of image building essential to the survival of contemporary institutions? If it is, can the image building be managed in such a way that it does not subvert the very objectives that the institutions exist to serve? In other words, is there any way that clients can come first instead of last?

*Hospitals and Patients*, by WILLIAM R. ROSENGREN and MARK LEFTON. New York: Atherton Press, 1969. 225 pp. \$8.95.

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The authors of this book have done a remarkably succinct job with the demanding tasks of considering how to maximize health care resources and how to utilize the sociological perspective in the interests of both practical and theoretical problems. The former, though hardly the exclusive province of any social science and despite a rapidly burgeoning literature, has yet to be sharply addressed by sociologists. The reasons for this failure to come directly to grips with such a critical practical problem, the authors imply, perhaps lie in the choice of inappropriate conceptual models or—worse yet—no model at all.

Thus, after first organizing the sociological literature dealing with hospital structure and operation into the several prevailing models for organizational analysis (i.e., the classic bureaucratic model, a social system approach,

symbolic interaction and related social psychological schemes, and a community network orientation), the authors develop their own analytical model. This is the substance and contribution of the book.

The central feature of their scheme is its concentration specifically on the *patient* as a person. This contrasts with more conventional, however respected, approaches which interpret organizations in terms of their administrative authority, power, control mechanisms, division of labor, goal conflict, or system maintenance. Client-centered concern, nevertheless, has been covertly pervasive, stemming from a wide range of sources: on the one hand, the "long-standing preoccupation in the social sciences with the points of contact between the individual and social organization," on the other, to Redfield's "claim that a notable drift in modernization is increased dominance of the moral over the technological order."

Postulating that "people processing" organizations interrupt the course of their clients' lives, such organizations must be structured so as to effect the chosen interruption. "Choice of interruption," of course, need not be explicit. Thus the authors isolate, as their independent variables, two axes for organizational intervention in the "client's biography": a *longitudinal*, or social time dimension, and a *lateral*, or social space variable. These concepts respectively describe the hospital's concern with patients chiefly as (1) over a long period of time, though affecting primarily the patient, and (2) encompassing the patient and his network of social relations, though over a short period of time.

Since hospitals allegedly take positions towards their clients on both longitudinal and lateral dimensions (i.e., they vary independently of each other), four logical types of interaction occur. An acute general hospital, for example, would have low biographical concern both longitudinally and laterally; a long term psychiatric hospital, on the other hand, would have high client interest on both longitudinal and lateral dimensions. To complete the permutations, a short term psychiatric hospital may be "high lateral—low longitudinal" in its orientation, while a medical school hospital may be "low lateral—high longitudinal" (p. 125). These dichotomous types, coupled with the elaborations following, are presented in a Merton-inspired paradigm of pluses and minuses.

Methodologically the model is elaborated into a 4-celled table with one type of longitudinal or lateral representation in each cell, accompanied by a type of some other variable. For example, hospitals with low orienta-

tions on both the longitudinal and lateral dimensions are conceptualized as having neither "conformity" nor "commitment" problems on a "compliance" variable.

In addition to the compliancy variable, other problem areas (or "dependent variables") discussed, with their dichotomizations, are *consensus* (by means and ends), *authority* (administrative and operative), *staffing* (professional and administrative), *innovations* (technical and ideological), *work rhythms* (by two types each of decision making and work tempo), *social types of clients* (by socialization to values and behavior, and Rosow's four socialization types), *hospital subcultures* (by two types each of conflict and subculture pattern), *size of reference set* (by two types each of operative and administrative set) and *modes of collaboration* (by two types each of formal and informal collaboration). Each variable is briefly defined and its potential relationship to the longitudinal-lateral dimensions pointed out. Possible empirical illustrations are provided. The concise presentation of such an elaborate model is made, however, at the expense of additionally needed detail and illustration. This fact, plus the obvious need for "patient-centered organizational studies," requires that the model remain speculative, highly suggestive, and—hopefully—heuristic. Any attempt to assess its validity at this point would be totally premature.

One further theme runs throughout the text. In addition to regarding the hospital as an abstract organizational entity and then as an organization implicitly catering to patients' needs, it finally is considered in the context of its "community relations" with other health-care delivery systems and organizations. At several points the authors discuss current governmentally sponsored plans to "regionalize" the provision of health care. Their discussion of community aspects of client-centered care provides an interesting and perhaps prophetic finale to this ambitious and useful book.

*Men, Money, and Medicine*, by ELI GINZBERG with MIRIAM OSTOW. New York: Columbia University Press, 1969. 291 pp. \$8.50.

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The main task of this study is to reconcile the improvement of the quantity and quality of health services available with the realities of the economic system as a whole. The focus of the analysis is the "political economy of health" which also means "competition in the market



place" by medicine for relatively scarce dollars and manpower. This emphasis upon so-called economic realities produces some pungent and rather blunt comments on such subjects as increasing the number of physicians, making clinical test laboratories more efficient, and improving the over-all environment for human life.

Curiously, in spite of an apparent assumption of intrinsic good in the concept of medical input, including its technological apparatus, there are unexpected pronouncements which seem almost contradictory to that belief. For instance, it is stated that "people who are ill, even seriously ill, will generally get well without the active intervention of a physician." The author then plays down the importance of medical care to health, happiness, and longevity.

This ambivalence about medicine as a social and economic structure and about its value to society leads to an early conclusion that even a factual (rather than a fanciful) examination of medical economies and human resources will produce no decisive program of reform. We shall "be forced to muddle ahead" because the American public is not nearly ready to abolish the existing system of medical care. Thus, by the end of Chapter 2, we are given the verdict on reconciliation of health service improvement and economic reality: it is not likely to be accomplished.

The bulk of the remaining chapters deal selectively with the growing pressure by non-professional hospital workers to organize and bargain collectively in the "health services industry," with government health plans and their costs, and with various professional and occupational problems in the medical hierarchy—physicians, nurses, clinical laboratory personnel, social workers. The final section, called "Sickness and Society," is a loose collection of chapters on such topics as the mentally handicapped, mental health in industry, and tuberculosis control.

This brief outline may suggest that the book is unsuccessful and indecisive. However, the author's attempt to be "realistic," pragmatic, and debunking is not a failure. The style is readable and devoid of jargon. The data are neither tedious nor irrelevant; and, despite the lack of customary tables and graphs, there is a distinct flavor of expertise and of a solid data base underlying the comments made.

The analyses of the various parts of the medical system are more decisive than the general conclusions. This willingness to make specific judgments is welcome in a setting where tentative analysis and equivocation have often prevailed. For example, concerning Medicaid, it

is observed that the increased infusion of federal funds has simply solidified the maldistribution and inequities of the present medical system. Regarding the physician shortage, the evaluation is that we are not making efficient use of the physicians we already have and that less expensive personnel should be doing some of the traditional "doctor's jobs." The tightness of the manpower market is viewed positively as an incentive to improve efficiency. Since medical care cannot be run at a profit, the society will have to bear both increased consumer costs and much of the expense of training and paying the workers in the medical system. The whole field of clinical laboratory work is viewed as a fertile area for automated procedures run by a few highly trained computer specialists. This would be preferable to the army of technicians now employed.

After this series of concise and clear analyses of several segments of the system, we are again treated to an overview of things to come. It is felt that the present system, as it exists, could yet absorb much higher cost without breaking down. The physician supply will not worsen, and the "odds" favor a closer match in the future of supply and demand for all levels of personnel.

Finally it is contended that the people still *Health* know less about illness than they should for their health protection. The basic lesson of the *bast* is that improving the environment is the key to better health. Yet the "realistic" view is again injected via the contention that present deficiencies probably won't be eradicated. Nevertheless, we should keep trying to improve the system. Unfortunately, we are given no clear direction for improvement; general comments on "better exploitation" of technology and "more efficient" use of personnel are small recompense for all of the "realistic" fact-finding. Despite my sense of futility on these general levels of analysis, the particular evaluations of various parts of the medical system are well made and reflect a welcome economic style in an economist author. On balance, the book is one of the best presentations of where we are in the socio-economic area of health care that I have read. Where we should go is still the task to be tackled.

*People in Pain*, by MARK ZBOROWSKI. San Francisco, Calif.: Jossey-Bass, 1969. 274 pp. \$8.50.

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Many medical sociologists and knowledgeable hospital administrators have long maintained

that a clear knowledge of the patient's cultural background can be instrumental in affecting treatment cure, particularly of those suffering from psychosomatic disorders. *People in Pain* is thus timely and significant, addressing itself to ways in which people of contrasting cultural backgrounds respond to pain. A number of important questions are raised: Why do some cry when in pain while others maintain a complete silence? Why are some patients eager to describe their symptoms freely while others minimize their suffering? What role does variation in culture play in the expression or inhibition of pain? Zborowski, an anthropologist, contends that responses to pain are learned as part of one's cultural heritage.

The study examines the attitude and response to pain of four groups: Irish, Italian, Jewish, and "Old" American (at least "third generation" and maintaining no ties with the old world). Data were collected in a large metropolitan VA hospital through direct observation; intensive taped interviews; and information provided by physicians, nurses, or members of the family. Two groups of patients were utilized in the research: one group suffering from an identical illness and the other group chosen from patients with different diagnoses ( $N=142$ ). No sampling technique was used either in the selection of the hospital or of its population. "The only criteria for selection of patients for the interviews were ethnic origin and the presence of pain. . . . Accordingly, there was no proof whatsoever regarding their statistical representativeness" (pp. 12-13).

The findings confirmed the clinical impressions of medical practitioners that patients of Jewish and Italian origin are more emotional while experiencing and expecting pain than are Old American patients. Jewish and Italian patients tended to play up pain, whereas the Old American and Irish tended to play it down. Patients' descriptions revealed that such characteristics of pain as its intensity, location, and duration were expressed with less precision among the population. "The only criteria for selection of Irish and Italian than among the Old American and Jewish.

Old American and Irish patients tended to express more confidence in the doctor's skill than did Jewish and Italian patients. Jewish patients were more likely to check up on doctors and "shop around," suggesting to the researcher a lesser degree of confidence in the physicians. The Irish seemed to stand out as most cooperative. In contrast, Jewish and Italian patients seemed to be more dissatisfied and more critical of the hospital care. Throughout the study, Irish patients expressed worry

and pessimism about the outcome of their illness and its effect upon their body and masculinity. The Old American patient lives up to the image of being a desirable patient in that he is not as demanding on staff.

Unfortunately, this study is methodologically weak. Zborowski could have obtained much better indices of the effects of cultural backgrounds of patients and their response to pain. He might better have presented many of his findings in tabular form and thus avoided the redundancy of repeated examples and case histories. Despite its inadequacies, the book has a great deal to offer anyone who is interested in the socio-psychomedical care of patients.

*Social Status and Psychological Disorder: A Causal Inquiry*, by BRUCE P. DOHRENWEND and BARBARA SNELL DOHRENWEND. New York: Wiley-Interscience, 1969. 207 pp. \$9.50.

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While many persons interested in the sociology of mental illness have been off pretending to be very much wiser by conjuring subtleties from a labeling approach, the Dohrenwends have been making a vigorous, frontal attack on that worst of all mental-disorder monsters, the problem of the relative etiological power of environmental and genetic determinants. When we welcomed the Scheff-Lemert strategy of putting aside primary deviance to focus on the secondary processes of stabilization and amplification, it was clear we would sooner or later have to get back to the larger causal framework. If labeling has provided a moratorium from the research frustrations that had accumulated in the quarter century since the Chicago ecology studies, they are all still there; and now they have to be dealt with. In this important book the Dohrenwends point in the direction of completeness again, and one has the feeling that these difficulties are at least better under control.

This second monograph in the Wiley series on psychological disorders is a kind of *tour de force* that extends, as well as incorporates and revises, earlier reports by the authors and their associates. Its major contribution is a new implementation of a natural "crucial" experiment intended to support either the nature or nurture explanation of mental illness. The design requires an open-class society in which differentially favored ethnic groups have become assimilated. With certain assumptions, social causation is supported if *within-a-class* disadvantaged ethnic groups exhibit higher prevalence rates than those

which are advantaged; while, if the disadvantaged rates are lower, support is obtained for a social *selection* (presumably genetic) explanation. The social stress reasons for the former outcome are familiar enough, but the rationale for the latter is less obvious. It suggests that, since capable (i.e., mentally healthy) members of the disadvantaged groups are less likely to rise from a given class position, their continuing presence dilutes the class-specific prevalence rate for psychopathology; and by the same token, since the capable advantaged are more likely to move upwards, their subsequent absence serves to inflate the corresponding rate for these groups.

This quasi-experiment is conducted primarily with data from a study of the Washington Heights area of New York City, with instruments partly derived from the Midtown Study, in which Blacks and Puerto Ricans were cast as the disadvantaged ethnics and Irish, Jews, and "Others" (Italians and Greeks?) as the advantaged. Unfortunately the results are equivocal since the Puerto Rican data appear to support social causation and the Black data (maybe) the selection hypothesis! This difference is not so much reconciled as it is dismissed in a ruthless methodological critique whose conclusion is that neither these data nor anyone else's to date can be taken at face value. Nevertheless, the Dohrenwends intend to keep the experimental design and to overcome current methodological problems through a sophisticated weaving—begun here—of a nomological net that incorporates the construct validation of psychological health and illness.

The design appears sound, and its ultimate value will be seen in the comparative studies it encourages. It would probably be wiser not to insist that it is "crucial." With respect to the substantive section (Chapter 6), it is not clear from the methodological critique why the study's major faults and problems—e.g., response sets, yea-and-nay saying, interviewer effects—were not anticipated before the research was carried out. Furthermore, one feels somewhat played with by analyses that are open to these criticisms when one of the authors has already published a similar critique of related data in 1966. Regarding the experimental test itself, it should be noted that there is no explicit check on an important aspect of the manipulation, namely, the fact of differential mobility in the populations studied (thus a necessary condition for the genetic proposition may not have obtained). Finally, a better test of the alternative hypotheses might have been accomplished had rates for the comparison groups been standardized.

Besides these substantive matters, the remaining chapters provide excellent reviews of studies of true prevalence, stress, and the stabilization of situation-specific stress reactions. Each chapter has its own summary, and the last chapter offers a tight summary of the book. It might usefully be read first.

Although it may upset some to discover that most of what labeling theory has said is included here as only a partial cluster of events tied to the notion of secondary gain and loss, Chapters 8 and 10 (concerning such gain/loss and stability) are really needed contributions to labeling theory. And for those who are upset, it may be instructive to note that in presenting Shirley Star's well-known descriptions of psychiatric cases," the Dohrenwends appropriately leave them unlabeled for the reader.

*Migrants in Europe: Problems of Acceptance and Adjustment*, by ARNOLD M. ROSE.  
Minneapolis, Minn.: The University of Minnesota Press, 1969. 194 pp. \$7.50.

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Although the late Arnold Rose's research on intra-European migrants (amounting to some eight million persons during the period 1955–1966) was planned and developed as a part of an ambitious interdisciplinary project to study the integration of Europe, this book is valuable as an independent sociological analysis of recent intra-European migrations.

In the first three chapters Rose discusses the meaning of integration and the causes, patterns, and probable future trends of European migrations, and presents his theory of acceptance of migrants. In the next two chapters, he examines the independent variables—the official policies of the host countries toward the migrants and various "nonpolicy" factors affecting acceptance and adjustment of migrants, notably the economic factors, the attitudes and ideologies of the nationals of the host countries, and the characteristics of the migrants and their countries of origin. In Chapter 6 he discusses the dependent variables—the extent and nature of acceptance and adjustment, psychological problems and social pathologies (e.g., relatively low crime, sickness, accident rates), and the return of temporary migrants to their countries of origin. Finally, the findings are summarized and briefly discussed with regard to their possible relevance for the post-World War II integration of Europe.

Belgium, France, Germany (FR), Luxembourg, Netherlands, Sweden, Switzerland, and

the United Kingdom were the principal countries of immigration; Greece, Ireland, Italy, Portugal, Spain, Turkey and Yugoslavia were the main countries of emigration. Although several countries had immigrants as well as emigrants, and frequent complaints about a significant "brain drain" were recorded, a large majority of the cross-national migrants came from the less developed rural areas and found employment in industry, mining, service occupations, and other jobs which require relatively little training.

With considerable ingenuity and some arbitrariness, Rose ranked countries on several variables. With regard to his first independent variable, the officials openness of the host country, he concludes that while "certainly no country is really very bad for immigrants . . . and there are also gaps and faults in every country," France and Sweden have made the most significant formal efforts to "accept" the migrants; Switzerland, on the other extreme, "has chosen the conscious path of rejection." One must, of course, remember, that Switzerland has an exceptionally high proportion of foreign nationals who constitute 32% of its labor force, 45% of physicians, 26% of mechanical engineers, 19% of university professors, etc.

As could be expected, no country was consistently rated as the best or the worst on all variables. The Netherlands, for instance, ranked very low on official openness and relatively high on informal attitudes and behaviors toward the migrants. Germany discouraged assimilation and viewed migrants only as "guest workers," but the German trade unions welcomed and protected foreigners and the German recreational programs for migrants were best organized. France was most anxious to assimilate migrants on a permanent basis, but had less to offer economically than Sweden, Germany, the United Kingdom, and Luxembourg. The United Kingdom developed excellent local organizations to fight prejudice against migrants, but also had enough prejudice to keep these organizations busy.

Undoubtedly, Rose's greatest contribution in this study is the testing of the main hypotheses "that acceptance, integration, and adjustment of foreigners into a host society is a function of the following: (1) the openness of the host society; (2) the degree of attachment the immigrants feel for their society of origin; and (3) the similarity of cultures of the country of emigration and the country of immigration." A positive correlation between the openness of the host country and the adjustment of immigrants was found for the eight countries of immigration ( $\rho=.47$ ). When,

however, the Netherlands was left outside the correlation (having ranked very low on official policies and relatively high on informal attitudes and behaviors which the total openness score failed adequately to reflect),  $\rho$  jumped to .73. Concerning other relationships, Rose states that:

The facts give no reason to believe that the degree of attachment of the immigrants to their society of origin inhibits good integration or adjustment to the immigrant country ( $\rho=0$ ). Nor do the facts give reason to believe that similarity of the cultures of the country of immigration and the country of emigration encourages better integration and adjustment of the immigrants ( $\rho=.10$ ). . . . If the true relationship should be nothing, as the crude indexes of this study suggest they are, a most valuable finding emerges, and it is one that goes against current sociological thinking: the cultural and political backgrounds of migrants—by country, not as individuals—appear to have little or nothing to do with the kind of adjustment they make to the immigrant society. . . . What is important for integration and adjustment is the openness of the *immigrant* country.

In spite of the fact that Rose could do relatively little with his data to interpret the present status of integration of Europe, and not withstanding the ambiguity of some of his concepts and the crudeness of his indexes, *Migrants in Europe* is undoubtedly one of the best and most stimulating books in the area of human migrations and is likely to exert a profound influence on future research and thinking.

*Social Structures and Systems: A Sociological Overview*, by WILLIAM M. DOBRINER. Pacific Palisades, Calif.: Goodyear Publishing Co., 1969. 273 pp. Paperbound. \$3.95.

*The Sociological Experience: A Modern Introduction to Sociology*, by SCOTT G. McNALL. Boston, Mass.: Little, Brown and Co., 1969. 218 pp. Paperbound. \$3.50.

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These introductory texts are relatively brief presentations of traditional perspectives and concepts. Dobriner utilizes a "functional perspective" as a "theoretic bias," the topics and style of his book are somewhat similar to Kingsley Davis' text, with additional segments explaining the pattern variables and the four functional "imperatives" from Talcott Parsons. McNall attempts to develop "a framework for studying and analyzing social behavior" (p. ix). His book is organized in reference to general topics, including a study of the discipline and its methods and rhetoric, basic patterns of so-

cialization and interaction, association in large groups, the breakdown of associations, and some selected social institutions.

While these texts are similar in the absence of large doses of anecdotal, humorous, and illustrative material and in the coverage of standard topics, there are substantial differences between the two books. McNall has integrated an impressive and thoroughly documented review of the literature; Dobriner has "not attempted to footnote heavily or document" (p. viii). Moreover, McNall uses a very impressive range of sociologically relevant material from non-sociological sources. McNall places primary emphasis on theory as "a formal deductive system." In contrast, Dobriner considers the "logic of sociological inquiry," taking a causal, probabilistic approach.

It is especially noteworthy that Dobriner optimistically expects that "the sociologist will happily settle for 99.99 per cent probability" (p. 41). Such statements may be misleading when currently path coefficient analyses suggest that 50 to nearly 100% of the variation in behavior is *not* explained. It is difficult, moreover, to see a clear connection between his causal analytic concern and his functionalism.

Both books tend to make the introduction to the theoretical form of sociology needlessly abstract and irrelevant. The initial introduction to theory does not make clear a very simple operation—examining the relation of two concepts to each other in a theoretical form along with specific data. As a result, tables that are presented will be ignored by introductory students since they will not be able to move from theory to data and back again. In both books it also appears that the initial consideration of theory and methodology is not really integrated into subsequent material. Regardless of their differences and defects, both books are thorough, substantial developments of concepts in sociology, with McNall's being the more precisely constructed and the more interesting to read. It is likely that they will primarily interest introductory students who have a real desire to learn about sociology and instructors determined that students will learn about sociology and unaffected by student evaluations.

*Sociology: An Introduction to Concepts, Methods and Data*, by MARK ABRAHAMSON. New York: Van Nostrand Reinhold Co., 1969. 340 pp. \$7.50.

*Introductory Readings on Sociological Concepts, Methods, and Data*, edited by MARK ABRAHAMSON. New York: Van Nostrand

Reinhold Co., 1969. 466 pp. Paperbound \$4.95.

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Abrahamson claims that he is trying to present the modern, rather than the traditional, sociology. He is, in part, reacting to the "unjustifiably low esteem" in which students and professors of other disciplines regard sociology and to the "enormous discrepancy" between the sophisticated research of sociologists versus the unsophisticated presentations of sociology in introductory texts. This book is intended to show that an introductory text can be written which requires no apology, depicting a developing science where concepts are used whose "usefulness can be empirically demonstrated" and presenting recent research.

To say that the book is pretentious does not convey much useful information but does summarize my reaction. The author claims to be presenting modern rather than traditional sociology. If *modern* means an emphasis on empirical research and concepts, the claim is reasonable enough. But there is no qualitative difference between this book and many other introductory texts in sociology, although about 15% of the book is spent on shop talk and methodological concerns of the discipline. Little attention is given to developmental aspects of sociology or key personnel in its historical development. Instead, a much greater concern is devoted to explaining not only what sociologists know but how they know it, a concern which, in addition to a discussion of ways in which data are obtained, also contains a rudimentary treatment of statistics. Evidently it is still a matter of debate and choice as to how much methodology can be presented and understood in an introductory course.

There is a second basis on which the "modernity" of the book can be evaluated: the types of subject matter discussed, especially in reference to contemporary social issues. The book is decidedly weak in this regard. Minimum discussion and few examples are drawn from the dynamic concerns of race, power, peace, protest, collective behavior, population or ecology. Even in sections of the book where one would expect cognizance of these contemporary realities, little awareness is shown (e.g., the chapters on "Industrial-Urban Social Organization," "Social Stratification," and "Deviance and Control.") In this sense the book is quite traditional instead of modern. The companion book of readings can have a similar indictment leveled against it. It would seem that, if the text is functionally and traditionally oriented,

it would do no harm to have a book of readings geared to social and psychological issues. Many of the examples in the text are drawn from primitive social organization and 19th century sociology, examples more adapted to functional analysis and anthropological concerns than to contemporary sociology. Yet, surprisingly there is little detailed discussion of specific institutions, such as the religious, political and economic; failure to provide a full explication of the nature of culture, i.e., its evolution, variability, form, and content; and no discussion of the basic concepts in demography, human ecology, and community.

A very strong point in the book's favor is the readable manner in which it is written. Selected concepts and ideas are presented in terms of their use in research and their bearing on methodological and theoretical issues in sociology. The author is also to be commended for having produced one of the shortest introductory texts on the market.

*Social Structure and Social Process: An Introductory Reader*, edited by PETER ORLEANS, in collaboration with SONYA ORLEANS. Boston, Mass.: Allyn and Bacon, 1969. 729 pp. Paperbound. \$5.95.

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This reader is made up of 44 articles (covering 729 pages) from the journals and a few books; there is little abridgement. The underlying premise of the reader is that Nisbet's essay on "Sociology as an Art Form" is "correct in stressing the idea that, at root, sociology is a humanistic endeavor." This essay and Schnore's paper on the Myth of Human Ecology form Part I and set the tone for assessing the orientation underlying the remaining pieces.

The final section—Part IV—consists of three papers which relate nicely to the above two. They are Gouldner's Anti-Minotaur paper, Greer's searching questions on traffic and transportation (for whom is it a problem?), and Vestermark's exposure of the ideology underlying Waskow's report of the 1962 Social Science Conference on the Shelter-Centered Society.

Part II ("Theoretical Constructs and Research Strategies") includes an excellent selection of papers by Campbell on experiments, Becker and Geer on participant observation (with Trow's discussion), Merei on groups, Bierstedt on majorities, Rabb and Lipset on prejudice, and Turner on sponsored and contest mobility and on personality. The Grusky-Ganson dialogue on managerial succession in baseball is Books' thesis pointedly supports Nisbet and Gouldner on values, and a ten-page selection from Mead is enough to give the student an awareness of why Mead is so esteemed.

Part III ("Aspects of Social Structure and Social Process") is organized in reference to four focal concerns: (1) dimensions of social order—including a good British critique by Goldthorpe of the prevailing view on the similarities of social stratification in industrial societies; (2) maintenance of the social order, with a section on socialization and one on social control; (3) tensions in the social order—marked by presentation and testing of the mass society argument by Gusfield, Trow, Gamson, and Dahrendorf; and (4) changes in the social order: social transformations—possibly the section which will elicit the least enthusiasm from users of the volume.

The Orleans reader is one of the few intellectually respectable readers available for introductory courses. Moreover, the editor limits his remarks to a page or two when introducing a chapter. Taken together, these virtues may be the book's undoing: many instructors may think it simply too demanding.

#### COMMENT ON BACK'S REVIEW

(In response to Curt Back's Review Essay on Behavioral Sociology: The Experimental Analysis of Social Processes, in the December 1970 issue of the ASR, pp. 1098-1100.)

The purpose of this communication is not to defend the orientation represented in our book—we have already attempted that in the book itself and we invite readers to examine our position and reach their own conclusions. What we would like to do, however, is to point out certain errors in Professor Back's Review Essay.

(1) Nowhere in the book do we claim that "reinforced behavior will be *stamped in*, and that nonreinforced or punished behavior will be *stamped out*" (p. 1098, italics ours). This is an outdated and oversimplified conception of behavior propounded long ago by early behaviorists such as Thorndike and Watson which research simply does not support.

(2) Back states that "For all the vaunted scientific method, questions regarding the demand characteristics of the situation, experimenter effects, or what the subject really thinks about in the situation are not considered at all" (p. 1099). This assertion is wrong. In point of fact, the bulk of one entire chapter (7) and a good part of another (8) are devoted to the topics of internal validity, experimenter effects, and other social control features of experiments. Further, the function of "what the subject really thinks about" as a behavioral datum is discussed at several points throughout the book and even explored empirically in one study (Chapter 5).

(3) Back also claims that "The non-experimental evidence comes from so-called 'field studies' in the treatment and control of retarded and emotionally disturbed children" (p. 1099). There are two errors here. First, these studies in field situations were not "non-experimental"—they were, with one exception, all experimental. The one exception was an observational study designed to illustrate how experimental and non-experimental studies can be integrated. The second error is in his reference to "retarded and emotionally disturbed children." Of the five studies presented in this section, one had "retarded" children as subjects, three others had middle to upper-middle class normal children as subjects, and one had lower class slum children as subjects. Which of these were "emotionally disturbed" is anyone's guess.

(4) In referring to the subtitle of the book,

"The Experimental Analysis of Social Process," Back asks, "And where is the social process?" The social process is to be found in the laboratory studies investigating the effects of supervision on productivity, the development of imitation, cooperation, status differentiation, and communication networks. The field experiments investigate an array of social behaviors including cooperative play, team teaching, and a variety of other institutionally prescribed behaviors. These studies do analyze basic social processes as do a host of other studies referred to throughout the book.

(5) Finally, he suggests that we display an "extreme repugnance" to treat the true subject matter of sociology—"the functioning of human beings in society . . . ." Is it possible Professor Back has our book confused with another? In terms of the studies presented in full in our book, they were conducted in such settings as a child's home where the behavior under study was a boy's interactions with his mother. Other studies presented dealt with such everyday activities as cooperative play, imitation, manual labor, aggression, and children teaching each other. And these activities were investigated where they customarily occur, in preschools, a music school, a neighborhood church, and in a cottage home for young delinquents. The subjects were human, and their actions personally and socially meaningful.

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DON BUSHELL, JR., *University of Kansas*

## ERRATA

In the review of *Brazilian Secondary Education and Socio-Economic Development* in the February 1971 issue of the ASR, p. 155, the name of the second author is misspelled. It should have been Aparecida J. Gouveia, not Gouveia.

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(Listing of a publication below does not assure or preclude its subsequent review.)

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# AMERICAN SOCIOLOGICAL REVIEW

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## Format of References in Appendix

List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "REFERENCES." Use no italics and no abbreviations. For typing format, see the following examples:

Davis, K.

1963a "The theory of change and response in modern demographic history." *Population Index* 29(October):345–366.

1963b "Social demography." Pp. 204–221 in Bernard Berelson (ed.), *The Behavioral Sciences Today*. New York: Basic Books.

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1967 "The protection of the inept." *American Sociological Review* 32(February): 5–19.

Moore, Wilbert E., and Arnold S. Feldman.

1960 *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt (ed.)

1962 *The American College*. New York: Wiley.

THE CHANGING SOUTH: NATIONAL INCORPORATION  
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American Sociological Review 1971, Vol. 36 (June):399-412

*The South is herein viewed as a subsystem of a larger, American social system. The paper attempts to demonstrate, using a variety of socioeconomic and demographic indicators, the rapidity with which the South is becoming an integral part of American Society. The analysis is based upon a set of two-way comparisons: the South with itself over time; the non-South with itself over time; and the South with the non-South at specified points in time. The measures used are grouped into five areas: urbanization, industrialization, occupational redistribution, income, and education. The evidence indicates that in these sectors the South has been changing more rapidly than the rest of the nation for the past forty years and moreover is becoming increasingly indistinguishable from the rest of American society.*

THE American South has traditionally been viewed as a passive, backward, and often recalcitrant region of the country: a section whose existence the rest of the country would often like to ignore. Yet, in many ways, this image is deceptively simple. Such an image ignores the fact that the South, in most sectors of socioeconomic behavior, has been changing *more rapidly* than the rest of the nation during the last forty years.

Numerous and clearly visible changes are taking place in Southern society. These changes can be focused into two major areas of concern: (1) those changes which directly involve the processes by which persons in the South obtain a living, and (2) those changes which are more specifically concerned with daily life style. Under the first category might be included industrial and technological change, increased differentiation of the occupational structure, elaboration of transportation and communication facilities, and increased urbanization. Such development and all it entails have enabled

the South to become more like the rest of the American society in terms of its primary dimensions of living. Gradually the scope and responsibility of the educational system have expanded. Political traditions are crumbling, and the monolithic, Democratic South is no longer the deified or, possibly, even the viable power it once was. Race relations have altered, and, in some respects, the changes have been more substantial in the South than in the rest of the country. All of these developments change the demographic and ecological characteristics of the Southern region, and are rapidly transforming it into an integral part of the rest of the nation.

However, three traditional modes of observing and reporting about the South have prevented this integration from being realistically recognized. First, the historical tradition with its emphasis on the uniqueness and difference of the South has contributed heavily to a "Southern mystique." The presentation of "Southern history" has persistently emphasized the separateness and distinctiveness of the region in contrast to its shared experiences and characteristics with the rest of the nation. Second, the belletristic tradition with its emphasis on local color has presented a view of dramatic rather than objective value. The writers of Southern novels and plays have written with dramatic rather than representative purpose and have "extracted" from Southern life with legiti-

\*The authors would like to acknowledge the assistance of Marilyn Uyemura and the technical advice of Hallman H. Winsborough. The Duke University Research Council gave financial support to the project, which was conducted under the auspices of the Center for Southern Studies, Duke University. The paper was presented at the annual meeting of the Pacific Sociological Association, Honolulu, Hawaii, April 8-10, 1971.

mate but misleading license. Third, the mass media with their search for the "interesting and newsworthy" have placed an emphasis on the South's intransigence in race relations and the idiosyncracies of Southern politics. The mass media typically emphasize the extraordinary rather than the commonplace, the pathological rather than the ordinary, the newsworthy rather than the day-to-day routine. In aggregate, these three sources have drawn a picture of the South as a colorful land peopled by a variety of picturesque types of humanity—blacks, crackers, creoles, red-necked sheriffs, mountaineers, sharecroppers, itinerant preachers, snake handlers, planters, decadent aristocrats, Klansmen, and political demagogues; a South that is characterized by poverty, ignorance, backwardness, agrarianism, traditionalism, and extreme forms of resistance to change.

On the basis of hard evidence, this gives a greatly distorted and grossly inadequate picture of this sector of American society. When American Society is viewed as a social system, within which the South is seen as a subsystem, quite a different picture is presented. A social system may be defined as a network of role relations. This implies both foci of activity and continuity of interaction. A social system is sustained by established norms, beliefs, values, and sentiments. A subsystem subscribes to many of the norms, beliefs, and values of the dominant or systemic culture and yet retains certain characteristic norms, beliefs, and values which allow it to be defined in part as unique. These values and norms which appear to differ from those of the dominant culture generally differ more in degree than in kind. Occasionally, the values of the subculture come in direct conflict with those of the dominant culture, but this is more the exception than the rule.

In appraising the South, the tendency has been to emphasize these unique and conflicting aspects of the subculture rather than attempting to objectively observe those characteristics and relate them realistically to the numerous attributes, values, and norms, which the South shares with the rest of the United States. This faulty, often flagrantly inaccurate, perception of the South has been perpetrated by both Southerners and non-Southerners. For southerners this has taken the form of pride—we're really better than the Yankees, so never mind what they think

of us. For non-Southerners, the motivation has often been the need for a scapegoat. If we can sadly shake our heads over the racial attitudes of Southern whites, it isn't necessary to concern ourselves with the racial attitudes of whites in Boston, Detroit, or Los Angeles.

The Southern region, like other parts of the society, and indeed, like other parts of the world, is seething with active and frequently contradictory movement. In order to capture this sense of activity and movement, the South is herein viewed as a subsystem of a larger American social system. This system consists of a set of fundamental social institutions which organize the life of man and direct his capabilities in the continuing process of solving his problems and realizing his aspirations. These social institutions permeate all the regions and subsystems in America. Our focus in this paper is to show, using a variety of socioeconomic and demographic indicators, the rapidity with which the South is becoming an integral part of American Society. The analysis will be based upon a set of two-way comparisons: the South with itself over time; the non-South with itself over time; and, the South with the non-South at specified points in time.

#### INDICATORS OF REGIONAL INTEGRATION

In order to ascertain whether our hypothesis that the South<sup>1</sup> is increasingly becoming indistinguishable from the rest of the United States, a number of indicators can be examined. The measures used can be grouped into five areas: (1) urbanization, (2) indus-

<sup>1</sup> The South used in these analyses is the "Confederate South," which includes twelve states: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, Virginia. This is adjudged to be a more meaningful region than the "Census South." The processes and events preceding and consequent to the Civil War defined this South as a differentiated, separatist, and culturally distinct region. It is the persistence and diminishment of this cultural heritage which is viewed as problematic in this paper and is what is being explored sociologically. The non-South is defined as the conterminous United States minus the twelve states listed above. We recognize that a different grouping of states, more nearly matching the North as it existed in 1860, might yield somewhat different results and require a modification of some of our conclusions. However, the results of such an analysis, in our judgment, would tend to uphold our conclusions.

trialization, (3) occupational redistribution, (4) income, and (5) education. It is commonplace to think of these indicators as being interlinked and interdependent. Our purpose is to *describe* using the above indicators, the increasing similarity between the South and non-South, and to argue that the outcome of this process is the increased integration of the South with American society. Indices of dissimilarity<sup>2</sup> will be used to describe increased similarities between the South and non-South. These measures are summary and descriptive rather than analytic.

### Urbanization

In 1900, 11% of the national urban population resided in the South. By 1960, 21.5% of the urban population were located in Southern urban areas. Table 1 shows the dramatic movement of population into cities in the South. In 1900, only 16% of the total population in the South lived in cities as compared with 49% of the total population in the non-South. "Prior to 1940 the South

<sup>2</sup> The index of dissimilarity ( $\Delta$ ) for the South with the nation can be interpreted as the proportion of the population in the South that would have to change their place of residence or their occupation from one area of occupation to another in order to reproduce the nation-wide ratio of the relationship between residential areas or occupational groups. The index of dissimilarity has a range from zero, for the ideally distributed group, to 100 for a completely segregated group. The index is derived by taking the absolute differences between the percentages in each category for the two distributions to be compared, summing them, and taking half the difference. (See Duncan *et al.* 1961:83-90; Meidav, 1970; and Taeuber and Taeuber, 1965.) The index is used in Tables 1, 2, 3, and 4.

We have used partial deltas in Tables 3 and 4 since there is no other opportunity to utilize partials given these data. Partial delta is useful for determining whether a total relationship is possibly spurious and attributable to the merger of subgroups that differ in their statistical relationships. But our aim was to examine the outcome of a process without regard to the disaggregated details of the process. Our concern is with trends and their implications, and not with the causal process producing those trends. Hence, a simple descriptive measure such as delta is appropriate. One could utilize the same indicators comparing Japan and the United States with the possibility of having rather similar results. One would not, however, argue sociologically from those data that Japan was becoming more integrated into the American social system. It is *only* when the concept of social system is invoked that these indices can be utilized inferentially with respect to the implication of integration.

Table 1. Percent of Southern and Non-Southern Populations in Urban and Rural Areas, 1900-1960 (Population in millions).

Region	Year					ID* 1900- 1960
	'00	'30	'40	'50	'60	
South:						
Rural	84%	68%	65%	57%	48%	36
Urban	16	32	35	43	52	
Pop.	21	31	35	39	46	
Non-South:						
Rural	51%	36%	36%	34%	33%	23
Urban	49	64	64	66	67	
Pop.	55	93	97	111	132	
ID*	33	32	29	23	14	

Source: 1960 Census of Population, Volume 1, United States Summary, General Characteristics of the Population, Part A, Number of Inhabitants, Table 20, pp. 1-29.

\*ID stands for Index of Dissimilarity.

could fairly be described, with one or two states excepted, as a predominately rural region here and there dotted with cities" (Reissman, 1965:79). By 1960,<sup>8</sup> over half of the Southern population were living in cities as compared to 67% of the non-Southern population. Indices of dissimilarity, comparing the Southern and non-Southern rural-urban distribution of population, indicate a wide discrepancy between the two areas in 1900 ( $\Delta = 33$ ), which had narrowed significantly by 1960 ( $\Delta = 14$ ). The major thrust of this change has been since 1940.

In spite of substantial change within the Southern rural-urban balance since 1940, it should be noted that this change finds the South in 1960 with a rural-urban balance which is remarkably similar to that of the non-South in 1900. From the data available, it would appear that once a population becomes 60-65% urbanized, a balance of sorts is produced. This balance is probably dependent on a variety of industrial and occupational factors. Consequently, the rapid change in the South since 1940 reflects corresponding changes in industrial distribution (Tables 2 and 3) which had already been completed in the non-South. In particular

<sup>8</sup> When this paper was prepared, data on urbanization were not available for years following 1960.

Table 2. Number and Percent Agricultural and Non-Agricultural Workers 10 Years Old and over, 1900-1960 (00 omitted).

	1900		1930		1940		1950		1960		ID
	Number	%	Number	%	Number	%	Number	%	Number	%	1900-1960
<b>South:</b>											
Ag.	52,374	65.8	50,547	42.5	41,347	33.4	30,560	20.1	15,981	10.2	
Non-Ag.	27,221	34.2	68,421	57.5	82,432	66.6	117,581	79.9	139,454	89.7	55.5
<b>Non-South:</b>											
Ag.	60,506	28.7	54,173	14.7	45,657	12.3	39,068	8.6	26,597	5.4	
Non-Ag.	150,631	71.3	315,158	85.3	326,818	87.7	414,800	91.4	462,371	94.6	23.3
ID	37.1		27.8		21.1		11.5		4.9		

Sources: Population Redistribution and Economic Growth, Vol. 1, p. 609.  
 1960 Census of Population, Vol. 1, Characteristics of the Population,  
 Part 1, U.S. Summary, Tables 124-25, p. 1-270-1.

this change reflects movement out of agriculture and other extractive industries. During this period, the South has moved from a position as a specialized industrial subsystem relative to the total United States into a position more nearly equivalent with, and indistinguishable from, the greater system. Winsborough (1965:52) suggests that this "... makes the place of the South in the internal division of labor in the nation as a whole rather more similar to that of other areas within the nation."

In commenting on the phenomenon of Southern urbanization, Reissman has pointed to the relatively recent emergence of five metropolitan conurbations and two lesser metropolitan complexes that divide the South into new constellations.<sup>4</sup> These conurbations and chains now include over 40% of the population living in the South. The conurbations not only increase the South's resemblance to the national pattern, they signify the end of the region as a relatively homogenous unity and imply a new alignment in which the older boundaries and loyalties have less meaning and consequence (Reissman, 1965:96). One can expect these conurbations to increasingly transcend state and local boundaries and bring about the development of new alliances of common interest and outlook.

<sup>4</sup> The conurbations have been delineated as the Gulf Coast, Western Inner Core, Atlantic Coast, Carolinas, and Eastern Inner Core. The chains are described as Nashville-Memphis and Shreveport (See Reissman, 1965:93).

Population redistribution and the growth of urban conurbations is merely indicative of the deeper social, economic, and political changes the South has been experiencing in recent decades. As is well known, urbanization involves not only a change in locale and in the development of cities, it involves a change in attitudes, values, and institutions (Wirth, 1938). The social structure, characteristic of the older feudal and agrarian South, is being rapidly dismantled and left behind in response to the demands of urban living.

#### Industrialization

The movement from rural to urban areas reflects, or is correlated with, changes in the industrial and occupational structure which came about as a result of technological advances. During the 20th century, mechanization has both allowed and forced persons to leave the rural areas for industrial jobs in the cities. In 1900, 65.8% of all Southern workers were engaged in agriculture, as compared to only 28.7% of the non-Southern workers (See Table 2). This represents a large discrepancy between the two areas. By 1960, this discrepancy had almost disappeared ( $\Delta = 4.9$ ). The large index of dissimilarity ( $\Delta = 55.5$ ), comparing the South in 1960 against itself in 1900, seems to indicate that much more change has taken place within the Southern region than did, over the same period of time, within the non-Southern region.

Table 3 indicates the distribution of the

Table 3. Percentage Distribution of the Population in Industrial Categories for the South and Non-South, 1940-1968 (Population in millions).

Industry	Year				% Change '68/'40	Index of Dissimi- larity '40-'68
	'40	'50	'60	'68		
South:						
Agriculture	34.8%	21.7%	10.2%	10.4%	29.9	31.5
Forestry & Fishing	.5	.4	.3	.04	8.0	
Mining	2.0	2.0	1.6	1.4	70.0	
Contract Construction	4.4	6.8	7.1	5.6	127.3	
Manufacturing	16.0	18.7	21.8	24.7	154.4	
Transp., Commun., Pub. Util.	5.3	6.8	6.5	5.8	109.4	
Wholesale & Retail Trade	13.2	17.3	19.0	19.0	143.9	
Finance, Insurance, Real Est.	1.9	2.4	3.4	4.1	215.8	
Service & Miscellaneous	18.3	18.3	21.8	12.5	68.3	
Public Administration	2.3	4.0	4.9	16.6	721.7	
Industry, not reported	1.3	1.6	3.4		261.5	
Population	11	14	16	18		
Non-South:						
Agriculture	13.4%	9.2%	5.4%	5.4%	38.8	24.6
Forestry & Fishing	.2	.2	.1	.02	1.0	
Mining	2.1	1.5	.8	.4	19.0	
Contract Construction	4.7	5.9	5.5	2.6	55.3	
Manufacturing	26.3	28.4	28.8	17.7	67.4	
Transp., Commun., Pub. Util.	7.5	8.2	7.0	3.8	50.7	
Wholesale & Retail Trade	17.9	19.1	18.0	12.4	12.4	
Finance, Insurance, Real Est.	3.7	3.7	4.4	3.0	3.0	
Service & Miscellaneous	19.4	17.7	20.8	12.1	12.1	
Public Administration	3.4	4.6	5.0	14.1	14.1	
Industry, not reported	1.7	1.5	4.2			
Population	34	43	49	50		

- (a) Index of Dissimilarity between South and Non-South 21.7 ('40) 14.8 ('50) 11.1 ('60) 12.9 ('68)
- (b) Index of Dissimilarity for Agriculture 11.0 ('40) 8.5 ('50) 8.7 ('60) 10.4 ('68)
- (c) Difference between (a) and (b) 10.7 ('40) 6.3 ('50) 2.4 ('60) 2.5 ('68)

Sources: 1960 Census of Population by States, Social and Economic Characteristics, Table 62.

Statistical Abstracts, 1969, Table 316, p. 216.

1968 County Business Patterns, United States Department of Commerce (Mid-March Statistics), Tables 1a and 1b, p. 3.

labor force by industry over time. Clearly there is a much greater similarity in the industrial distribution between the South and non-South in 1968 than there was in 1940. Observations which should be particularly noted are: (1) the proportionate decline of the extractive and service industries; (2) the shift into public administration which occurred during the 1960's—a change which South and non-South have in common; and (3) the relative decline of manufacturing and wholesale and retail activity in the non-South during this period. In contrast, the South continues to increase its proportionate activity in manufacturing while stabilizing

its wholesale and retail activity. Comparing the South against itself provides an index of 31.5, whereas comparing the non-South with itself over time indicates 24.6. In contrasting the South and non-South in 1940, we have an index of 21.7, while in 1968 the index is 12.9. These two-way comparisons seem to indicate that the South has changed more during these three decades and that it is increasingly similar to the non-South in industrial pattern. The partial indices of dissimilarity for agriculture shown in Table 3 indicate the role of agriculture in this process.

Despite the appreciable industrialization

of the South in recent decades, it is necessary to call attention to the fact that there are still very significant differences in the *type* of industry characteristic of the two regions. The general pattern of the South is a movement away from extractive industries into "primary" industries representing a first-stage use of raw materials. In contrast, the general picture of the non-South is one of a movement away from primary industry into "secondary" (dependent upon inputs from primary industries rather than upon raw materials) and "tertiary" industries (most heavily dependent upon research and development activity). This means that, in contrast to the non-South, a very large segment of Southern industry—textiles, apparel, food and related products, and sectors of the furniture and wood products industries—is labor intensive. Labor intensive industries are "low-wage" endeavors since they employ a high percentage of semi-skilled and unskilled workers and utilize limited capital. They offer little upward mobility for the majority of workers, and productivity is low. The specialization of the South in this type of industry appears to reflect a stage of development rather than a fixed style or absolute pattern. During the 60's there was a noticeable increase in secondary and tertiary activity in the South. This was primarily a product of the increase and expansion of federal installations in the region and "capital mobility" in the private sector. Large multi-plant, national and international firms have been making profits in other areas and increasingly investing them in the South (Greenhut and Whitman, 1964). Capital inputs from outside the region are essential to a rapid economic development process. The fact that they are being made indicates that the present industrial pattern differences between the South and the non-South represent a temporal gap rather than a fixed division of labor.

### *Occupational Redistribution*

Occupational and industrial redistribution (Tables 3 and 4) are strongly correlated with the movement out of rural areas and agricultural jobs. Table 4 shows the movement out of agriculture within the context of the total job distribution in the South and

non-South between 1940 and 1960.<sup>5</sup> The partial indices of dissimilarity at the bottom of the table indicate that the greatest part of the change is accounted for through the movement out of agriculturally oriented jobs into professional, clerical, craft, and operative's jobs. Indices of dissimilarity indicate that more radical changes occurred between 1940 and 1960 in the South ( $\Delta = 29.9$ ) than in the non-South ( $\Delta = 13.1$ ).

Although the flux and change in the occupational structure of the South is impressive, one must take note of its persistently disadvantaged labor force as contrasted with that of the non-South. In the South's move toward the national norms, it is obviously confronted with a moving target. The labor force of the non-South continues to differentiate and to reflect qualitative upgrading. As a result the South continues to suffer by comparison although it has made impressive, absolute gains.

The South is still the most agricultural of the regions and its farmers are the nation's poorest. The types of industry it has developed provide proportionately fewer craft, white-collar, and professional jobs. By comparison with the non-South, it continues to have fewer people in the higher trained and rewarding occupational strata. Nevertheless, it is impressive to note that by 1960 the South had 50% of its workers in white-collar and skilled craft occupations in contrast to 29% in 1940. In 1940, 36% of those employed in the South were in agriculture in contrast to little more than 10% in 1960. Partial indices of dissimilarity in Tables 3 and 4 would indicate that almost all of the difference in occupational distribution between the two areas is due to differences in proportion of workers in agriculture.

Simpson and Norsworthy (1965:223) remark that the shift out of agriculture "... represents an almost unmixed blessing for the region; for almost any occupation is an improvement over southern agriculture except for the prosperous elite of farmers who ideally would be left to run large farms. The hard fact is that agriculture has always been, for most southerners in it even more than for

<sup>5</sup> When this paper was prepared, appropriate and comparable data were not available for the period following 1960.



Table 4. Occupational Distribution of the Population in the South and Non-South, 1940-1960 (Population in Millions).

Occupation	South				Non-South			
	1940 (%)	1950 (%)	1960 (%)	ID '40- '60	1940 (%)	1950 (%)	1960 (%)	ID '40- '60
Profess., Tech., & Kindred	5.7	7.0	9.5		8.7	9.3	11.7	
Farmers & Farm Managers	20.4	13.5	5.5		8.3	5.8	3.4	
Managers, Officials & Propri. except Farm	6.4	7.9	8.5		8.6	9.2	8.3	
Clerical & Kindred	15.7	8.8	11.7		11.1	13.5	15.3	
Sales Workers	5.1	6.3	6.9		7.4	7.1	7.3	
Craftsmen, Foremen & Kindred	7.8	11.6	12.5	29.9	12.8	14.6	13.8	15.1
Operatives & Kindred	14.1	17.9	18.5		19.3	20.5	18.4	
Private House, Workers	7.0	4.2	4.7		3.8	1.9	2.0	
Service, exc. Private House	5.4	6.6	8.8		7.7	7.9	8.5	
Farm Laborers & Foreman	13.9	7.8	4.8		4.6	3.1	1.7	
Laborers, exc. Farm & Mine	7.7	7.0	5.5		6.7	5.8	4.5	
Occupation not Reported	.8	1.4	4.4		1.0	1.3	5.1	
Population	11	14	16		34	43	49	

(a) Index of Dissimilarity between South & Non-South	25.6 (1940)	16.0 (1950)	8.7 (1960)
(b) Index of Dissimilarity between Farm Related Occupations and Non-Farm Related Occupations	21.4 (1940)	12.4 (1950)	4.4 (1960)
(c) Difference between (a) & (b)	4.2 (1940)	3.6 (1950)	4.3 (1960)

Source: 1960 Census of Population by States, United States Summary General, Social and Economic Characteristics, Table 127, pp. 1-276.

the farm population elsewhere in the United States, not just a horizontal sector of the nation's economy but a stratum at the bottom." It is anticipated that the general farm population and the number of farm operators will decline more rapidly in the South than in

other regions during the 1970's. The expected development of additional secondary and tertiary industry in the South will result in additional shifts in the Southern occupational structure. Again, this will be in the direction of the national norms.

Table 5. Per Capita Personal Income in the South and Non-South, 1900-1968 (Population in Millions).

	South						Non-South					
	'00	'30	'40	'50	'60	'68	'00	'30	'40	'50	'60	'68
Pop.	55	92	97	112	133	147	21	31	35	39	46	53
\$ Income	234	725	680	1,640	2,411	3,719	102	326	356	1,070	1,663	2,559

% (South/Non-South): 43.6('00), 45.0('30), 52.4('40), 65.2('50), 69.0('60), 68.8('68)  
 % Change (1968/1930): South 785, Non-South 513

Sources: Statistical Abstract of the United States, 85th Edition, Table 9, p. 12 and Table 446, p. 329.  
 Survey of Current Business, Department of Commerce, Table 2, p. 16, August, 1964.  
 Personal Income by States, Supplement to Survey of Current Business, Tables 2 and 3, pp. 142 and 144, 1957.  
 James Esterlin, Population Redistribution and Economic Growth in the United States, 1870-1950, Vol. I, Tables Y-1 and P-4A, pp. 753, 349.

*Per Capita Income*

As persons have moved off the farms, their relative income has increased (Table 5). The greatest increase in per capita income in the South, as in the United States as a whole, occurred between 1940 and 1950. However, the extent of this increase was greater in the South than in the non-South—300% in the South as compared with 241% in the non-South. This greater increase in income is explained in part by the shift in occupations which was occurring during the same period (1940–1950) in the South (see Table 3). With new sources of employment, per capita income began to rise.

Improved income has reduced the margin of the South's inferiority, but it has not moved Southern incomes up to the national levels. Note the virtual stabilization of the ratio between 1960–1968 (Table 5). The 12 Southern states regularly fall among the lowest 15 to 17 nationally in per capita income. Historically many factors have acted in combination to create this low income situation. Today, however, one can give credence to Spengler's (1965:117) statement that "Both average income in the South and capacity to increase it are depressed more by lack of training in the population than by any other condition, especially in the rural white population and in the non-white population, rural and urban." Consequently, the range of skills, levels of aspirations, and motivation have been relatively low. The long-term lack of investment in the development of "human capital" has its pernicious effects, but this situation is slowly but surely being altered by the convergence of modernization factors. As indicated previously, the Southern labor force is being dramatically upgraded and, therefore, one can reasonably expect a continued upward trend in per capita income.

As the individual income increased, the per capita savings in the South increased. In 1940 per capita liquid savings in the South were 15% of that of the non-South (\$66.84 per capita in the South and \$445.83 per capita in the non-South). By 1960, per capita liquid savings in the South were 42.4% of those in the non-South (\$467.70 per capita in the South and \$1,104.13 per capita in the non-South). This represents a 600% increase for the South in contrast to

a 147% increase for the non-South (Floyd, 1965:133). Certainly this development does not signify or suggest that the South will soon become economically self-sufficient. Much of the recent economic growth of the South must be attributed to capital inflows from other parts of the nation, and such inflows must continue and increase if the future economic growth of the region is to be assured. Capital inflows, together with increased amounts of Southern savings, make possible long-term investments in the development of the region. Such investments are gradually creating a Southern economic capability that more nearly approximates that of the rest of the nation. The combination of capital formation and labor force improvement is demonstrably having its effect. "The ultimate sources of our large and growing wealth . . . are to be found . . . in the accumulation of capital and application of technical progress by corporate enterprise and the acquisition of increasing skill and knowledge by individuals—the accumulation of human capital" (Johnson, 1962:166, 180). The historic pattern of capital scarcity, high interest rates, and short-skilled labor force has been broken in the South. As a consequence, a more rapid growth of Southern income, savings, and investment can be anticipated in the future—a movement toward national norms.

*Education*

With more money available within an industrializing area and with the increased demand for technical skills, the number of persons obtaining an education and the monetary investment in education has also expanded. Table 6 indicates that the South is about ten years behind the rest of the United States, and the ratio between the South and non-South would indicate that while the median education attained by the adult population has increased for both regions, the South has *not* gained on the non-South. However, Table 7 shows that school enrollments in the South were much closer to those of the non-South in 1960 than they were in 1950. Projecting these statistics, the implication is that the median education of persons 25 and over in the South should be very close to that of the non-South by 1980.

As the school enrollment has expanded, so

Table 6. Median Number of School Years Completed for Persons Aged 25 Years and Over (Population in Thousands).

	1940	1950	1960
Non-South:			
Population	55,741	66,758	75,161
Median Number School Years	8.7	9.5	11.0
South*:			
Population	19,035	20,813	24,277
Median Number School Years	7.6	8.4	9.5
(So./Non-So.) x 100	87.3	88.4	86.4

Sources: Statistical Abstract of the United States, 1964, Table 148, p. 114.  
 —, 1952, Table 132, p. 114.  
 —, 1953, Table 129, p. 129.  
Population, Vol. 2, Characteristics of the Population, Part 1, United States Summary, Table 26, p. 60.  
United States Census of Population: Educational Attainment, PC (2) - 5B, p. viii.

\*Persons who had progressed beyond the 7th grade were treated as though they were through the 8th grade. This reflects the fact that many Southern students had completed only 7 years of education when they entered high school prior to 1950.

has the per pupil expenditure, but, again, the South appears to be about ten years behind the non-South (Table 8). College enrollment and per pupil expenditure in higher education have also increased, but in these

indices the South appears to be slowly gaining on the non-South (Tables 9 and 10). Although the characteristic "lag" with respect to the rest of the nation remains, the South has obviously improved its capability of both investing in and capitalizing on education. Education, of course, is crucial with respect to the whole problem of creating human capital. The educational resources, increasingly available within the South, are rapidly effecting qualitative changes in the population with obvious economic consequences. Beyond that, however, there is another very relevant aspect of education, namely that it is a *generalizing* medium. Physics is physics, and mathematics is mathematics whether you study them in New Orleans or Chicago. As a much larger porportion of the Southern populace attends school for longer periods of time—approximating national norms—it is in effect sharing in a national (in many respects international) culture. Our educational system is truly national, both in organization and substance. As such it provides many commonalities of experience and outlook. By increasingly sharing in that sector of national culture, the region is inevitably divorcing itself from those values, attitudes, and folkways attached to the status and outlook of being "uneducated." The values and attitudes imputed to the South have historically derived from the older feudal and agrarian traditions and from an epoch in which education of any extended duration was for the elite. That epoch is past, and now education is for the mass of population, in the South as well as the rest of the nation. This is a major factor

Table 7. Percent of School-Age Population Enrolled in School in the South and Non-South, 1930-1960.

Age	South					Non-South				
	1930 (%)	1940 (%)	1950 (%)	1960 (%)	Percent Change (1960/1930)	1930 (%)	1940 (%)	1950 (%)	1960 (%)	Percent Change (1960/1930)
5, 6	26.7	27.4	39.6	44.4	187.3	50.5	50.9	62.2	71.1	140.8
7-13	90.0	85.3	94.6	96.8	107.6	97.5	99.8	96.0	97.8	100.3
14,15	81.3	81.6	89.8	91.6	112.5	92.0	93.6	94.3	95.1	103.4
16,17	56.0	56.1	65.5	71.3	137.4	59.5	74.0	78.2	93.7	157.5
18,19	23.0	24.0	28.5	40.2	175.8	26.3	30.9	33.8	43.0	163.5
20-24	5.5	5.0	11.3	12.5	227.3	8.1	7.2	13.6	15.5	191.4

Source: 1960 Population by States, General Social and Economic Characteristics, Table 45.

Table 8. Per Pupil Expenditures (Nearest Dollar) for Primary and Secondary Education (Population in Thousands).

	Non-South					South				
	1929- 1930	1939- 1940	1949- 1950	1957- 1958	1968- 1969	1929- 1930	1939- 1940	1949- 1950	1957- 1958	1968- 1969
Enrollment	18,087	17,590	17,292	23,627	32,585	7,591	7,844	7,819	9,902	12,376
Expenditure	\$105	\$107	\$240	\$377	\$863	\$ 43	\$ 46	\$140	\$255	\$597
% (South/Non-South)		41.1 ('29-'30)			43.5 ('39-'40)			58.1 ('49-'50)		
		67.5 ('57-'58)			69.2 ('68-'69)					
% Change (1968-69/1929-30)				South 1380, Non-South 822						

Sources: Biennial Survey of Education in the United States, 1956-58, Health, Education, and Welfare, Chapter 2, Table 17, p. 39, and Table 43, p. 73.  
Digest of Educational Statistics, 1969, Health, Education, and Welfare, Tables 25 and 74, pp. 22 and 56.

in the generalizing of the nation with a consequent diminishment of the salience and significance of regional differences.

#### DISCUSSION

Obviously, the indices explored above are only the tip of an iceberg—the increasingly identifiable similarities which can be readily isolated and measured. Underneath these measurable indices is a complex structure of

values, opinions, and attitudes which define whether a viable, distinctive regional culture exists. This substructure and changes which may have occurred within it are much more difficult to identify and measure. Yet there is good reason to believe that to the extent that the daily occupational and educational environment of the Southerner becomes similar to that of the non-Southerner, the attitudes and values of the two will also become indistinguishable.

Table 9. Percent of Total Population (in Millions) Enrolled (in Thousands) in College.

	Non-South					South					
	1929- 1930	1939- 1940	1949- 1950	1963	1968	1929- 1930	1939- 1940	1949- 1950	1963	1968	
Population	92	97	112	139	147	31	35	39	49	53	
Enrollment											
N	903	1,192	2,123	3,576	5,945	198	302	536	919	1,568	
%	.98	1.22	1.89	2.56	4.04	.62	.87	1.35	1.85	2.98	
% (South/Non-South)			63.3 ('29-'30)		71.3 ('39-'40)			71.4 ('49-'50)			
			72.3 ('63)		73.8 ('68)						
% Change			Non-South (1968/1930)		480.7		South (1968/1930)			412.2	

Sources: Statistical Abstract of the United States, 1964, Table 9, p. 12.  
United States Office of Education, Biennial Survey of Education, 1948-50, Table XII, p. 19.  
Digest of Educational Statistics, 1964, Table 49, p. 73, Department of Health, Education, and Welfare.  
Statistical Abstracts of the United States, 1969, Table 11, p. 12.  
Digest of Educational Statistics, Table 11, p. 12, Department of Health, Education, and Welfare.

Table 10. Current Expenditures (Nearest Dollar) Per Pupil Enrolled (in Thousands) in Higher Education.

	Non-South					South				
	1929- 1930	1939- 1940	1949- 1950	1963	1968	1929- 1930	1939- 1940	1949- 1950	1963	1968
Enrollment	1,495	1,116	2,163	2,547	4,714	471	288	718	679	1,206
Exp. per Pupil	\$170	\$413	\$642	\$1,177	\$2,169	\$103	\$384	\$441	\$892	\$1,895
% (South/Non-South)      60.4 ('29-'30)      93.0 ('39-'40)      68.7 ('49-'50) 75.7 ('63)                      87.4 ('68)										
% Change (1930/1965-66)      Non-South 1,277.5, South 1,847.5										

Sources: Population by States, General Economic and Social Characteristics, 1960, Table 45.

Biennial Survey of Education, Chapter 4, Section II, Table II, p. 80, 1957-58.

Statistics of Higher Education, 1939-40, 1941-42, Table IX, p. 11, and Table 16, p. 90.

Biennial Survey of Education in the United States, 1928-30, Part III, Table 16, p. 227.

Biennial Survey of Education, 1948-50, Chapter 4, Section II, Table 7, p. 52.

Statistical Abstract of the United States, 1959, Table 168, p. 125.

Digest of Educational Statistics, 1963, Table 120, p. 97.

Nichols (1960) has suggested that the historical Southern "tradition" with its persistent agrarian values, rigid social structure, and undemocratic political structure has prevented, or at least inhibited, economic development in the South. As the South gradually confronts its economic problems, it also loosens its traditional social rigidity, adopts greater social responsibility, and lessens its requirements for total conformity in thought and behavior. "Southern tradition is both gangrenous and dispensable and . . . , if the South wants a vigorous and prosperous economic life, it must be sacrificed for the good (progress) of the whole" (Nichols, 1960:x).

It seems reasonable to assume that the above evidences about education, occupational distribution, urbanization, and the like are salient indicators that (1) Southern tradition, as a preventative of social change, is gradually fading, and (2) the South is becoming increasingly integrated into the larger United States' social system. "Thus, the South has been choosing progress over tradition almost in spite of itself and done it so gradually as to be largely unaware of what was happening" (Nichols, 1960:157).

These indicators—and many others which could be offered—lend support to the two propositions which we suggested in the introduction of this paper. First, the South, in most sectors of socioeconomic behavior, has been changing *more rapidly* than the rest of the nation since 1930. Second, the South is becoming *more like* the rest of American society in terms of its primary dimensions of living. Paradoxically, as American society becomes larger and more complex in terms of its structural differentiation, it is also becoming more generalized. This growth of generalism has the effect of steadily eroding regional differences.

Although earlier we alluded to the relationship of education to generalization of culture, this phenomenon is the product of many operating factors. The great increases in the variety and utilization of *modes of exchange* between the people involved are major causes of cultural generalization. Among the many modes operative, it is necessary to make special mention of the role of transportation and mass communication. The development of modern transportation facilities has facilitated access to and exchange with regions.

Traditionally the transport network has been keyed to the railroad. This is no longer true, but it is still noteworthy that rail traffic between the South and non-South continues to increase. The great expansion of automobile ownership, the development of the interstate highway system, the great elaboration of roadways, and the dramatic increase of use of the airplane for long-distance travel have literally brought the regions closer together.

Similarly, the saturation of the nation with telephones and the development of the long-distance telephone system have facilitated communication. The influence of mass communication via radio, television, and the printed word seems incalculable. The now common practice of reading urban newspapers which in turn are fed by national wire services provides a common informational input no matter where one lives. The effect of the ubiquitous radio is similar. Perhaps most important of all, however, is the effect of television as an instrument of mass culture. The South, like the rest of the nation, is blanketed with television sets. Viewers everywhere are bombarded by the same programs as a consequence of the network approach to programming. This is in effect subjecting a total population to a common culture and thereby standardizing it. Influences such as these have served to break down and destroy the traditional isolation of the Southern region. This isolation has been geographic, mental, and social. The South is being transformed by its multiple contacts with the rest of the nation and with a resultant further involvement in the activities characteristic of the nation generally.

As involvement with the greater society increases, this change does not say that the special traditions and local characteristics which make the South distinctive will "disappear." Rather, it is merely to say that the similarities with and involvements in American society will become increasingly predominant. The changes which have occurred in the last forty years in the relationship between the Southern region and the remainder of the nation are in keeping with the forward looking assessment made by Howard Odum in 1936:

There is no longer in the United States any single entity which may be designated as "the

South." . . . It is . . . neither possible nor desirable to present a single authentic picture of "the South" any more than it is of "the North" or "the East" or "the West," . . . the combined interrelated forces of state, regional, and national agencies are predicated as the minimum practical requirements for the Southeast which must somehow combine a new motivation and realistic design with adequate stabilizing and permanently reinforced agencies of action . . . [R]egional-national co-operation as the minimum essential for bridging the extraordinary chasm between the region's possibilities and its actualities thus reappears as the practical test of the region's capacity for development. The urgency of the situation justifies the conclusion that this test of American Regionalism should be made before the turn of the mid-century. (Odum, 1936:5, 603)

At the time of Odum's assessment, Southerners were generally incredulous and mildly hostile toward his ideas. Nell Battle Lewis, a columnist for the *Raleigh News and Observer*, stated after reading Odum's book:

Now I am the last woman in the world to lift a finger against genuine regional analysis (whatever that is), but the South continues to exist for me . . . Dr. Odum can keep his 'social science' and be damned, and I shall keep the half-mystical tenderness of October in North Carolina . . . Detrimental to genuine regional analysis my eye, but very warming to the cockles of my heart [sic].

Today the evidence in support of Odum's view is far stronger. While Lewis' attitude continues to predominate for many Southerners, increasingly residents of the South see the advantages of a viable incorporation of their region into the economic, financial, and communications networks of the larger society. The nostalgia for the traditions of the South and the attitudes and values which support and emanate from these traditions are not going to die immediately. Change is often painful and the breaking down of old standards and barriers is no exception. The sentiment involved is not different in kind from that expressed by an older generation as it watches in relative helplessness the assault by youth on established ways of thinking and doing things. Such sentiment, even if deeply felt, will evidently have little effect on the course of the future in comparison to the effect of the processes of development and change now in motion. The directionality is expressed in those processes.

The pace of change in the South after a long period of latency has accelerated rapidly since 1940. History is *event-structured* and the events since that time have had a profound effect on the South and have inexorably drawn it into the national corpus. World War II had a major impact on the South. Millions of young men received their military training in the region. Conversely, millions of young Southerners in service left the region for the first time to see and experience other parts of the world. They were not the same on their return. This period also precipitated a redistribution of the population, altered migration patterns, and accelerated the urbanization process. The rapid expansion of communication and transportation facilities in the post-war period facilitated further economic development. The establishment of the space and missile programs with their huge Southern installations presented the South with a new role and capability. The U.S. Supreme Court decisions on school integration in 1954 and urban representation in 1962 have had a profound effect on the social and political climate. Implementation of those decisions is still only partially complete but is proceeding in waves of activity. The educational systems of the South, despite resistance, are being restructured. Similarly the political influence of the urban areas has increased. Finally, the massive and dynamic social movements of the 1960's have had a *national* impact inclusive of all regions. The civil rights movement, launched in the South, and originally directed toward the attainment of objectives in the South, mushroomed into an organized black militancy, intensely dedicated to racial freedom everywhere and in all spheres of activity. It not only quickly "nationalized," it influenced the attitudes and postures of other ethnic minorities. The manifold youth movement which has flourished since the mid-sixties has been national, indeed international, in scope. The women's liberation movement is as dedicated to the elimination of the "Southern belle" as it is to the liquidation of "dragon ladies." The burgeoning concern over environmental quality and pollution control and abatement is a national phenomenon. Movements such as these are essentially

*urban* phenomena and hence regional boundaries are of little consequence to their spread and effect. They are manifestations of divisiveness and conflicts within the society as a total social system, not a regional conglomerate.

In sum, the present South exists in one of those moments in time in which deep concern in it for the continuity of traditional values, clashing with equally deep concern for advance toward specific as well as general goals, has provided wide discrepancies and imbalances between the verbal and the real situation. Techniques of institutional self-preservation are being devised to counter movements and strategies looking toward change. There have been many such periods in the history of mankind which to the peoples most immediately involved have been, of course, profoundly disturbing. Although these last few remarks have been directed toward the South, upon reflection it would appear that they would apply with equal validity to American society.

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## HOMICIDE AND A REGIONAL CULTURE OF VIOLENCE

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*American Sociological Review* 1971, Vol. 36 (June):412-427

*The homicide rate in the United States has been very high relative to the rest of the modernized world for as far back as evidence is available. It has long been known that in the United States there is a wide variation in the rates of different races and between North and South. Both qualitative historical evidence and multiple regressions indicate that the degree of "Southernness" in the culture of the population of the states accounts for more of the variation in homicide rates than do other factors such as income, education, percent urban, or age. It is suggested that high homicide rates in the United States today are related primarily to the persistence of Southern cultural traditions developed before the Civil War and subsequently spreading over much of the country.*

A NUMBER of theories have been advanced on why the homicide rate in this country has historically been higher than that of other economically advanced nations. This is particularly important today because homicide is considered to be one indicator of the violence that some critics believe characterizes our culture. Meanwhile, it has been known for a long time that homicide rates among Negroes and

Southern whites are well above national averages. In this paper I will develop an argument and present statistical evidence that suggests more conclusively than previous studies that it is a predisposition to lethal violence in Southern regional culture that accounts for the greater part of the relative height of the American homicide rate. This regional culture was already developed before 1850. Popularly mentioned



causes of violence such as poverty or inequality or ignorance seem of relatively less importance, at least as far as the historic homicide rates are concerned. It is hoped that these findings will help to define the problem for later research in homicide and certain other aspects of American violence.

Although there have been few studies of homicide, reference to a broad spectrum of research may be found in Wolfgang and Ferracuti, *The Subculture of Violence* (1967). In a less theoretical manner Wolfgang's *Patterns in Criminal Homicide* (1958) summarized what was known up to that time and presented a detailed study of a series of homicides in Philadelphia. Perhaps his most controversial finding was that murderers are generally people who have been involved in crime previously, as are a large proportion of their victims. He confirmed the general impression that most murders are intraracial, which is related to the evidence that they are among those known to one another. (Since this evidence allows us to use *Vital Statistics* for murder rates, investigators can examine these rates with more assurance than any other criminal rates.) It is generally established that in the United States murder is more often committed by blacks than whites, by lower class than middle class, by men than women, by Southerners than Northerners.

Wolfgang offered evidence that Negro rates in Philadelphia could not be accounted for on the basis of social class alone (1958: 37-38). Although most murders are committed against persons known to the murderer, and often within the family, murderers and victims tend to be engaged in criminal activities or living within a seriously disturbed, criminal milieu. However, criminal activities and disorganization are associated with murder much more in some groups than others. The Irish, for example, have long had a reputation for a great deal of scrapping, but this very seldom results in murder. While alcohol has been shown to be highly correlated with murders in Philadelphia, the heavy drinkers of Ireland today are little inclined to murder no matter how drunk they get. We should not be surprised by this result, for we have learned that behavior, no matter how rational or irrational,

is almost always patterned in terms of the culture of the actors.

The remarkable height of the murder rate for the United States can be established by considering Table 1. It should be noted, however, that U.S. rates are high only for a developed country. Comparisons of figures for the British Isles with those for Australia or Canada suggest that we should expect the settled colonies to have about twice the rates, today, of their original homelands'. (On the other hand, if we compare other relatively developed countries in the Americas with little remaining Indian population, e.g., Chile and the United States, with their homelands, we see that the rates are several-fold higher.) The U.S. rate for whites alone in 1966 was 3.1, still about three times what we should expect in terms of the general experience of the developed world.

There have been a number of studies of the differential between Northern and Southern behavior in homicide statistics (cf. Tables 2 and 3). These include works by Hoffman (1925), Brearley (1932), Porterfield (1949), Shannon (1954), Lottier (1938). Hoffman was particularly concerned by the high American rates compared to the rest of the world, going back at least to 1910, and not at all a rural phenomenon. He especially noted the city of Memphis which led the nation for a number of years with rates up to 88 per 100,000. Brearley,

Table 1. Homicide Rates (per 100,000) for Selected Countries, 1966.

Colombia	21.2	Canada	1.3
Mexico	18.7	Scotland	1.1
Thailand	14.9	W. Germany	1.1
Philippines	10.0	Italy	0.9
Venezuela	8.7	Sweden	0.8
Chile	6.4	France	0.7
U.S.	5.9	England	0.7
Argentina	5.8	N. Ireland	0.5
Japan	1.4	Ireland	0.4
Australia	1.3	Spain	0.2

Data from World Health Statistics Annual, 1966, World Health Organization, Geneva, 1969. (1965 data were used for Italy.)

a Southern scholar, seriously explored the reasons for differences between Northern and Southern states, developing figures for the 1920's comparable to those found today. He also was qualitatively interested in the traditions and quality of life that might be responsible. The later studies did not make the point as sharply because of their concern with other issues such as general crime rates or the relation of homicide and suicide.

In a recent review of the problem of Southern violence, Hackney (1969:479-500) questioned several noncultural explanations. He pointed out that many theories of differences in child training practices, frustration-aggression etc., do not hold up under examination. For example, it is the less competitive, less commercially minded rural South that has historically been the source of high homicide rates. European countries with relatively rigid child-training practices have much lower rates. In this country economic and status positions in the commu-

nity cannot be shown to account for differences between whites and Negroes or between Southerners and Northerners. While the possession of firearms does contribute to actual murder statistics, laws against firearms often seem ineffective in the South, and differences in aggravated assault rates reflect the murder differentials in any case.

This suggests that a cultural explanation be given particular emphasis in explaining American homicide rates, and that this explanation must be primarily based on an understanding of the influence of Southern regional culture. Psychological, societal and cultural explanations have been advanced in a number of forms, and each "explains" part of the evidence. For example, Henry and Short (1954) tried to tie the first two together by viewing murders as aggressive reactions to frustration, with differences in rates stemming from variations in the social situation of groups or classes. Cultural explanation can be related to general theory in criminology by pointing out that cultural differences both produce and result from "differential association" (Sutherland, 1966: 80-98; also Cressey, 1969). This is particularly true if "culture" refers to all group-related differences in learning (Gastil, 1961). In groups with high murder rates, individuals have on the average (or a few regularly have) a different set of learning experiences than those in groups with lower rates. There is a different balance of culturally defined rewards and punishments, and these are determined by differences in the subcultures of the respective groups.

The following material will support the case that persistent differences in homicide rates seem best explained by differences in regional culture. Qualitatively a number of elements in Southern life and very early references to a Southern tendency to lethal violence lend support to the cultural explanation. With the mingling of the American population through internal migration, the Southern tendency to violence has diffused broadly, but the differences between sections of the country in homicide rates can still be related to an inferred degree of Southernness based on migration patterns. This paper is concerned primarily with the transmission of Southern culture from generation to generation within families and the movements

Table 2. Homicide Rates for 24 States, by Color, 1920 and 1925.<sup>†</sup>

State	White	Non-white	State	White	Non-white
Fla.	12.5	62.9	Wash.	4.8	26.0
Ky.	8.7	49.0	Mich.	4.7	88.9
La.	8.7	35.0	N.C.	4.7	20.8
S.C.	8.6	18.5	N.Y.	4.4	28.7
Miss.	8.3	32.0	Pa.	4.4	26.7
Tenn.	8.0	48.8	N.J.	4.1	20.9
Ala.	7.4	34.4	Ind.	3.9	65.0
Ill.	7.2	72.4	Kans.	3.8	36.9
Cal.	7.1	27.5	Md.	3.2	21.2
Mo.	6.9	66.9	Del.	3.0	9.8
Va.	6.7	20.0	Mass.	2.3	12.8
Ohio	5.1	70.1	Wis.	1.8	28.4

<sup>†</sup>States are excluded for having too small a sample of nonwhite homicides or because of lack of data for 1920 and 1925 (averaged) (Brearley, 1932:99). It must be remembered here, and below, that for a number of states, especially Washington, nonwhite is significantly affected by other than Negro populations.

Table 3. State Homicide Rates, by Color, 1960.

State	White	Non-white	Total	State	White	Non-white	Total
Ga.	4.4	27.3	10.0	Mich.	1.8	20.4	3.6
Ala.	4.2	25.3	10.5	W. Va.	3.1	13.3†	3.6
S.C.	4.9	19.5	10.0	Haw.	2.5†	3.9†	3.5
Va.	6.9	20.2	9.7	Ohio	1.9	21.3	3.5
Alas.	6.3	19.2†	9.3	N.Y.	1.8	18.7	3.3
Fla.	3.8	34.4	9.3	Ind.	1.8	22.6	3.0
N.C.	3.9	24.9	9.2	Kans.	2.2	18.7†	2.9
La.	3.1	19.1	8.3	S.D.	1.7†	33.3†	2.9†
Nev.	7.2	18.2†	8.1	Pa.	1.5	18.0	2.7
Miss.	2.5	15.5	8.0	N.J.	1.6	13.8	2.6
Tenn.	3.7	27.0	7.6	Wash.	2.2	12.8†	2.6
Tex.	4.3	30.7	7.6	Ore.	2.0	27.0†	2.5
Ark.	3.7	21.7	7.5	Conn.	1.2	18.0†	2.0
Ariz.	4.5	27.3	6.8	Neb.	.87†	40.5†	1.9
Ky.	4.5	27.1	6.1	Me.	1.8†	0.0†	1.8†
N.M.	4.8	22.9†	6.1	Idaho	1.1†	30.0†	1.5†
Okla.	4.0	23.2	5.8	Wis.	1.3	12.9†	1.5
Md.	2.5	21.3	5.6	R.I.	1.3†	4.8†	1.4†
Del.	2.3†	24.2†	5.4	N.D.	1.0†	23.1	1.4†
Wyo.	4.3†	42.9†	5.2†	Mass.	1.1†	10.4†	1.3†
Ill.	2.3	25.1	4.7	Minn.	1.1†	16.7†	1.3†
Mo.	2.8	22.9	4.6	Iowa	.9†	10.3†	1.0†
Cal.	3.3	18.3	4.5	N.H.	1.0†	.0†	1.0†
Colo.	3.0	32.1†	3.9	Vt.	.0†	.0†	.0†
Mont.	3.1†	25.0	3.9				

†20 or fewer homicides.

of successive generations among states. The exchange, both horizontally and vertically, of cultural patterns of violence or nonviolence among peoples with differing backgrounds is considered when we speak of the influence of elites on the general populace, but this type of transmission should be examined in more detail elsewhere.

Before turning to the evidence, we must consider the limitations on both differential association and cultural variation as explanatory concepts. Both Sutherland's theory and the theory of a regional culture of violence advanced here are suggested as explanations of the epidemiology of crime or a particular crime (cf. Cressey, 1969). Neither predicts how an individual will behave, but both suggest that past rates in a group are the result of, and result in, a continuing cultural tradition that will tend to produce

similar rates in the future, and that external changes in the context of a group will be only slowly reflected in changing behaviors. Whatever the problem of the individual—psychosis, marital troubles, personal danger—the outcome for that individual is more likely to be homicide in a group with a culture of lethal violence than in another group. There should be no attempt in any social science theory to suggest that *all* of anything categorized so loosely as "crime" or even "murder" can be explained by a single theory, particularly an epidemiological theory. The relative magnitudes of age-standardized murder rates are determined by four additive and interactive conditions or states: (1) the universal conditions of social life, (2) the rates of certain other criminal activities, (3) the extent and severity of "disorganized conditions," and (4)

the cultures or subcultures of the population (these vary in the extent to which the above conditions lead to lethal violence). It is primarily with the fourth condition that cultural explanations such as differential association should be concerned, although many other sociological and psychological explanations are primarily concerned with the first three. Without going deeply into these other factors, it is suggestive of universality that as murder rates go down, relatively more murder is committed by females and those in middle and upper classes (see Wolfgang and Ferracuti, 1967:258-263 and references cited there). However, the low murder rates of areas, such as the British Isles (Table 1), suggest that less than a tenth of U.S. rates could be explained as a universal accompaniment of social life. Since in Wolfgang's (1958) study murder was associated with more general criminal careers for over half of his sample, there is probably some relation of murder to other crime. In the last few years with rapidly rising general crime rates in the nation, homicide rates have also been edging up, probably because of their ties to other types of crime. However, only a small part of the historical experience of homicide rates can be explained in this way. The areas of highest crime rates have often been areas of relatively low homicide rates and vice-versa. (For recent statistics see FBI 1969: 2-6, 54-63). Turning to the third general category of explanation, in the United States disorganized conditions such as those found on the frontier (Graham and Gurr, 1969: 93-233) and in the skid roads of central cities have been related to high murder rates. In these situations, one finds a high ratio of males to females, relatively few persons living in organized families, high mobility, and an anonymity that tends to attract people with criminal tendencies or psychological abnormalities (Schmid, 1960: 655-678).

The theory of a subculture of violence advanced by Wolfgang and Ferracuti (1967) is similar to that of a "regional culture of violence" advanced here. There are, however, several differences. The concept of a subculture of violence is explicitly based on a view of culture that stresses norms and values (Wolfgang and Ferracuti, 1967:97

passim) while the regional concept does not. Violent people do not necessarily develop a culture that condones violence. A violent tradition may be one that in a wide range of situations condones lethal violence, or it may be a tradition that more indirectly raises the murder rate. For example, the culture may put a high value on the ready availability of guns, or it may legitimize actions that lead to hostile relations within families or between classes, and these in turn may frequently lead to lethal violence. The regional concept also suggests more persistence over time and intergenerational reinforcement than does the subcultural concept. As an additional note, in this paper "violence" means "lethal violence"; Wolfgang and Ferracuti seem at times to have a broader definition. In any event, differences in the lethality of violence among groups should not be obscured. If "subculture of violence" were given a broader significance and limited to lethal violence, then the concept would be fully complementary to what is being advanced here. In these terms a society characterized by a regional culture of violence would be likely to be characterized also by: (1) more extreme subcultures of violence and/or a larger percentage of the population involved in violence (with less limitation by class, age, race etc.); (2) lethal violence, a more important subtheme in the general culture of the region; and (3) weapons and knowledge of their use as an important part of culture. The concept of a regional culture of violence will help to explain why subcultures of violence do not develop equally everywhere under apparently similar conditions and go only part of the way toward explaining the particularly lethal subculture among black Americans.

After establishing the theoretical context, let us return to a more detailed examination of the evidence for a regional culture of violence. If Southern culture is primarily responsible for the high murder rates in the United States, then at what time and in what way did Southern culture come to support such a noticeable tendency? John Hope Franklin (1956) has diligently explored the Southern tendency to violence in the period before the Civil War. Franklin's evidence for a violent Southern culture in

the antebellum era ranges from the continued institutionalization of the duel long after it had been given up in the North to the love of violence on much lower levels of society. The rulers of the South were seldom any longer the more aristocratic Southerners of Virginia that ruled during the golden age of the republic. Other than carrying the Southern-Northern difference back to 1800, Franklin also documents the relation of Southern violence to an expansive foreign policy and to the deep Southern interest in everything having to do with military training and display.

More important are clues to the Southern tradition that Franklin offers. Large landholders generally came to the South for easy wealth to establish for themselves the type of feudal estates that were not available to them in England. In the 19th century their vision remained that of the exploiting, 17th-century rural aristocracy, and many reached their goal through the availability of poor indentured whites and then Negroes. Extreme class differences were expected from the beginning, and the poor white and Negro fit into this picture, as did the acknowledged fact that such societies had ultimately to be maintained by violence. It is also important to remember that more whites in the South came from a low status in England than was true in New England.

However, perhaps more important is the fact that the South remained a frontier society geographically for a much longer time than the North. Settlement in the North was to a greater extent continuous and contiguous; towns were more important and closer together and cities grew more rapidly. Until the Civil War, many parts of the "settled" South were wilderness and remained wilderness, suggesting that there might have been a decline in civilization in the South among the descendants of the immigrants, regardless of the original culture. Distances made schools harder to support or reach, and made the frontier skills with rifle and knife much more vital generation after generation than they were in the North. In the North this unsettled, dispersed, isolated type of life was only experienced by one generation in most places; the people could preserve in their memories

the image of another more civilized life until it became possible for their children.

In a careful study, H. V. Redfield (1880) examined the difference between murder rates in the North and South in the 1870's. A native of Cincinnati, he travelled widely in both areas, but particularly the South, gathering data from official statistics and developing complete newspaper files. As close to the Civil War as he was, he believed that the Civil War was not an important cause of Southern violence, for the patterns considerably antedated the War. He compared rates for "old states," North and South, and for frontier states, finding that South Carolina was as much in excess of New England as Texas over Minnesota. In both cases he estimated well over ten times as much murder per 100,000 in the South as in the North. He clearly saw that Texas was populated largely by the old South, and Minnesota by the North. By comparing counties in Ohio, Indiana, and Illinois, he noticed that the line between North and South ran through these states, while Iowa was characterized by Northern rates.

Redfield identified a number of different patterns of Southern murder. People were killed because of various kinds of "difficulties." Drunken brawls in the South might not be too different from those in the North, yet in the South they more often led to murders, both because of attitudes and the practice of carrying guns or knives. Murder might also occur to redress insult to personal honor, or because of a tough's desire to show off. Groups, whether they be gangs or clans, often attacked rival groups out of animosity or for political ends. "Feuds," as such, play less of a part in Redfield's analysis than we might expect of one so familiar with the situation in Kentucky.

Two aspects of history that we understand today rather differently were illuminated by Redfield. (1) The western street duel of later romance was described as Southern, as characteristic of South Carolina as of Texas. (2) The stealthy night killing by an organized group or mob was common throughout the South, though especially in Texas. Murder of this type might be accomplished by ambush, by a raid on a house, including the wiping out of the whole family as in the recent Yablonski case, and might

include a lynching. It is pertinent that the pattern of terror we associate with Ku Klux Klan attacks on Negroes was a white against white pattern as well (Brown, 1969, esp. p. 184).

It is useful to consider what Redfield regarded as the forces that maintained the tradition of violence in the South. Most generally he believed that there was a lack of regard for human life; he also believed that the South's exaggerated sense of honor contributed, as did the unnecessary carrying of weapons (apparently much more common then in the South than it is today). He pointed out that in many rural areas a lone law officer was often powerless or afraid to intervene—the rate of death by violence to peace officers was very high at that time—particularly in Texas. But even if caught, the murderer was unlikely to be convicted, and was seldom given a heavy sentence. This seemed to be due to several factors: (1) the jury was not likely to take the crime as seriously in the South as in the North; (2) it was more likely to accept the reasons given as justifying the killing; (3) the jurors themselves had often been involved in violent affrays and might be again, and they

feared the possibility that a relative or friend of the murderer before them might be on their jury in the future; (4) if they convicted the murderer, they might be in direct danger from relatives of the murderer; (5) the murder might have been committed for reasons they directly or indirectly approved (e.g., a political murder). Redfield gives many examples of his belief that murder was seldom punished as severely in the South as in the North, including a number of cases of men who had committed several murders previously being brought to trial for another murder.

After Redfield, the basic data on Southern-Northern homicide differentials have been presented a number of times. Table 2 gives the figures developed by Brearley for 1920 and 1925. Table 3 gives both white and nonwhite figures for 1960. The definite spatial arrangement of these figures is depicted by Figure 1 in which homicide rates have been averaged for 1964–1965.

In spite of considerable work there has been relatively little attempt to see how much of the Northern-Southern differential could be attributed to factors other than a separate, identifiable homicidal trait

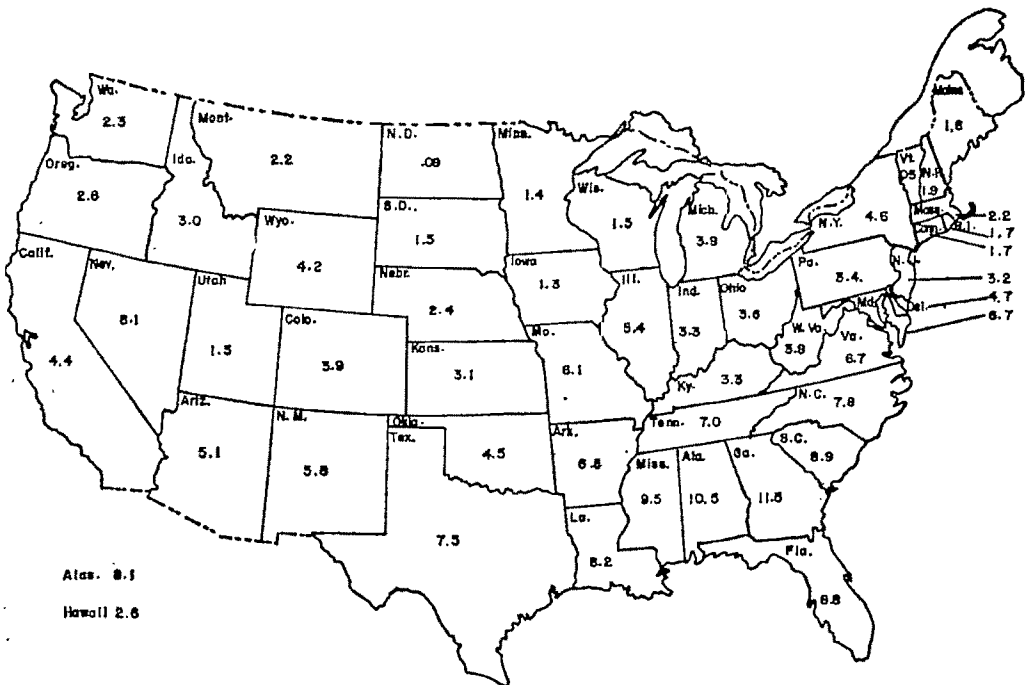


FIGURE 1. Murder and Non-negligent Manslaughter: 1964–1965 Averaged "Offenses Known to Police" (Statistical Abstract, 1967, p. 151)

among those of Southern background. In attempting to apply the multiple regression approach to this problem, I have included evidence relating to a number of alternative hypotheses. It has long been thought that homicide in this country was associated with factors such as low education and low income. In particular, the positive relation of "low status" to homicide suggested by Henry and Short may be checked by including these factors. Popularly it is believed that crime is related to urbanism or the large city environment, while others have pointed to its rural affinity (Henry and Short, 1954: 90-92). It is obvious that variations in the percent of a population at ages (20-34 or 18-45) with high murder rates would also be responsible for much of the difference in the reported rates. Since murder rates reflect victim survival rates, a decline in murder rates has been associated with improving medical services (Wolfgang, 1958:116-118); it also seemed reasonable to suppose that differences in the availability of medical services might affect murder statistics. One could only discover the influence of "Southernness" \* while controlling for such factors. Of course, even if it appeared that the apparent influence of "Southernness" \* on homicide rates could be explained by those common sociological variables, such as low education and income that are also associated with Southernness, one could imagine that Southern culture "caused" them all. However, there would not be, in this case, a separate regional cultural trait for homicide, and the claim of Southern cultural influence on the rate would be much harder to make convincing.

It is difficult to define exactly where and in what proportions the influence of Southern and Northern culture are to be found today. To examine the relation of homicide to Southernness<sup>1</sup> however, we must, give a

\* Elsewhere in the text we give the conventional spelling of "Southernness," except in the tables and Figure 2, where the spelling is as given by the author.

<sup>1</sup> The author's current research is primarily in the area of regional cultures. Aside from standard histories, the patterns of diffusion of population westward have been described by Semple (1933). A basic typology of the regional cultures and their distribution may be found in Arensberg (1955: 1142-1161). Another way to determine the relative influence of Southernness is to examine the

numerical value to its cultural influence in the states, or other units, we wish to examine. For this purpose an Index of Southernness (see Appendix and Figure 2) was constructed on the basis of available evidence with a high score of 30 given the most purely Southern states and a low score of 5 to the least. This is at least an improvement over a simple South or Non-South dichotomy; it makes it easier to check the validity of the regional differences in homicide rates in 1960 that Redfield noted in 1880. The Index reflects the parallel expansion of our population from a core of New England culture and a core of Southern culture, with migrations generally much more out of the South than in. To this must be added the fact that immigrant groups after 1880 came almost exclusively to non-Southern areas, and Southerners have migrated very little to New England or northwest of Illinois and southern Iowa. They have had overwhelming influence in parts of the Southwest. Southerners made up a large part of the original migration to the Far West and Rocky Mountain states, and of the later migration to work in industry in the Middle States from New Jersey to Illinois. As the Index suggests, Mormon Utah is relatively uninfluenced by the South.

Applying standard techniques of multiple correlation to the murder statistics, we ob-

distribution of religious affiliation (Zelinsky, 1961; Gaustad, 1962). The use of housing types to differentiate regions establishes similar boundaries (Kniffen, 1965). Another approach is to consider the linguistic evidence (Kurath, 1940:331-345; Mencken, 1963). A recent popular attempt by Phillips (1969) to analyze voting patterns in terms of ethnic and regional affiliations results in an analysis of the nation roughly similar to my Index (especially pp. 47, 94, 122, 209, 295, 302). The reader may wish to compare the maps of Wallace's and Humphrey's voting strength in 1968 with my Index (Phillips, 1969:28).

Specific studies of areas or communities used in determining the Index include those of Meinig (1965:191-220; 1969) for the Mormon and Texas cultural areas; Pollard (1951:187-214) for the Pacific Northwest; and Vogt (1955:1163-1173) for an Area in New Mexico.

It should be remarked that there are several "Souths." Sometimes the Kentucky-Tennessee area and its cultural extensions into the Midwest are excluded from the South, e.g. in Zelinsky (1951: 172-178), and in Kniffen (1965). However, for the cultural trait of violence, a broader concept of the South is more useful.

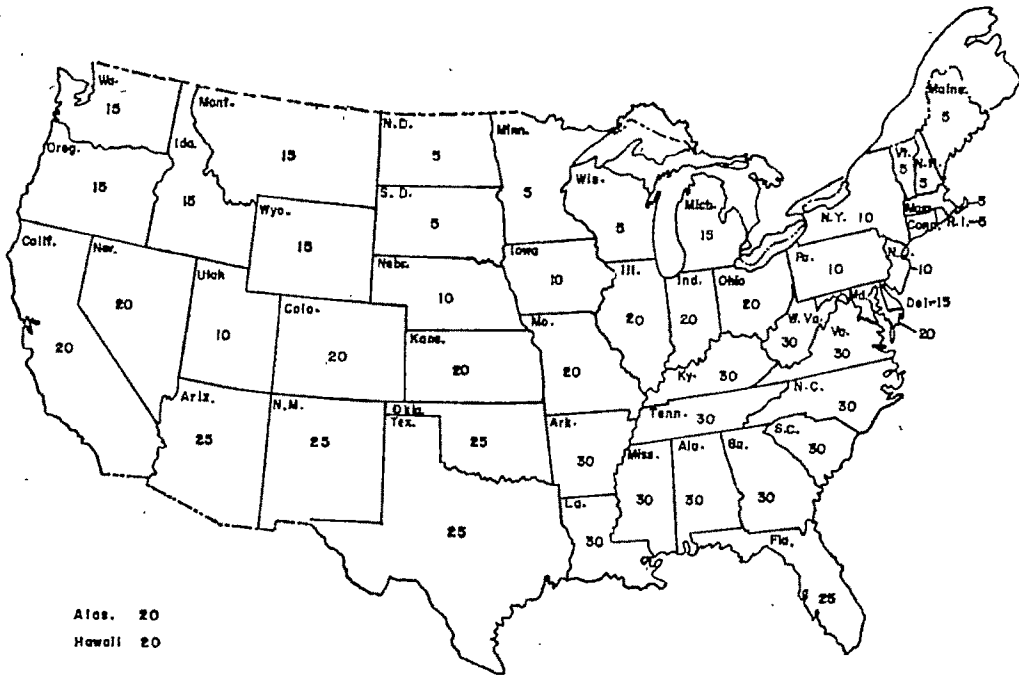


FIGURE 2. Index of Southernness

tained the results of Tables 4-7. Table 4 presents simple and multiple correlation data of the 48 continental states with homicide rates in 1960. Index of Southernness explains a great deal of the variance and more than any other variable. (With all 50 states, including Alaska and Hawaii, where our approach makes the least sense, the total variance explained drops to 87.7% and that explained by Index of Southernness to 71.3%). However, it is characteristic of this form of regression that the relative influence of the first variable entered appears to be more than it is, even if there are low intercorrelations. It will be noted below that we have made several attempts to get around this difficulty although a factor analysis was not done. The high correlation of Index of Southernness with percent Negro, low education, and low income must be noted (intercorrelations of .71, .52, and .47 respectively). As elsewhere, since the Index of Southernness and percent 20-34 are not closely correlated (intercorrelation of .39 is higher here than is general), they contribute quite separately to the explained variance. In Table 4a the independence of Index of Southernness from other factors is suggested by forcing it last in the multiple regressions. It is important to note that it is still highly

significant in the partial correlation after all other variables have been accounted for.

If we take white homicides alone, in Table 5, the correlation of Southernness and homicide is somewhat reduced, although still remarkably high and significant. Percent nonwhite has significant positive correlation with homicide, although it adds little to the variance explained. However, if we look at the partial correlation of *percent nonwhite* after Index of Southernness is entered, it falls to .07; after both Southernness and percent 20-34 have been taken out, it becomes -.37. This indicates that having nonwhites in a state certainly does not add to tendencies to white homicide, as a theory of generalized conflict might predict. Omitting percent nonwhite entirely in an excursion did not reveal any other significant variable that had been submerged by its presence. Forcing Index of Southernness last in this case (Table 5a) gives a considerably more decisive result than for all homicides. Here the Index adds in this last position 16% to the variance, and its partial correlation, after everything else is accounted for, is almost as high as the original correlation.

It seemed useful to try to investigate the degree to which Index of Southernness was "explained" by the other variables when it



Table 4. Relation of State Homicide Rates to Selected Variables,<sup>†</sup> 1960.

Variable	Expl. Var.	Partial r at Entry	Simple r
Southernness Index	74.6%	(.86)*	.86*
% Negro	82.5	.56*	.87*
% 20-34	87.3	.53*	.57*
Median Income (Adjusted)	88.4	-.29*	-.43*
% Urban	88.7	.16	-.15
Physicians/1000	89.1	-.20	-.46*
Median Yrs. School Compl.	89.2	.09	-.51*
Hospital Beds /1000	89.4	.13	-.48*
Population	89.4	-.05	.05
% in Cities over 300,000	89.4	.02	.001

<sup>†</sup>The Program (U.C.L.A. bio-medical data) is such that the variable with the highest single correlation to the dependent variable is entered first, the one with the highest remaining partial correlation next, and so on until there is no remaining significant partial.

\*Significant at .05 level or higher.

became the dependent variable. The correlation of Southernness with homicide was obviously high; so we dropped homicide out of the regression. Table 6 shows the results with percent Negro still in the regression while Table 6a omits both homicide and percent nonwhite (or Negro) from the regression; 65% of the variance in Index of Southernness is correlated with variables such as education and medical facilities, although "explanation" probably goes the other way. The high negative correlations of medical services with Southernness suggest that the contribution they make to lowered death rates after criminal assaults (as predicted) was *not* the reason for their contribution to previous regressions for homicide. It is surprising that the hospital-bed item should be in as crucial a relation to Southernness as

it seems to be. Forcing hospital beds last, physicians per 1,000 explained 22 percent of the variance in first position, but hospital beds per 1,000 still explained 12 percent of the total variance. Further speculation on what is involved is left for another study.

Some evidence of the relation of Negro homicide to Southern homicide was examined. For example, Pettigrew and Spier (1962) found that the highest positive correlations by states of Negro homicide rates were with a "homicidal culture index" of Negro offenders and with the white homicide rates. Correlation values were .55 and .50 respectively, both significant at the .01 level. The homicidal culture index was prepared on the basis of the white homicide rates of the states of origin of the Negro offenders. There were also high correlations with Negro mobility when the homicidal culture index was held constant. However, my own work and census information would suggest that this may reflect the contribution that a relatively large percentage in the 20-34 age

Table 4a. Relation of State Homicide Rates to Selected Variables, 1960; Southernness Index Forced Last.

Variable	Expl. Var.	Partial r at Entry	Simple r
% Negro	66.0%	(.81)*	.81*
% 20-34	78.0	.60*	.51*
Median Income (Adjusted)	83.0	-.48*	-.43*
Hospital Beds /1000	84.6	-.29*	-.48*
% in Cities over 300,000	85.1	.18	.001
Physicians /1000	85.4	-.15	-.46*
% Urban	85.7	.13	-.15
Pop. Size	85.75	-.06	.05
Median Yrs. School Compl.	85.75	.001	-.51*
Southernness Index	89.4	.51*	.86*

\*Significant at .05 level or higher.

Table 5. Relation of State Homicide Rates (for Whites) to Selected Variables, 1960.

Variable	Expl. Var.	Partial r at Entry	Simple r
Southernness Index	52.0%	(.72)*	.72*
% 20-34	70.0	.16*	.60*
% Nonwhite	73.6	-.37*	.38*
White Pop. Size	74.7	-.20	-.12
Physicians /1000	75.0	-.10	-.47*
% Urban	75.7	.17	.10
Median Income (Adjusted)	75.9	-.08	-.15
Hospital Beds /1000	75.98	.06	-.51*
Median Yrs. School Compl.	76.09	.07	.03
% in Cities over 300,000	76.2	-.07	-.12

\*Significant at .05 level or higher.

bracket makes to the variation, and the fact that high mobility may be related to an under-reporting of the population base, particularly of the urban Negroes.

Redfield believed that Negro homicide rates in the South were lower than white; although many more Negroes were killed by whites than vice-versa. His data showed there were fewer Negroes murdered per hundred thousand than there were whites. In the South he had to rely on a detailed compilation of newspaper reports; generally these seemed to be remarkably complete, especially in states with one state-wide newspaper. But it may be that black deaths tended to be ignored by the white newspapers to a very large extent, either out of disinterest or a desire of the blacks to keep their difficulties to themselves. If this is not so, then there has been a tremendous change since the 1870's, a change already underway by the 1920's. One explanation may be that the Negroes were so poor in the 1870's that guns and even knives in Negro possession

Table 5a. Relation of State Homicide Rates (for Whites) to Selected Variables, 1960; Southernness Index Forced Last.

Variable	Expl. Var.	Partial r at Entry	Simple r
% 20-34	35.7%	(.60)*	.60*
Hospital Beds /1000	52.3	-.51*	-.51*
Median Yrs. School Compl.	57.2	-.32*	.03
Median Income	58.5	-.18	-.15
% Urban	59.2	.13	-.13
Physicians /1000	59.5	-.09	-.47*
% in Cities over 300,000	59.8	.09	-.07
White Pop.	60.1	-.08	-.12
% Nonwhite	60.2	-.06	.38*
Southernness Index	76.2	.63*	.72*

\*Significant at .05 level or higher.

Table 6. Relation of Southernness Index to Selected Variables, 1960 (48 States).

Variable	Expl. Var.	Partial r at Entry	Simple r
% Negro	50.2%	(.71)*	.71*
Hospital Beds /1000	67.2	-.59*	-.61*
% 20-34	70.4	.30*	.39*
Median Yrs. School Compl.	74.2	-.36*	-.52*
% in Cities over 300,000	75.4	.22	.04
% Urban	78.0	-.32*	-.24
Median Income (Adjusted)	78.4	-.13	-.47*
Physicians /1000	78.5	.04	-.48*
Population	78.5	-.02	.05

\*Significant at .05 level or higher.

Table 6a. Relation of Southernness Index to Selected Variables, 1960 (50 States).

Variable	Expl. Var.	Partial r at Entry	Simple r
Hospital Beds /1000	35.7%	(-.60)	-.60*
Median Yrs. School Compl.	45.3	-.39*	-.26
% 20-34	53.8	.39*	.27
Median Income (Adjusted)	58.3	-.31*	-.45*
% in Cities over 300,000	63.2	.34*	.03
% Urban	64.6	.19	-.23
White Pop.	64.6	.03	-.02
Physicians /1000	--	(.00)	-.47*

\*Significant at .05 level or higher.

were rare (as well as sometimes unsafe and illegal). With fewer weapons, the same amount of Negro violence might have resulted in relatively fewer homicides than is true today. If we can believe the figures, neither in the twenties nor the sixties have the highest Negro rates been in the heartland of the South (Brearley, 1932:99; U.S. Public Health Service, Vital Statistics, 1966). On the other hand, Wolfgang's study of murder in Philadelphia suggested that the highest rates were among recently arrived Southerners (1958:331). However, it seems likely that differences in age and sex ratios play a part in his results.

As an excursion to the manipulation of the data correlated in Table 4, percent Negro was forced last. It was important to know if blacks could be considered as indistinguishable from other poorly educated and low-income persons of Southern background. In the stepwise regression, percent Negro in last position added 5.25 percent to the variance explained and when entered had a highly significant partial correlation of .58. In light of the small influence of percent Negro on white rates, this suggests indirectly that the Negro tendency to higher homicide rates is not simply a heritage of general, backward Southern culture. (It is likely that there are other Southern subcultures that

would appear to diverge in the same direction.) This is clearly a question which should, however, be investigated by other more adequate means.

Detailed examination of results and further statistical excursions suggest that it would be possible to create an Index of Southernness that would correspond both more closely with the homicide rates and with the historical backgrounds of the people in the several states. Examination of residuals suggests that there should be a reduction in extent of ascribed "Southernness" above the North-South regional frontier; on the other hand, Texas and Florida act like extreme Southern states. We should also examine, for historical reasons, where we might make changes in the Index of Southernness, although it would not help the correlations, e.g., New York should perhaps be treated as a New England state in terms of Southernness. However, I thought it more useful here to stay with the original fairly clean index, developed largely independently of knowledge of homicide rates.

Homicide rates in Puerto Rico (UN Demographic Yearbook, 1953) and Mexico (Table 1) suggest that Spanish speaking Americans would be likely to increase greatly homicide rates where they are in significant numbers. Perhaps this is true of the Puerto

Table 7. Relation of Homicide Rates to Selected Variables, SMSA's 250,000+, 1960.

Variable	Expl. Var.	Partial r at Entry	Simple r
Southernness Index	46.6%	(.68)*	.68*
% Nonwhite	55.0	-.40*	-.64*
Pop. Size	58.6	-.28	-.14
Median Income (Adjusted)	60.2	.20	.39*
Physicians /1000	61.9	.21	.068
% 20-34	62.2	.082	.031
Hospital Beds /1000	62.26	.048	.023
Median Yrs. School Compl.	62.31	.037	.087

\*Significant at .05 level or higher.

Ricans in New York, but a detailed look at states and localities in the Southwest with large Mexican-American populations suggests behavior rather similar to that of the Southern whites in their area. Nor does a look at other minorities such as the Indian suggest either direct or indirect influence.

I next applied the approach to metropolitan areas and cities in terms of what was known of historical backgrounds (most cities and SMSA's were considered to have the same Index of Southernness as their states). If we look at homicide rates for SMSA's for 1960 (Table 7), the importance of Southernness declines somewhat while that for percent nonwhite rises. Moving to cities (Table 8), one notes the considerably lessened influence of Index of Southernness, although it is still significant at the .001 level. After correcting for percent 20-34, the basic correlation rises to .58. There is not a significant correlation of 20-34 and Index of Southernness, but the intercorrelations of Southernness with percent white and income (inverted) are .53 and .46 respectively, accounting particularly for the lowered influence of the nonwhite variable. My application of the index to cities was least firm, and the variations among cities in percent 20-34 were much greater than for states.

Table 9 resulted from an attempt to check

Table 8. Relation of Homicide Rates to Selected Variables, 130 Largest Cities, 1960.

Variable	Expl. Var.	Partial r at Entry	Simple r
% not 20-34	50.3%	(-.71)*	.71*
Southernness Index	67.1	-.58*	.48*
Median Yrs. School Compl.	72.9	-.42*	.17
% White	74.2	.22*	-.50*
Median Income (Adjusted)	75.3	.19*	.28*
Physicians /1000	76.3	.21*	.34*
Population	76.4	.08	.05
Hospital Beds /1000	76.4	.01	.33*

\*Significant at .05 level or higher.

Table 9. Relation of State Homicide Rates to Selected Variables, 1959-60 (Averaged).

Variable	Expl. Var.	Partial r at Entry	Simple r
Southernness Index	77.5%	(.88)*	.88*
% Nonwhite	85.5	.60*	.85*
% 21-44	86.4	-.25	.18
% Large City	87.9	-.33	.067
Murder-Punishment Ratio	88.5	.23	.25
% Urban	88.7	-.18	-.21

\*Significant at .05 level or higher.

on the hypothesis that the likelihood of being punished for a murder influenced the propensity to commit murder in a society. In spite of the many doubts that have been raised by criminologists to this common sense belief, it appeared to me that Redfield may have been right as to the genesis of the Southern pattern, and it was possible that there was still an operating differential today. Since so few people have been executed in the last few years for murder, I assumed that certainty of punishment had more of a deterrent effect than severity, and I developed a measure for each state that summed numbers imprisoned for homicide and numbers executed, and then compared this to the number of murders. The resulting "murder-punishment ratio" was then compared to the murder rates by states. In this instance I averaged murder rates for 1959-1960 as the dependent variable and compared a number of other factors with it as in the foregoing tables. The addition of the murder-punishment ratio to the explanation is not very large, with significance at about the .10 level (partial correlation in fourth position is almost the same as the simple correlation). Using largely the same material independently, Gibbs (1968) found about the same level of relationship. Using a more complicated approach correlating severity and certainty of punishment, Tittle (1969) makes a more convincing case for deterrence. Together these studies hardly present overwhelming proof, but they do

indicate that Redfield might have been right about one aspect of the genesis of the pattern. In addition, Table 9 offers further corroborative evidence of the central thesis of the relationship of Southern regional culture to homicide.

#### CONCLUSION

It seems established that there is a significant relationship between murder rates and residence in the South. This relationship can be traced back to differences between the North and South that were already observable before the Civil War. It can be shown that outside of the centers of Southern and Northern society, state homicide rates grade into one another in rough approximation to the extent to which Southerners have moved into mixed states. While differences in standard demographic or economic variables such as age composition, median education, or median income account for a good deal of the variance among sections of the country in murder rates, there is a significant remainder that may be related to "Southernness" alone. There seems, then, on both qualitative and quantitative grounds to be evidence that the culture that developed in the Southern states in past centuries leads to high murder rates. International and state comparisons seem to indicate that this specifically Southern culture accounts for most of the difference between the murder rates of the United States and those of comparable countries such as Canada and Australia. A description of the traits accounting for violence in the several Southern subcultures, of their rise and fall, of the diffusion of these traits outside of the South, of present trends in their importance, and of the cause of alternative subcultural traits accounting for high homicide rates unrelated to Southern culture—all these are subjects for further study.

#### APPENDIX

The basic rules for constructing the Index of Southernness were: (1) Give scores of 30 to Virginia, West Virginia, Kentucky, Tennessee, North Carolina, South Carolina, Georgia, Alabama, Mississippi, Louisiana, and Arkansas. If we define Southern culture in Redfield's terms to include the culture of both lowland and mountain South in the 19th century and to

grade both South to North in the Central Middle West, then the elite and the general population of these states have up to now been under overwhelming Southern influence. (2) Give scores of 5 to states with only indirect Southern influence and virtually no white Southern population (New England and most Upper North Central states). (3) Give scores of 20 to states with about half of the population of Southern background and a Southern majority at time of first settlement—South being defined in terms of (1) above. This represents states felt to be half or slightly more than half of Southern background. (4) Give scores of 25 to states with overwhelming Southern background and a white population primarily from the South, but with strong non-Southern minorities that have preserved some independence from this heritage by virtue of a separate existence or recent movement to the area. (5) Give 15 to definitely non-Southern states with a strong representation of Southern population in either the formative period or more recently. (6) Give 10 to states overwhelmingly non-Southern, but with a weak Southern representation in the population or a Southern cultural influence in the formative period. It should be noted that a purely Southern state would score 35; and a purely Northern state, 0; but these are empty categories.

Such "typical American states" as Kansas, Colorado and California received scores of 20. West of the Plains Southern influence is rather evenly spread because of the original dispersion, the later dispersion of Texas culture, and the recent dispersion to Western cities from West South Central states. In Ohio, Indiana, and Illinois a detailed map would show a gradation from 5 in some Northern fringe counties to 30 in some Southern counties, but these are averaged out in state index values. With urbanization many Southerners have crossed over the graded area to Detroit but not to Milwaukee. This difference in recent migration does show in the figures. Idaho represents a combination of counties that should be scored 20 in its northern and central areas and counties that should be scored 10 in the southwest.

Many of the index scores could be questioned. However, it would be difficult to reverse the relationship of a state to its neighbors or change greatly the extremes. The Index can be confirmed or refined by looking at both Bureau of the Census material in successive censuses on state of birth and at qualitative studies of the antecedents of elites, as well as studies of current cultural foci on elite and popular levels. Religious data give fairly detailed breakdowns in percentage terms that

enable us to draw cultural lines along the county lines where they are ultimately most appropriate. Linguistic data can be very useful in some circumstances. An excursion with a quite different Index of Southernness (generally reducing the "Southernness" of border and Western states) explained less of the variance (27%) using the Table 5 variables, but Index of Southernness still entered first and had the highest F value at the completion of the analysis (18.2).

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## THE CAUSES OF RACIAL DISTURBANCES: TESTS OF AN EXPLANATION \*

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*American Sociological Review* 1971, Vol. 36 (June):427-442

*The adequacy of a recently proposed explanation for the location of racial disorders during the 1960's is evaluated in this paper. Two approaches to evaluation are used: (1) The proportion of variation accounted for by the variables assumed to be related to the occurrence of disorders is compared with an estimate of the "maximum explainable proportion of variation," and (2) the structural equation derived from an analysis of the 1961-67 disorders is used to predict the locations of the 1968 disturbances. The conclusions from these investigations support the proposed explanation only with respect to the non-South, but indicate that the distribution of disorders among southern cities has been converging during the late 1960's to the pattern which has been prevalent in the non-South throughout this decade. This finding is interpreted as evidence of the decreasing importance of regional cultures as an intervening factor in the development of black solidarity.*

### I. INTRODUCTION

IN a recent analysis of racial disturbances which occurred during the 1960's (Spilerman, 1970b), it was argued that the distribution of disorders among cities is not consistent with an explanation which locates the causes of rioting in community conditions. Communities do differ in the rate at which they have incurred racial violence during this period, yet the conclusion from the analysis was that these differences cannot be accounted for by any of the most common explanations which attribute dis-

order-proneness to aspects of community organization.<sup>1</sup>

Using data on the demographic and organizational characteristics of all 673 cities in the contiguous United States with populations exceeding 25,000 in 1960, in conjunction with information on the location of racial disorders during 1961-68,<sup>2</sup> we ex-

<sup>1</sup> Studies which have sought to attribute the location of racial turmoil during the 1960's to political and economic characteristics of communities include Downes (1968), White (1968), and Maloney (n.d.).

<sup>2</sup> Political and economic characteristics of the communities were obtained from the Alford-Aiken data file at Wisconsin. Sources of the disorder data were: Lemberg Center's *Riot Data Review* (1968), Congressional Quarterly's *Civil Disorder Chronology* (1967), and *The New York Times Index*. In order to reduce heterogeneity in the type of disturbance (which may reflect a particular set of underlying conditions), only instances of "spontaneous outbreaks" which were characterized by primarily Negro aggression were included in the analysis. This was the most common type of racial disorder in the 1960's, and the most severe

\*The research reported here was supported by funds granted to the Institute for Research on Poverty at the University of Wisconsin by the Office of Economic Opportunity, pursuant to provisions of the Economic Opportunity Act of 1964. I wish to thank David Dickens for his assistance with the statistical computations; I. Richard Savage for comments on an earlier draft; and Robert Alford and Michael Aiken for permission to use their data file on characteristics of cities. The conclusions are the sole responsibility of the author.

amined a number of explanations. Specifically, it was considered whether disorders are more likely events in communities characterized by high levels of social disorganization; whether rioting tends to occur where absolute deprivation is high and, hence, can be explained in terms of the material conditions of Negro life; alternatively, whether relative deprivation appears to produce the most intense frustration with the result that disturbances are likely where Negroes fare poorly in comparison to white residents; whether rioting may have its genesis in the gap between expectations and reality; or whether the turmoil of this period may be construed as a response of frustrated individuals when the institutionalized political channels for securing redress and articulating group interests are unresponsive. Several objective measures relating to each thesis were examined in order to ascertain the respective abilities of these alternative explanations to account for the distribution of the disorders. However, all were found to be inadequate.

Only two variables—the numerical size of the Negro population and a dummy term for region—proved to be related in a substantial manner to the location of the disorders. Together, these variables explain 46.8% of the variation in the dependent variable, the number of disorders in a city during the eight-year interval. The addition of 16 community characteristics, selected as indicators of the above explanations, to a regression equation containing the two variables accounted for only 4.5 additional percentage points of the total variation.

This result was attributed to the emergence, in recent years, of a number of factors which operate to promote geographic uniformity in the impact of stimuli that are frustrating to Negroes. (1) The activities of the federal government on behalf of Negro interests during the preceding decades are salient to the residents of every ghetto and may have sensitized them to this level of government. As a consequence, federal actions which are vacillatory in character or

otherwise insensitive to Negro concerns would be frustration provoking to Negroes irrespective of where they reside in the country. (2) The development of racial solidarity among Negroes—the recent growth of black consciousness and identity—has served to make the fate of Negroes in distant cities a matter of concern in all ghettos. (3) In addition to reinforcing the above processes through the provision of information relevant to these matters, the medium of television induces geographic uniformity by virtue of its own organizational structure. Through network news programming, the more graphic instances of discrimination and maltreatment of Negroes are transmitted to all sections of the country.

If, as a result of these considerations, a Negro individual is in fact apt to participate in a disorder irrespective of where he resides, then the location of instances of racial turmoil should reflect the numerical size of the Negro population in different communities (Spilerman 1970b:643).<sup>3</sup> Moreover, since individuals would be responding primarily to frustrations generated outside their own communities, *no other community characteristic should be related to the location of the disorders*. Thus, although local conditions undoubtedly vary for Negroes, the results of the earlier study suggest that during the 1960's they have responded instead to frustrations which are felt by black persons in all communities, such as the slight attending the defeat of a rat control bill in Congress or the sight of Negro demonstrators being mistreated elsewhere.

The significance of the second variable, a dummy term for South, was attributed to the different traditions in race relations in the South and non-South. In the former region, past experiences of Negroes probably operate to reduce their expectations regarding the likelihood of rapid improvement in racial or economic matters, while the remembrance of past repression may lower the rate at which they permit their frustration to

disturbances were of this character. For additional detail on the coding of disorders, see Spilerman (1970b). These same materials are also used in the present investigation.

<sup>3</sup> The numerical size variable has frequently been used as an indicator of organizational complexity (cf. Blau and Scott, 1962:227; Blau, 1970; Aiken and Alford, 1970). In the present conceptualization, however, it is fundamentally a measure of the availability of participants.



be translated into hostile outbursts. However, there is evidence that, apart from a constant term, the relationship between number of disorders and Negro population size is quite similar in the two regions. The partial correlation between these variables controlling for region (.668) is greater than the zero-order correlation between them (.586), suggesting that a covariance model (which formally assumes parallel lines) improves the relationship.

The above results derive from an analysis of the components of variance that are explained by different clusters of variables, which were selected to provide a comparison among the alternative explanations. The intent in the present study is to consider several related topics which were not raised in the prior report as they require a fundamentally different methodological perspective. In broad terms, our interest here will be to ascertain how satisfactory is the explanation which was proposed in the earlier analysis. This general concern will be pursued in the context of examining the following specific topics:

(1) There is the problem of specifying the *form* of the relation between Negro population size and the number of disorders in a city. This is necessary in order to measure the disorder-proneness of particular communities and ascertain how this value is likely to respond to a change in Negro population. More importantly, considering the intent of the paper, this specification is required for topics (2) and (3).

(2) If the explanation which was proposed in the introductory paragraphs is indeed correct, and Negro population size and a contextual variable for region are the sole community characteristics which are related to the location of disorders, there arises the question of accounting for the *unexplained* variation (which, in the previous analysis, amounted to 53.2% of the total variation). Does the magnitude of this value not mean that important variables have been neglected? This question is investigated in Section III by means of a simulation study.

(3) There is the problem of using these results to predict the locations of future disorders. The prediction of racial disorders has received some preliminary attention

(Maloney, n.d.), but a procedure which derives from empirically grounded theory is yet to be devised. However, if Negro population size and region are in fact the only variables necessitating consideration (and assuming that the causes of racial violence do not undergo essential change), an efficient predictive scheme can be constructed. This topic is addressed in Section IV. Considered together, the results from these analyses will permit an assessment to be made regarding the adequacy of the hypothesized explanation for the location of racial disorders in the 1960's.

## II. COMMUNITY DISORDER-PRONENESS AND NEGRO POPULATION SIZE

In the previous analysis (Spilerman 1970b) we were primarily concerned with ascertaining the relative importance of a number of community characteristics in accounting for the distribution of disorders among cities. Because of the large number of variables that were considered, only the possibility of linear relationships—the simplest of interdependencies—was investigated.<sup>4</sup> Now, having demonstrated the importance of Negro population size for community disorder-proneness (and the apparent insignificance of other community characteristics), a more detailed investigation into the shape of the relation is undertaken.

There are compelling reasons for expecting a distinctly non-linear form to characterize the dependence of disorder-proneness upon Negro population size. At the lower end of the Negro population continuum, given the Negro protest character to racial turmoil during the 1960's, the human resources necessary to produce a racial incident of sufficient magnitude to qualify as a disorder would seem to be lacking. Consequently, one should notice a threshold effect in the data, a critical Negro population size below which racial disorders fail to develop.

At the upper end of the Negro population

<sup>4</sup> Transformations were used but for the purpose of linearizing particular relationships for statistical analysis. The emphasis was not on the substantive meaning of the functional form.

continuum, other considerations suggest that a ceiling effect may operate to limit the number of disorders that can occur in a community in a fixed time interval. In part, a ceiling effect would be an artifact of definitional difficulties with the concept of a disorder. When several disturbances occur in a city within a short time period, they may be recorded as a single incident of long duration or high severity.<sup>5</sup> Aside from this definitional problem, however, there are substantive considerations which argue for the presence of a ceiling effect.

First, in the days subsequent to a disturbance, the police and other agencies of social control are probably sensitized to the possibility of further violence. As a result, they may avoid actions which antagonize ghetto residents and, at the same time, remain prepared to respond in force to incipient riot situations. "Keeping the lid on" in this way is one activity which would serve to limit the number of outbreaks that can occur in a city in a specified time interval.

A second reason for expecting fewer disorders in large ghettos than a linear relation with Negro population size would predict derives from the conceptual model presented earlier, namely, the thesis that Negroes were responding primarily to stimuli which are geographically diffuse in their impact. On first impression this might appear as support for the existence of a linear relation between Negro population size and racial turmoil; however, introduction of the additional consideration of disorder *severity* alters that inference. There is some evidence that severe disturbances, which involve many individuals, have occurred to a disproportionate extent in the larger ghettos (Spilerman 1970a). Assuming the effect of geogra-

phic uniformity to mean that an individual is equally apt to participate in a disorder irrespective of where he resides (so long as the city exceeds the critical threshold in Negro population), in communities with large Negro populations he would be more likely to discharge this proclivity in severe disorders involving many participants. These communities, in turn, would witness fewer disturbances than expected from a linear relation with Negro population size, though of a more serious nature. Consequently, at the upper end of the Negro population scale, either because of a saturation effect to the number of distinct disorders which a city can sustain<sup>6</sup> or because disorders in large ghettos tend to encompass many individuals, the marginal increase in community disorder-proneness, per additional Negro individual, should decrease.

Support for these contentions is found in Table 1. Row 4 of this table shows the average number of disorders which occurred in a city during 1961-68, with the cities categorized by Negro population size. As the linear correlation reported in Section I between number of disorders and Negro population (.586) suggests, disorder-proneness exhibits a consistent tendency to increase with this variable, varying from zero disturbances per city for communities with fewer than 1,000 Negroes, to an average of 4.95 disorders per city for the 19 communities with more than 100,000 Negroes. Row 5 presents the racial disturbance rate per 1,000 Negroes in a category. These values were computed by dividing the total number of disorders in a category (Row 1) by the aggregate Negro population for the cities (Row 3). The results provide evidence both for the necessity of a minimum critical size before racial incidents of sufficient magnitude to be labeled as disorders can occur, and for the contention that the disorder rate is constrained by a ceiling effect. Disorders

<sup>5</sup> Although the riots in Watts, Detroit, and Newark each lasted for several days, they are recorded in most compilations as single events. Other disturbances, which consisted of a sequence of incidents over more than one day, are sometimes listed as multiple disorders. For coding purposes, it was arbitrarily decided that reports of multiple disturbances in a city which are separated by less than five days would be recorded as a single event. The only other consideration given to disorder severity was the requirement of a minimum level of turmoil for inclusion in the study. An incident had to involve at least 30 individuals and result in some violence or destruction to be classified as a disorder.

<sup>6</sup> Since the disturbances were compiled primarily from news accounts, there is also a possibility that the incremental news value of an additional disturbance in a city decreases as a function of the number of disturbances which already occurred. It is unlikely, however, that this consideration can account for a saturation effect since even the most disorder-prone cities (Chicago, New York) have averaged fewer than three reported disorders per year over the eight-year interval.

Table 1. Disorder Data for Cities with 25,000 or More Inhabitants, by Negro Population Size, 1961-1968.

	Negro Population of City (in thousands)							
	0.00- 1.00	1.00- 2.50	2.50- 5.00	5-15	15-25	25-50	50- 100	100+
1) Number of Disorders <sup>a</sup>	0	11	31	85	37	49	34	94
2) Number of Cities	261	93	80	118	44	37	21	19
3) Aggregate Negro Population (1,000's) <sup>b</sup>	92	151	300	1,076	846	1,289	1,490	6,212
4) Disorders per City	0	.118	.388	.720	.841	1.324	1.619	4.947
5) Disorders per 1,000 Negroes	0	.073	.103	.079	.044	.038	.023	.015

<sup>a</sup>Sources of the disorder data are *The New York Times Index*, *Congressional Quarterly's Civil Disorder Chronology* (1967), and the *Lemberg Center's Riot Data Review* (1968).

<sup>b</sup>1960 Census of Population.

per individual increase with Negro population size until the category 2,500-5,000 and thereafter decrease.

The preceding discussion, together with the empirical evidence from Table 1, suggests that the relationship between number of disorders in a city (D) and Negro population size (N) should be S-shaped. With a curve of this form, the rate of increase in disorder-proneness as a function of Negro population would be low among cities with small Negro populations, high among cities with intermediate size Negro populations, and low again among large ghettos. The most common S-shaped curve is the logistic (Lotka, 1956:68-70; Coleman, 1964:43); however, the properties of this curve are too restrictive for use with the disorder data.<sup>7</sup> A more general S-shaped function,

$$D = e^{[a - \frac{\beta}{N^2}(e^{-\gamma N}) - \frac{\delta}{N}(1 - e^{-\gamma N})]} \quad (1)$$

where  $\alpha$ ,  $\beta$ ,  $\gamma$  and  $\delta$  are parameters, was therefore constructed. Since the S-form is not compatible with the notion of an addi-

tive constant term for region, this function was fit separately to the disorder data from each region. The resulting curves are presented in Figure 1.<sup>8</sup>

To measure the explanatory power of the Negro population variable, we calculated the proportion of variation in number of disorders accounted for by these curves. In the non-South, 73% of the variation is explained by the nonlinear regression; among Southern cities 34% of the variation can be attributed to Negro population size. The curves also indicate that the specification of a covariance model, which was used in the previous study, provided a reasonable approximation to the true relationship. Beyond a Negro population equal to 10,000, the curves are nearly parallel. What the covariance model obscured, however, is the very different  $R^2$  values for the two regions.

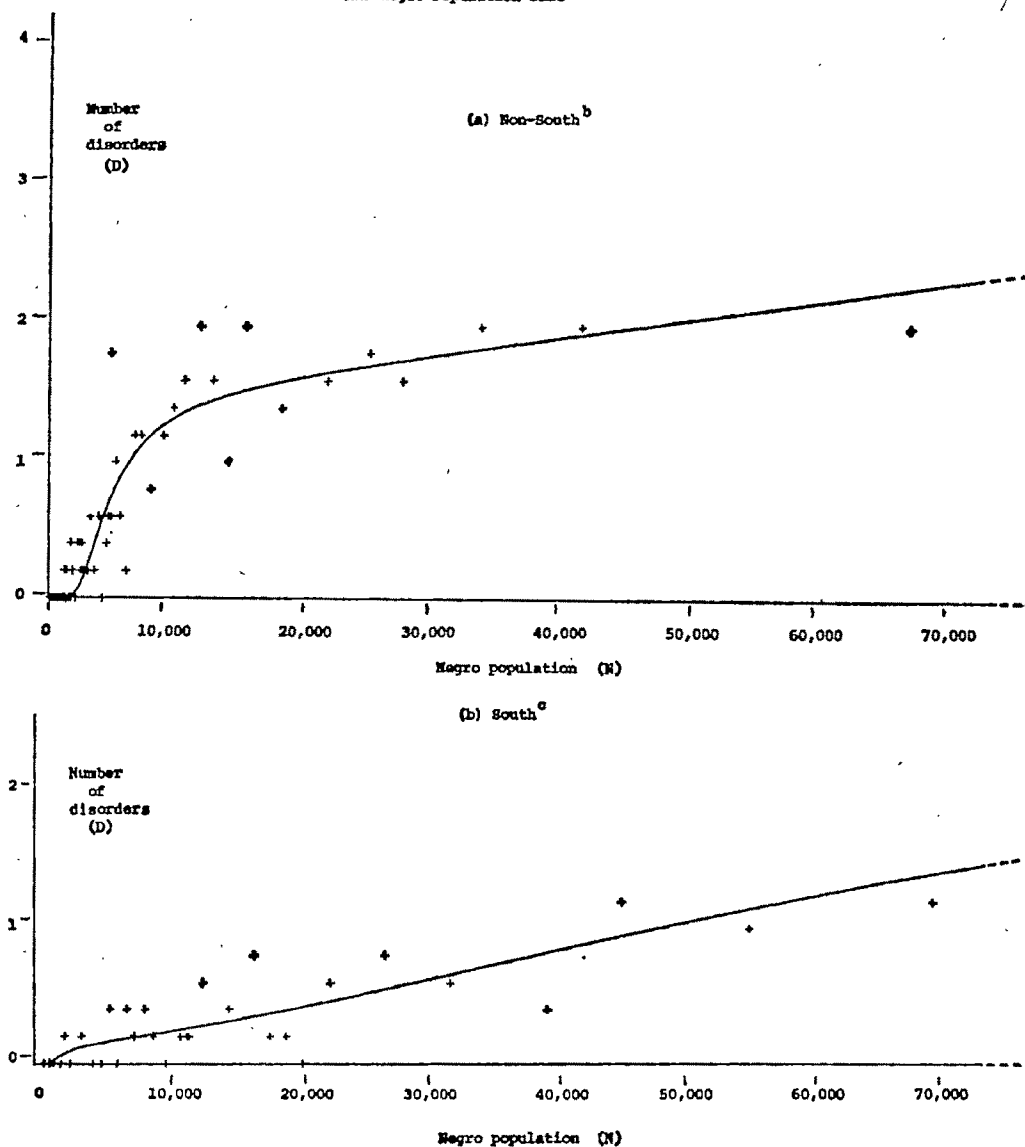
According to these curves, community disorder-proneness increases most rapidly with Negro population size for cities with approximately 3,500 Negroes in the non-South,<sup>9</sup>

<sup>8</sup> A nonlinear regression program (*Gaushaus*) was used to minimize the residual sum of squares by the method of steepest descent. For details on the program, consult the University of Wisconsin Computing Center's *Users' Manual* (1966). Note, incidentally, that while the curve is S-shaped over the range of Negro population values which characterize American cities, it does not have this form over the entire real line.

<sup>9</sup> The rate of increase of a curve at a particular point can be determined from its first derivative.

<sup>7</sup> The logistic curve requires the inflection point to be midway between zero disorders and the upper asymptote (the predicted maximum number of disorders). Furthermore, the two limbs of the curve are constrained to be symmetric about the inflection point. Since there is no reason to expect the disorder data to satisfy these restrictions, a more flexible S-curve was constructed.

FIGURE 1. Relationship Between Racial Disturbances During 1961-68  
And Negro Population Size<sup>a</sup>



a. Each point represents five cities that are adjacent in Negro population size. The group value for Negro population is the anti-log of the mean of the logged figures. The group value for number of disorders is the arithmetic mean. (Grouping is for presentation purposes only; all analyses were performed on the individual city values.)

b. Parameter values for the non-South are:  $\alpha = 2.649$ ,  $\beta = 1.708 \times 10^7$ ,  $\delta = 3.271 \times 10^5$ ,  $\gamma = 6.95 \times 10^{-6}$

c. Parameter values for the South are:  $\alpha = 1.270$ ,  $\beta = 2.103 \times 10^6$ ,  $\delta = 6.615 \times 10^4$ ,  $\gamma = 5.23 \times 10^{-5}$

Evaluating the derivative of equation (1) for the non-South at  $N=3,500$  and at  $N=100,000$  yields the following result: Disorder-proneness changed by .310 expected disorders per city (for the eight-year interval) with an increase of 1,000 Negroes at the first population value, and by .014 disorders per city with an identical sized increase in Negro population at the second value.

and 1,000 Negroes in the South. No special significance should be attributed to these precise values, however, since by fitting different S-shaped functions minor variations will result in the points of maximum increase. What is important, and is particularly evident from the plotted points for

the non-South, is the S-shaped character of the relation.

### III. WHAT ABOUT THE UNEXPLAINED VARIATION?

The  $R^2$  values associated with the S-curves indicate that the single variable, Negro population size, explains 73% of the variation in number of disorders in the non-South, and 34% in the South. These are substantial proportions; nevertheless, there is still the question of accounting for the residual variation. Is it the case that by considering additional community characteristics the proportion of explained variation can be increased? Or, alternatively, is it possible that the total proportion of variation which can be *theoretically* explained is actually less than one?

The commonly employed measure of the inadequacy of an explanation,  $1 - R^2$ , is predicated upon an underlying explanatory model which is fundamentally deterministic. The concept of randomness is employed in that formulation to compensate for errors resulting from inadequate measurement and neglected effects. Presumably, by improving data quality and incorporating additional variables into an explanation, the proportion of explained variation could be increased and, theoretically, made to approach the value "one."

In contrast to that view of randomness as disturbance, a process may be conceptualized as inherently stochastic, and this type of explanatory model is consistent with there being a complete explanation (in the sense that all relevant knowledge has been incorporated) in which the  $R^2$  value is less than one. As an illustration of inherent randomness, Ernest Nagel (1961:332) writes, many physicists "maintain that sub-atomic processes . . . are characterized by absolute chance, so that, for example, the emission of particles by radioactive substances is regarded as 'a process due to the spontaneous decomposition of its atoms' (Plank, 1936: 52)." In this view, knowing the mass of a radioactive substance is tantamount to having complete knowledge about the number of disintegrations which will occur in a specified time interval, since no other variables are relevant to the process of radio-

active decay. Nevertheless, if, after the fact, the number of disintegrations is regressed against mass (the observations being different sized pieces of the same radioactive substance), the resulting value of  $R^2$  will be less than one. It is neither possible to predict the exact number of disintegrations which will occur, nor which particles will decay, although the process is completely specified by the mass variable.

The phenomenon of hostile outbursts has frequently been cast in an analogous framework (Smelser, 1963; Lieberman and Silverman, 1965; Spilerman, 1970b). In this view, the structural conditions which create a conducive context for racial disorder are to be distinguished from the precipitating incidents. The structural conditions are amenable to analysis in terms of community level variables; for example, disorder-proneness may be consequent upon economic structure, political organization, or demographic characteristics. However, disorder-proneness is a measure of the *expected number* of outbreaks in a time interval. The actual number is dependent upon the occurrence of precipitating incidents, which are conceived of as random events. Assuming this conceptualization of disorders, the  $1 - R^2$  values reported for the regions would overestimate the true proportions of residual variation which can be explained by the inclusion of additional community characteristics. Just what values of  $R^2$  are consistent with a *complete* explanation of disorder outbreaks is the subject of the present section.

We approach this question by means of a simulation methodology. This strategy requires that we first ascertain what  $R^2$  value is associated with a complete explanation of the location of disorders, *assuming that disorder-proneness is a function of Negro population only*. Afterwards, the proportion of variation explained by Negro population size in the actual disorder data will be compared with the estimates of the maximum attainable values for the regions.

For the purpose of simulation, it was therefore assumed that the disorder-proneness value for a city ( $\lambda_i$ ) depends only upon its Negro population size and, moreover, is specified in terms of that variable by equation (1). Using this formula, an expected number of disturbances for the eight-year

period was calculated for each city.<sup>10</sup> A hypothetical distribution of disturbances was then generated from these expected values under the assumption that hostile outbursts follow a Poisson process with parameter value  $\lambda_i$  for city  $i$ .<sup>11</sup> Considering the manner by which this hypothetical distribution was constructed, it is evident that only Negro population size can be related to the number of disorders in a city since no other variable was involved in the simulation. However, because the individual outbreaks are random events, the proportion of variation in this hypothetical distribution which is explained by Negro population size will necessarily be less than one.

To ascertain what value of  $R^2$  is consistent with an explanation in which Negro population size is the sole determinant of disorder-proneness, the above simulation was carried out ten times for non-Southern and Southern cities. For each of the resulting distributions, the number of disorders assigned to a city was regressed against Negro population size, with the S-shaped function (Equation 1) fit by means of the non-linear estimation routine. Consequently, the  $R^2$  values associated with these regressions provide independent estimates of the maximum proportion of variation which can be explained by Negro population size when it

is assumed that this variable alone determines community disorder-proneness, and that precipitating incidents are random events.

The mean and variance of the  $R^2$  values generated by the simulation procedure are presented in Columns 2 and 3 of Table 2. Referring to the mean figures, we conclude that, on the average, the maximum proportion of variation which can be explained by Negro population size is .75 in the non-South and .48 in the South, *even though all information about the generative process is contained in that variable*. As a percentage of these estimates of the maximum explainable proportions, 98% of the variation in the actual disorder data is accounted for by Negro population size in the non-South, and 72% in the South (Column 4). Consequently, this single variable seems to produce an even more satisfactory explanation of the disorder distribution than was apparent from the unadjusted  $R^2$  values (Column 1). The value for the non-South is consistent with the hypothesized explanation being complete for this region. For the South, however, although the  $R^2$  value is improved, the operation of additional factors is suggested.<sup>12</sup>

To pursue these questions from a different perspective, we considered the explanatory contribution from 16 additional community characteristics. These variables have been justified elsewhere (Spilerman, 1970b) as being indicators of social disorganization, absolute deprivation of Negroes, relative deprivation, and lack of political responsiveness by the municipality.<sup>13</sup> Since

<sup>10</sup> Under the assumption that disorders are random (Poisson) events, with disorder-proneness value for city  $i$  during 1961-68 equal to  $\lambda_i$ , the expected number of disorders for this period would also equal  $\lambda_i$  (see Feller, 1957:209). Thus, the  $\lambda$ -values are the  $y$ -coordinates for the cities from the curves presented in Figure (1).

<sup>11</sup> The simulation was performed for each city in the following manner: Using the Poisson formula,  $P_k = \frac{\lambda^k e^{-\lambda}}{k!}$  with the appropriate  $\lambda$ -value for the city, the probability of  $k=1, 2, \dots, 20$  events was calculated. These values were combined to provide a cumulative probability distribution:

$$f_0 = P_0; \quad f_1 = f_0 + P_1; \quad f_2 = f_1 + P_2; \dots; \\ f_{19} = f_{18} + P_{19}; \quad f_{20} = 1.$$

The resulting sequence is therefore nondecreasing, and  $f_k$  measures the probability of  $k$  or fewer events. A random number  $r$ , rectangular between zero and one, was then generated by the computer program, and a hypothetical number of disorders equal to  $j$  was assigned to the city, where  $j$  satisfies the relation  $f_{j-1} < r \leq f_j$ . This procedure was repeated for each of the 673 cities with the  $\lambda$  value determined by the S-curve for the appropriate region.

<sup>12</sup> These conclusions must be cautiously stated. First, they are based upon the *mean* of the  $R^2$  values from the simulation. As indicated by the standard deviations in Column 3, quite different individual values of  $R^2$  can result from the same assumptions. Second, the calculations for the non-South indicate only that the thesis concerning the lack of importance of other community characteristics has not been disconfirmed, not that it is correct.

<sup>13</sup> The following variables were used. Indicators of social disorganization: percent change in total population; percent change in nonwhite population; percent of housing units dilapidated in 1950 (1950 Census of Population). Indicators of absolute deprivation: percent of nonwhite males employed in traditionally Negro occupations (service workers, household workers, laborers); nonwhite male unemployment rate; nonwhite median family income;

Table 2. Observed and Simulated Squared Correlation Coefficient ( $R^2$ ), by Region.<sup>a</sup>

Region	(1) Observed $R^2$ value <sup>b</sup>	(2) Mean of $R^2$ 's from simulation <sup>b</sup>	(3) Standard deviation of simulated $R^2$ 's <sup>b</sup>	(4) (1)/(2)	N <sup>c</sup>
Non-South	73.0	74.7	4.1	.98	501
South	34.4	47.8	11.1	.72	172

<sup>a</sup>Ten simulation runs were made for each region. See text for description of the procedure.

<sup>b</sup>Entry is multiplied by 100.

<sup>c</sup>All cities in the contiguous U.S. with total population greater than 25,000 in 1960.

1960 census data on Negro population characteristics are not available for cities with fewer than 1,000 Negroes, the present analysis was limited to the 413 communities (from among the original 673) with Negro populations in excess of this figure. Also, since non-linear estimation is cumbersome with several independent variables, a linear approximation to the S-shaped curve, in the form of an additive function in  $\log N$  and  $(\log N)^2$ , was used. This approximating function reproduces the shape of the S-curve (Figure 1), except for the lower bend. However, because of the required deletion of communities with small Negro populations, the discrepancy between the curves was expected to be negligible.

Columns 1 and 2 of Table 3 present results from fitting the S-curve and the polynomial in  $\log N$  to disorder data from the 413 communities. In Column 1, the  $R^2$

nonwhite median education. Indicators of relative deprivation: percent of nonwhite males employed in traditionally Negro occupations divided by white figure; nonwhite median family income divided by white income; nonwhite unemployment rate divided by white rate; nonwhite median education divided by white education; percent nonwhite ( $\sqrt{x}$ ). Indicators of political responsiveness: population per councilman; percent of city council elected at large; presence of nonpartisan elections; presence of mayor-council government (all variables in this cluster are from *The Municipal Yearbook*, 1967).

Percent nonwhite was included with the indicators of relative deprivation because an alternative explanation, which emphasizes interracial competition, can also be associated with this cluster of variables. See Spilerman (1970b:641).

values are shown for the S-curve, which was estimated by the non-linear method; while in Column 2 the corresponding values for the approximating polynomial,<sup>14</sup> estimated by least squares, are presented. It is evident from these entries that the polynomial in  $\log N$  provides a reasonable approximation in each region. Column 3 contains the  $R^2$  values from a regression equation in which the 16 community characteristics have been added to the  $\log N$  terms. Inclusion of these 16 variables is seen to explain only 3.9 additional percentage points of the variation in the non-South, but 9.2 percentage points in the South. Consequently, the results in Table 3 present further evidence for the contention that different processes were operating in the two regions. The thesis that Negro population size is the sole community characteristic relevant to explaining the number of disorders in a city again appears to be supported only with respect to the non-South. Negro population size is also a variable of immense importance in the South, but it apparently is not the only determinant of disorder-proneness.

<sup>14</sup>In the previous study (Spilerman 1970b), the dependent variable was transformed according to  $\sqrt{D'+\frac{1}{2}}$ , where  $D'=D$  (the number of disorders in a city) for  $D \leq 5$ , and  $D'=5$  for  $D > 5$ . The square root transform is recommended for a Poisson variate to stabilize its variance (Goulden, 1952: 98). In this study, since the shape of the curve is being emphasized, the untransformed variable  $D$  is used. With respect to the significance of independent variables, the results with the two versions of the dependent variable are identical.

Table 3. Percent of Variance in Number of Disorders Explained by Negro Population Size and by Subsequent Addition of 16 Community Characteristics, by Region.

	$R^2$ (x 100)			(4) (3)-(2)
	(1) S-shaped curve	(2) $D = a + b_1 \log N$ $+ b_2 (\log N)^2$	(3) $D = a + b_1 \log N$ $+ b_2 (\log N)^2$ $+ 16$ Community Characteristics <sup>a</sup>	
Non-South	68.1	64.5	68.4	3.9 (258)
South	32.7	31.6	40.8	9.2 (155) (413) <sup>b</sup>

<sup>a</sup>See Table 4 for listing of the 16 community characteristics.

<sup>b</sup>All cities in the contiguous U.S. with total population greater than 25,000 and Negro population greater than 1,000 in 1960.

Which community characteristics are related to the distribution of racial disorders in the South? To investigate this question, we ran separate regressions with the variables from each indicator cluster. The results are reported in Table 4. Column 1 presents the proportion of variation in number of disorders accounted for by each cluster acting separately. In Columns 2 and 3 the unique components of variation explained by a cluster, controlling for the variables in another, are shown. In Column 2 the unique contribution to  $R^2$  made by the two Negro population variables is presented; while in Column 3 the unique contribution of each cluster, controlling on the Negro population terms, is shown.

The large magnitude of the entries in Column 2 is not surprising since the preceding analyses have already underscored the paramount importance of the Negro population variables. Since the model of disorder-proneness we are using attributes conceptual priority to the Negro population variables (Spilerman, 1970b:643-645), the relevant findings for assessing the roles played by different clusters of community characteristics are the entries presented in Column 3. From these values it is apparent that the indicators of political responsiveness substantially account for the additional variation explained by the community characteristics in the South.

The significance of this cluster is entirely

attributable to two of its four variables—the dummy variables for nonpartisan vs. partisan elections, and mayor-council vs. other administrative structure. It was hypothesized (Spilerman, 1970b:641) that partisan elections and a mayor-council structure form a consistent pair, in that each arrangement should promote political responsiveness to minority group interests. With this interpretation of the indicators, the results are contradictory. Controlling on the Negro population terms and on the other variables in the political structure cluster, number of disorders is positively correlated both with the presence of *non*-partisan elections ( $r=.241$ ), and with a mayor-council structure ( $r=.177$ ).

We may speculate upon a different interpretation of these indicators. Crain *et al.* (1969) have suggested an alternative formulation of the structural determinants of municipal responsiveness which also involves these two variables.<sup>15</sup> In an investigation of the relevance of community political structure to the fluoridation decision, they conclude that an elected official, such as a mayor, in a nonpartisan town is highly vulnerable to public opinion and, therefore, likely to refrain from taking controversial actions. "When elections are partisan, the office holder can rely on a party endorsement

<sup>15</sup> I am indebted to Robert Alford for bringing the Crain, Katz, and Rosenthal discussion to my attention.



TABLE 4. PERCENT OF VARIANCE IN NUMBER OF DISORDERS EXPLAINED BY CLUSTER ALONE (1); BY NON-WHITE POPULATION VARIABLES WHEN ENTERED AFTER CLUSTER (2); AND BY CLUSTER WHEN ENTERED AFTER NON-WHITE POPULATION VARIABLES (3); FOR SOUTH.

Variable Cluster <sup>a</sup>	(1)	(2)	(3)
Non-white Population <sup>b</sup>	31.6	----	---
Social Disorganization <sup>c</sup>	0.7	32.2	1.3
Absolute Deprivation <sup>d</sup>	5.9	26.9	1.2
Relative Deprivation <sup>e</sup>	13.9	20.0	2.3
Political Structure <sup>f</sup>	20.1	17.5	6.0
All Clusters Except Non-white Population	32.3	8.5	9.2
All Clusters	40.8	----	---

<sup>a</sup> All variables are from the 1960 Census of Population unless otherwise indicated in notes b-f.

<sup>b</sup> Non-white population variables are: Log(non-white population), [Log (non-white population)]<sup>2</sup>.

<sup>c</sup> Indicators of social disorganization are: Percent change in total population; percent change in non-white population; percent of housing units dilapidated in 1950 (1950 Census of Population).

<sup>d</sup> Indicators of absolute deprivation are: Percent of non-white males employed in traditionally Negro occupations (service workers, household workers, laborers); non-white male unemployment rate; non-white median family income; non-white median education.

<sup>e</sup> Indicators of relative deprivation are: Percent of non-white males employed in traditionally Negro occupations divided by white figure; non-white median family income divided by white income; non-white unemployment rate divided by white rate; non-white median education divided by white education; percent non-white ( $\sqrt{x}$ ).

<sup>f</sup> Indicators of political responsiveness are: Population per councilman; percent of city council elected at large; presence of nonpartisan elections; presence of mayor-council government (all variables in this cluster are from the Municipal Yearbook, 1967).

to guarantee a certain stability in his support . . . . In contrast, in a non-partisan system any group of citizens can agree to find a candidate for office and support him . . ." (Crain *et al.*, 1969:191-92). As a consequence, "the lack of a strong executive [in a nonpartisan/mayor-council town] might mean an effort to shirk responsibility for action in an area of controversy . . ." (Crain *et al.*, 1969:203).

Much of the racial turmoil in the South has been related to civil rights protests—to demands by Negroes for integration, municipal jobs, and higher wages—in short, to

issues requiring political negotiation. According to Crain *et al.*'s thesis, the communities which would be least capable of compromising with Negro protestors would be the nonpartisan/mayor-council cities. This argument therefore suggests an interaction effect between the two dimensions of political organization—a mayor-council arrangement should have very different meaning for the prospects of resolving community conflict in a party system than where elections are nonpartisan. Examination of the data provides some support for their contention. Controlling on the Negro population variables, presence of nonpartisan elections adds .36 expected disorders to a city (b-coefficient); presence of a mayor-council structure adds .13 expected disorders; while the presence of a nonpartisan/mayor-council arrangement adds *an additional* .41 expected disorders above the additive effects. However, although these regression coefficients are consistent with the interaction thesis, owing to the small number of nonpartisan/mayor-council cities (15 among 155 Southern cities), the interaction term is not significant.

#### IV. PREDICTING DISORDERS FROM NEGRO POPULATION SIZE

The above findings, particularly for the non-South, suggest an efficient procedure for predicting the location of future disorders. Assuming that racial disturbances will continue to be responses to the same type of stimuli which caused the spate of disorders during 1961-68 (namely, frustrations which are uniformly felt by Negroes in all sections of the country), then predictions based upon Negro population size alone should provide an accurate estimate of the distribution of future disorders.

To evaluate the utility of this approach, we used disturbance data for 1961-67 to predict the location of the 1968 disorders. Separate S-shaped curves were estimated for each region from the earlier data; then, using these curves, we calculated a disorder-proneness value for each community. The resulting values therefore define the relation between Negro population and outbreaks of racial violence which existed during 1961-67. In order to predict the locations of the 1968 disturbances, these values were

standardized to sum to the actual number of disorders in 1968. This is necessary because the explanations considered in this investigation relate to the *distribution* of disorders among cities, not to the absolute number which occurred in the nation.

How good are the estimates produced from these calculations? Since the prediction for a city is usually a fraction (expressing an expected number of incidents in 1968), while the actual data are integers (the number of disorders which occurred in a city), only aggregate figures for groups of cities can be meaningfully compared. Table 5 presents the predicted and empirical distributions of disorders for 1968, by region. In each panel of this table the cities have been grouped into six categories, ranging from the most disorder-prone communities (those with largest Negro populations) to the least disorder-prone (cities with smallest Negro populations). The divisions were selected to place an equal number of predicted disorders in each cell, except for the most disorder-prone category. It was arbitrarily decided to place 15 cities in that category in the non-South, and 5 in the South. Otherwise, too few cities would be present to permit stable estimates.

Once again very different results characterize the two regions. In the non-South,

the empirical distribution of disorders (Column 2) appears quite similar to the predicted distribution (Column 1) ( $\chi^2 = 1.85$ , insignificant at the .10 level) and, moreover, the departures from the predicted values do not show a systematic trend. By contrast, in the South, the predictions from the 1961-67 distribution result in a poor match with the actual events ( $\chi^2 = 20.8$ , significant at the .001 level). It is also noteworthy that the deviations are clearly systematic here: There was a tendency in 1968 for small Negro population communities to become increasingly disorder-prone, relative to larger ghettos in this region.

These findings further underscore the results obtained in Section III, namely, while Negro population size may substantially account for the explainable variation in number of disorders in the non-South, the etiology of disturbances in the South is not so simply dealt with. One possibility, regarding the shift in disorder-proneness in the South to small Negro population communities, is that more accurate data are available for 1968 than for earlier years. Indeed, the Lemberg Center's *Riot Data Review* was first published for 1968 and this source is far more comprehensive than the compilations of earlier disorders. However, deficiencies in the data are unlikely to be responsible

Table 5. Predicted and Actual Number of Disorders for 1968, by Negro Population Size, Largest (1) to Smallest (6), by Region.

Negro Popul. Size	Non-South <sup>a</sup>			South <sup>b</sup>		
	(1) Predicted <sup>c</sup>	(2) Actual	N (Cities)	(1) Predicted <sup>c</sup>	(2) Actual	N (Cities)
1	32.6	29	15	12.2	3	5
2	13.0	15	17	6.6	5	5
3	13.5	14	23	6.1	5	8
4	13.5	15	27	6.0	6	14
5	13.3	11	34	5.7	11	33
6	13.2	15	365	5.3	12	107
Total	99.1	99	501	41.9	42	172

<sup>a</sup>Parameter values of S-curve fit to 1961-67 disorder data from non-South are:  $\alpha = 2.212$ ;  $\beta = 1.309 \times 10^4$ ;  $\delta = 2.898 \times 10^5$ ;  $\gamma = 8.344 \times 10^{-6}$ .

<sup>b</sup>Parameter values of 1961-67 S-curve for South are:  $\alpha = 1.168$ ;  $\beta = 2.260 \times 10^6$ ;  $\delta = 1.120 \times 10^5$ ;  $\gamma = 4.420 \times 10^{-5}$ .

<sup>c</sup>Total predicted disorders were standardized to sum to empirical total for the region in 1968.

for the noted shift because of the regional difference in results. Presumably, any bias in compilations of the earlier disorders toward underrepresenting disturbances in small communities would also hold for the non-South.

A more likely possibility concerns the particular experiences of Negroes in small Southern communities. Small communities in this region have been notorious for their extensive control over Negro residents through the use of economic intimidation and repressive police practices (Vander Zanden, 1966:232-34). The adaptation by Negroes to this circumstance has commonly been one of passivity and servility (Matthews and Prothro, 1966:262). For example the "folk Negro" stereotype is associated principally with small towns and rural places in the South, not with metropolitan areas. For this reason, Negroes in small Southern communities may have lagged behind those living elsewhere in developing racial solidarity, and in collectively expressing outrage at instances of the brutal treatment of black persons. However, 1968 was probably a watershed year for residents of these communities because of the assassination of Martin Luther King. The high esteem in which he was held by Negroes is well known (e.g., Brink and Harris, 1967: 54); also, his movement was Southern-based, which may have made his career particularly salient to Negroes in this region. Thus, his death may have stunned these previously passive persons into participating in the black consciousness which had been developing elsewhere in the country. These comments provide one argument which is consistent with the noted increase in disorder-proneness among small Negro population communities in the South, relative to large ghettos.

An alternate possibility, though one not inconsistent with the preceding contention, involves the dual-phase history of Negro protest in the South during the 1960's. In the early years of this decade, the focus of militant civil rights activity—boycotts, freedom rides, sit-ins, and other actions by SCLC, SNCC, and CORE—was upon segregation and discrimination in Southern settings. In a number of instances, racial

violence followed in the wake of tensions generated in a community by these protests.<sup>10</sup> Since the location of civil rights activity in the early 1960's was, to a disproportionate extent, in communities with sizable Negro populations (Birmingham, Montgomery, Jacksonville, and Savannah are some cities which witnessed demonstrations and experienced disorders in subsequent days), the noted decrease in disorder-proneness among large Negro population centers, relative to smaller communities, could reflect the contraction of traditional civil rights protests in the South in recent years.

Both of these arguments suggest that the distribution of Negro uprisings in the South may be converging to the pattern which has prevailed in the non-South throughout the decade. If this assertion is correct, then the 1968 disorders in this region should conform more closely to the relationship between Negro population size and outbreaks of racial violence which has been characteristic of the *non-South* than to the earlier relation between these variables in the South. To investigate this possibility, the 1961-67 curve for the non-South was used together with the Negro population figures for Southern cities to compute an expected number of disturbances for each city, contingent upon the non-South relation.

The results are reported in Table 6. In Column 1 the grouping of disorders which was used in panel B of Table 5 is again presented. In Column 2 the number of disorders predicted for each category from the 1961-67 curve for the *non-South* is reported. It is evident from a comparison of these two distributions that the non-South relation is eminently more successful in predicting the 1968 disturbances in the South than is the curve constructed from the earlier disorders in this region. Consequently, the contention that the distribution of outbreaks in the South has been converging over time to the pattern which has been

<sup>10</sup> Disorders which could be directly linked to civil rights protests or to other organized activities were deleted from the analysis. In many instances, however, the decision as to whether a disturbance was "spontaneous" or derivative of some earlier protest was arbitrary.

Table 6. Predicted and Actual Disorders for South in 1968, from Non-South Curve for 1961-67, by Negro Population Size.<sup>a</sup>

Negro popul. size	(1)	(2)	N (cities)
	Actual	Predicted <sup>b</sup>	
1	3	4.4	5
2	5	2.7	5
3	5	3.5	8
4	6	5.2	14
5	11	10.2	33
6	12	15.9	107
Total	42	41.9	172

<sup>a</sup>Predictions are from S-curve for non-South, 1961-67. See footnote (a) in Table 5 for parameter values.

<sup>b</sup>Total predicted disorders were standardized to sum to empirical total for South in 1968.

characteristic of the non-South is supported by the analysis. Whether for the reasons cited above, or for others, it appears that Negroes in this region have begun to respond to the frustrations associated with being black in much the same manner that Northern Negroes have throughout the 1960's.

#### V. CONCLUSIONS

The question of the adequacy of an explanation has rarely been treated in a systematic fashion. In this paper, this matter was addressed with respect to a recently proposed explanation for the distribution of racial disorders in the 1960's. It has been argued (Spilerman, 1970b) that these incidents were primarily responses to deprivations which are felt by Negroes living in all cities in the country. A number of factors were cited in that study as having operated to override the impact of community variations in the material situation of Negroes as a potential source of unrest. Among these are the reliance by Negroes upon the federal government to promote equality, the emergence of a racial identity which transcends community boundaries, and the wide availability of television. Each of these factors—

a potential source of disappointment to Negroes in all cities, a common cognitive structure for interpreting events in racial terms, and a news medium structured by network programming—serves to promote geographic uniformity in the impact of frustrating stimuli.

The findings reported in this paper both qualify and strengthen the above contention. The explanation is qualified in that, while originally proposed as applicable to disorders in all sections of the country, upon closer inspection it has been found to be appropriate mainly to outbreaks in the non-South. With respect to this region, however, the contention is bolstered by evidence from three separate analyses which suggest that practically all the variation in number of disorders that can be explained using community level variables may be already accounted for by Negro population size. In the non-South, then, Negroes appear to have been responding as a cohesive ethnic unit, not as residents of 501 autonomous communities provoked by their parochial frustrations.

For the South, the results are less straightforward. Two political structure variables were found to be related to racial turmoil and, by one interpretation, they suggest that disorders were more likely where municipal officials lacked the structural support to make unpopular decisions (such as to compromise with civil rights protestors). Over time, however, the distribution of disorders among Southern cities has converged to the pattern which has been prevalent in the non-South, suggesting that black consciousness is beginning to pervade even small Southern communities.

Several additional points warrant comment in order to place these findings in proper perspective. First, this investigation has been restricted to ascertaining the causes of rioting as they relate to the *locations* of the disorders. The problem we have considered is whether particular institutional arrangements make for disorder-proneness. It is not possible to explain the *over-time* variation in number of outbreaks from these results or, in particular, to account for their sudden upsurge in the mid-1960's. Hypotheses have been advanced on this matter, the most common being that the gap between expectations and reality has increased for

the Negro during the Kennedy and Johnson presidencies (National Advisory Commission, 1968:106-110; Fogelson, 1968:38-39). The findings reported in this study, however, neither support nor contradict this or any other explanation of the over-time change. They are relevant to explanations of the temporal variation only in one way. By showing the national character to the outbreaks, they suggest that it will not be possible to choose among alternative explanations which are consistent with the trend in the *national* rate during the 1960's.

A second qualification concerns our ability to generalize from these conclusions to future disorders. It is not necessarily the case that the location of racial violence in subsequent years, if it should occur, will follow the relationships reported in this study. For that matter, trends are at work which may make these results idiosyncratic of the 1960's. Negro mayors have recently been elected in several cities and more are likely to follow. Militant nationalist groups, possibly prepared to employ violent means,<sup>17</sup> have become established in other cities. At the same time, accompanying the election of Richard Nixon, the federal government has become less articulate as a proponent of Negro rights.<sup>18</sup> In combination, these trends may mean that explanations of the location of future disorders will require a consideration of community characteristics. Correspondingly, there is evidence that community conditions were related to the location of racial violence in the first half of this century (Lieberson and Silverman, 1965). This is not at all surprising since the factors enumerated in the preceding discussion as having induced geographic uniformity onto the pattern of Negro aggression during the

1960's were hardly present before World War II.

In addition to these qualifications on generalizing from the findings, a final comment on the concept of randomness seems necessary. What is intended by the claim that the results for the non-South are consistent with<sup>19</sup> a complete explanation of disorder-proneness in that additional variables may be unable to reduce the unexplained variation? After all, at least theoretically, variables could be introduced to account for the probability of a car accident at a particular street corner in a ghetto, and even for the strolling behavior of every individual in the city.

As used in this study, randomness is tied to the level of observation. The findings reported here therefore apply only to community as the unit of investigation. No inference is intended with respect to observations at a different conceptual level, such as using the individual person as the analytic unit and predicting the occurrence of disturbances from the interactions among individuals. Following Nagel (1961:331-333), we therefore distinguish between relative chance and absolute chance. Under absolute chance, random behavior is held to be "uncaused." Under relative chance, the random behavior observed at one level may have causal antecedents at a different conceptual level. For our purpose, the weaker assumption of relative chance is sufficient. Nevertheless, with regard to theorizing on the relationship between community organization and disorder-proneness, reductionist explanations have little utility.

<sup>19</sup> This phrase is intended to convey the fact that it is not possible to prove an explanation by empirical examination. The burden of this paper has been an attempt to disconfirm a thesis by subjecting it to several tests. What can be said is that, because these attempts failed, the explanation is now more credible. For a discussion on the logic of theory verification see Stinchcombe (1968:15-56).

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<sup>17</sup> At this writing, December 1969, the Chicago and Los Angeles police have just raided the Black Panthers' headquarters in the two cities. The location of militant black groups would also be related to the distribution of future disturbances if police instigated actions against them provoke community uprisings.

<sup>18</sup> Following expectational theory, Bowen and Masotti (1968:24-25) suggest that the racial disorders of the 1960's may have been expressions of frustration in a milieu of hope and optimism. Negro aggression during a mood of futility and despair may take a very different form, possibly one of organized guerrilla attacks.

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# THE PUBLIC PERCEPTION OF THE WATTS RIOT AS SOCIAL PROTEST\*

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American Sociological Review 1971, Vol. 36 (June):443-451

*This paper presents a test of several hypotheses regarding the conditions under which publics will identify a collective disturbance as a social protest. Data consist of reactions of white respondents to the Watts Riot. An Index of Protest Definition is presented. Findings indicate that protest definition is most strongly predicted by credibility based upon a predisposing experience or ideology. Forming subjective common cause with protestors appears to be related to protest definition only among those of higher SES. Protest definition is weakly related to belief in support from the group the protestors claim to represent. Perceiving the disturbance as an appeal for help is positively related to protest definition, while experience of threat does not independently predict protest definition. The relationship between conciliation and protest definition is inconclusive.*

THE investigations stimulated by rioting and other forms of collective public disturbance have generally been directed toward identifying the causes for such behavior rather than their consequences. However, since many disturbances occur in the context of sustained collective efforts to bring about social reforms, there is special merit in assessing the effects of collective outbursts on the fate of the associated reform efforts.

There are many facets to any complete assessment of consequences, but one crucial component is undoubtedly the meaning that is attached to the outburst by publics in the larger community. During and immediately after such an event, the community is overwhelmed with massive efforts by special interest groups to use the disturbance to further their own purposes. But publics are responsive to some of these efforts and unresponsive to others, depending upon how they see the broad outlines of the event. One of the most important distinctions is between seeing the disturbance as an understandable expression of social protest, in contrast to seeing it as the unleashing

of massive criminal or anti-social behavior or a concerted group effort to overthrow the system of government to which the general populace is committed. We assume that protest definitions are more favorable to the promotion of social reforms than other definitions.

Elsewhere we have offered a speculative analysis of the conditions under which publics will identify a collective disturbance as an expression of social protest (Turner, 1965). Although gathered for a different purpose, data from the study of white community reactions to the outburst in South Central Los Angeles in 1965, commonly known as the "Watts Riot," can be used to test some of these speculations in a provisional way.<sup>1</sup>

## AN INDEX OF PROTEST DEFINITION

An act of social protest has been defined as including all of the following elements: "the action expresses a grievance, a conviction of wrong or injustice; the protestors are unable to correct the condition directly

\* The data used in this analysis were gathered in 1965 by Richard T. Morris and Vincent Jeffries as part of the Los Angeles Riot Study administered by the Institute of Government and Public Affairs at the University of California, Los Angeles. Funds for the research were provided by a grant from the Office of Economic Opportunities. Computer services were provided by San Fernando Valley State College Computer Center.

<sup>1</sup> Data for this study consist of 583 interviews with whites living in Los Angeles, California. The sample design provided for gathering 100 interviews in six communities in the Los Angeles metropolitan area. Two communities were selected to represent each of three levels of socioeconomic status—high, middle and low. One community at each level was an all white community, the other contained an approximately equal number of blacks and whites. All have a population containing less than 10% Spanish surnames.

by their own efforts; the action is intended to draw attention to the grievances; the action is further meant to provoke ameliorative steps by some target group; and the protestors depend upon some combination of sympathy and fear to move the target group in their behalf" (Turner, 1959: 816).

Three items in the survey of white reactions to Watts seem to express the more important elements, and can thus serve as an index of protest definition. In answer to the question, "Some people say that the riot, rebellion, etc. was a Negro protest. Do you agree with that point of view?" the "yes" answer was given by 54% (Morris and Jeffries, 1967:5). Answering the question, "What do you think caused the riot, etc.?", 33% said: current unfair treatment, history of injustice, police brutality or other sympathetic responses and combinations. The comparison question, "What steps do you think should be taken to prevent further riots, etc.?" produced 56% protest answers as follows: better jobs, housing, education, etc., change white attitudes, and other sympathetic answers.

When the items are appropriately dichotomized and cross-tabulated (Table 1), each is substantially related to the other, but the relationships are not so high as to make them redundant. Combining dichotomized responses to the three items produces an *Index of Protest Definition* (hereafter referred to as IPD), whose values range from zero—when none of the responses qualify as protest definitions—to three, when all the responses indicate protest definitions.<sup>2</sup> When the IPD is dichotomized as closely as possible to the median, the 52% (288) of the sample scoring 0 or 1 on the index are classified as not making a protest definition, while those scoring 2 or 3 are included among the 48% (263), classified as making a protest definition.

A pair of questions regarding the purpose of the disturbances provide a limited valida-

tion for the Index. Subjects were asked, "Do you think the riot, etc., had a purpose or goal?" Those who replied affirmatively were then asked, "What was the purpose?" Purposes were classified as (1) to call attention to problems or injustice, air grievances, change the situation, solve problems, or get help; and (2) show deviance, desire to get unjustified material gains, unjustified complaints, or conspiracy. In an exceptionally strong relationship, 77% of the people who gave the former answers were identified as protest definers by the IPD, compared with only 23% of persons giving the latter answers ( $\gamma = .82$ ,  $p < .001$ , 1 df). This observation is consistent with the idea of protest as *communication* more than coercion or direct action.

Table 1. Interrelationships among Protest Label, Cause, and Prevention.

Protest?	Cause		Total
	Protest	Not Protest	
Protest	132	171	303
Not Protest	<u>45</u>	<u>194</u>	<u>239</u>
Total	177	365	542
$\gamma = .54$ , $p < .001$			
Prevention	Cause		Total
	Protest	Not Protest	
Protest	143	162	305
Not Protest	<u>36</u>	<u>180</u>	<u>216</u>
Total	179	342	521
$\gamma = .64$ , $p < .001$			
Protest?	Prevention		Total
	Protest	Not Protest	
Protest	203	94	297
Not Protest	<u>99</u>	<u>122</u>	<u>221</u>
Total	302	216	518
$\gamma = .45$ , $p < .001$			

\* Responses are fairly evenly distributed as follows: 0, 18%, (105); 1, 26%, (152); 2, 24%, (141); 3, 19%, (108). For most purposes the index was dichotomized, enabling us to include a few more cases in which one question was not answered, but in which the answer could not have altered placement in the dichotomy.



Evidence that addition of the cause and prevention questions to the labeling question makes a more valid index comes from a standard probe included in the interview schedule. Subjects who agreed that the disturbance was a Negro protest were asked, "What do you think it was a protest against?" Answers were classified as (1) current unfair treatment, historical injustices, or police brutality; and (2) the triggering incident or unjustified protest. Eighty-six percent of the 287 persons who were asked the probe question gave answers in the former group. Eighty-one percent of these respondents were classified as protest definers using the dichotomized IPD, compared with only 45% of respondents giving answers in the second group ( $\gamma = .67$ ,  $p < .001$ , 1 df.).

There is a substantial relationship between IPD and the term used to describe the disturbance, with protest definition markedly more frequent among the users of transparently sympathetic terms.<sup>3</sup> "Riot" is the commonest term, and is also the most nearly neutral term, dividing respondents fairly evenly between protest and nonprotest definitions. Protest definition is highest (80%) among persons using terms such as insurrection, revolution, and revolt.

A measure of favorableness-antagonism toward blacks shows moderate correlation with the IPD ( $\gamma = .40$ ,  $P < .001$ ).<sup>4</sup> We shall frequently employ this measure as a control in order to insure that we are not simply measuring a generalized attitude toward blacks in a roundabout fashion. The IPD is slightly related to age and sex—females and younger age levels more often interpreting the disturbance as protest. The IPD is unrelated to a three-item measure of powerlessness based on the work of Seeman (1959).

*Main Areas of Inquiry.* The meanings

that people attach to an event reflect a complex interplay of personal preconceptions and emotions, perceivable characteristics of the event, and the social relationships to which attention is directed by the event. From among these many determinants, we have singled out the credibility of the protest image to the individual, the communication balance between appeal and threat, and conduciveness to gestures of conciliation.

#### CREDIBILITY OF PROTEST

*Sensitizing Experience or Attitude.* By credibility we mean the ease with which the individual can believe that what he is seeing, hearing, and reading about is an act of protest. An underlying determinant of credibility in any realm of life is the presence of some prior experience or knowledge or attitude that prepares the individual for what he encounters. A conclusive test would require awareness measures taken before the disturbance, but we shall make provisional examination of this and other credibility hypotheses on the basis of responses given soon after the disturbance.

Findings bearing on the hypothesis of prior experience and knowledge are as follows. (1) Subjects who believe there is "some" or "a great deal" of discrimination against the Negro in Los Angeles are substantially more likely to define the disturbance as protest ( $\gamma = .57$ ,  $p < .001$ ), and the relationship is almost unchanged when cases are separated into those with favorable and unfavorable attitudes toward blacks (Table 2). (2) As distinguished by a two-item index, subjects with positive attitudes toward the civil rights movement give substantially more protest definitions than subjects with unfavorable attitudes ( $\gamma = .42$ ,  $p < .001$ ). Controlling for favorability toward blacks reduces the magnitude of this relationship, but it remains highly significant among persons with favorable attitudes and of borderline significance among the unfavorable (Table 3). (3) Subjects who reported having participated in any of three types of civil rights activity gave substantially more protest definitions than subjects who had not ( $\gamma = .44$ ,  $p < .001$ ).<sup>5</sup> Controlling for attitude

<sup>3</sup> Interviewers always used whatever term the respondent selected in any questions which referred to the event.

<sup>4</sup> The measure of favorability-antagonism toward blacks is a composite measure which includes five social-distance items, a question pertaining to degree of trust in blacks, and three statements of negative beliefs about blacks. Each type of attitude is equally weighted in the index. When this distribution is dichotomized as closely as possible to the median, 53% (291) are classified as favorable and 47% (261) as antagonistic.

<sup>5</sup> The basic cross-tabulations of IPD and belief

Table 2. Belief about Amount of Discrimination against Negroes in Los Angeles and Index of Protest Definition, by Attitude Toward Negroes.

IPD	All Cases		Favorable Toward Negroes		Antagonistic Toward Negroes	
	Great Deal or Some	Very Little or None	Great Deal or Some	Very Little or None	Great Deal or Some	Very Little or None
Protest	59%	29%	67%	36%	48%	23%
Not Protest	<u>41</u>	<u>71</u>	<u>33</u>	<u>64</u>	<u>52</u>	<u>77</u>
Total	100	100	100	100	100	100
	(329)	(196)	(158)	(115)	(83)	(146)
	$\gamma = .57, p < .001$		$\gamma = .57, p < .001$		$\gamma = .50, p < .001$	

toward blacks reduces this relationship very little. (4) A three-item index of belief that the requests of blacks are just is likewise substantially related to protest definition ( $\gamma = .50, p < .001$ ). These four basic relationships might be viewed as obvious if they simply recapitulated the correlation between favorable attitudes toward Negroes and protest definition. But these associations are generally larger than the association with attitude toward Negroes, and remain substantial when the latter is controlled, lending plausibility to the assumption that credibility is an independently important determiner of protest definition.

**Folk Concept.** A second major determinant of credibility is the relationship between the event as it is perceived and the generalized conception people have of what an act of protest is like. Only one element of the folk concept lent itself to direct and uncomplicated examination on the basis of the Watts data. If the claim of persons who engaged in violence and disruption is to be given credit, they must have substantial support from the group in whose name they are protesting.

Unfortunately, the three available items are ambiguous with respect to this hypothe-

sis, since one who supported the aims but not the methods of rioters could be said either to support or oppose the disturbance. Consequently even weak relationships may be interesting. (1) Subjects who say that 30% or more of people in the area *participated* are somewhat more likely to give a protest definition than subjects who say fewer than 30% participated ( $\gamma = .18, p < .05$ ). (2) Belief that a large proportion of people in the area *supported* the disturbance appears to foster a protest definition, but the relationship is not significant. (3) Belief that fewer than two-thirds of the people in the areas were *against* the disturbance is mildly conducive to protest definition ( $\gamma = .23, p < .02$ ). Hence we find minimal confirmation for the hypothesis that identification of a disturbance as protest is more credible when there is thought to be substantial support and little opposition within the constituency.

**Comparison Theory.** According to popular thinking, those who share a common fate are the most likely to understand and sympathize with each other's complaints. But the long history of failure in efforts to unite disadvantaged groups suggests an opposing tendency for one group to evaluate another group's complaints *by comparison* with their own situation. Instead of saying, "Yes, I understand the Negroes' complaints because I have suffered many of the same disadvantages myself," the unskilled white laborer is likely to say, "What are they complaining about? They are no worse off than we are!" For this reason we speculated earlier that "groups who are clearly *advantaged* by

in discrimination, civil rights attitude, and civil rights activity were also controlled for sex, age, political party, income, occupation and education. Findings strongly suggest the relationships are not spurious. Forty-one of 51 partials show a significance level of .05 or less, with most of the nonsignificant partials being recorded for occupational categories in which the number of cases is less than 80. The average correlation ( $\gamma$ ) for 51 partials is .45.

Table 3. Civil Rights Attitude and Activity and Index of Protest Definition, by Attitude Toward Negroes.

	IPD	Civil Rights Attitude		Civil Rights Activity	
		Pos.	Neg.	Yes	No
All Cases	Prot.	59%	37%	60%	37%
	Not Prot.	41	63	40	63
	Total	100	100	100	100
		(278)	(257)	(264)	(281)
Favorable Toward Negroes	Prot.	64%	47%	64%	47%
	Not Prot.	36	53	36	53
	Total	100	100	100	100
		(192)	(86)	(183)	(101)
Antagonistic Toward Negroes	Prot.	46%	33%	51%	31%
	Not Prot.	54	67	49	69
	Total	100	100	100	100
		(74)	(161)	(75)	(162)
		$\gamma = .42, p < .001$		$\gamma = .44, p < .001$	
		$\gamma = .33, p < .001$		$\gamma = .34, p < .001$	
		$\gamma = .27, p < .10$		$\gamma = .38, p < .01$	

comparison with the 'protestors' can find the claim of injustice more credible than groups less advantaged" (Turner, 1969:819).

When reported income is divided into three categories, there is no significant relationship with IPD, though the trivial, apparent relationship shows more protest definition by high than low income respondents, with the noticeable break between the low and middle income groups. Similarly, occupation is not significantly related to IPD. But a four-category occupational classification shows 11 percentage points fewer protest definers in the bottom category of blue-collar workers than in the three higher categories. Educational attainment in four categories shows a slight relationship of borderline significance ( $\gamma = .15, p < .05, 3$  df.). But here the break of 10 to 14 percent is between the middle categories, namely, high school graduates and persons who have had some college education.

If we must choose between the popular view of sympathy from shared misfortune and comparison theory, the latter is favored by these data. The apparent relationship is not unilinear, but rather separates people whose socioeconomic characteristics are similar to those of the "protestors" from people of higher SES.

The common-cause hypothesis can be examined in another way. After asking about discrimination against Negroes in several different situations, interviewers asked, "Do you think white people with low incomes are discriminated against in any of these situations?" and "Have you yourself ever been discriminated against?" Both questions show weak relationships of borderline significance in the common-cause direction ( $\gamma = .20, p < .05, 1$  df, for both relationships).<sup>6</sup>

The different results obtained for common cause as an objective and as a subjective phenomenon suggested examining the relationships between SES and belief in discrimination against self and low income whites and their joint effect upon protest definition. With minor discrepancies the results show that for all three measures of SES the belief in discrimination against self and against low income whites increases as SES increases. In the most striking instance, 46% of respondents without a high school education, as opposed to 73% of college graduates, believe that low income whites are discriminated against. Comparison theory is thus favored by the data over a common-cause interpretation.

The joint relationship between SES, our two measures of subjective common cause and IPD, is shown in Table 4. The tendency of the lowest SES levels to give a protest definition least frequently remains regardless of whether subjective common cause is present or not, and the percent giving a protest definition is quite similar in the partials to that obtained in the original relationships. Opinions regarding the existence of discrimination against self or others appear to have almost no effect here.

<sup>6</sup> Four similar questions regarding unfavorable police practices showed no relationships to IPD. For most whites the police came into their lives only incidentally and occasionally, so their beliefs and attitudes about police may be of little significance for protest definition.

Table 4. Percent Giving Protest Definition According to Socio-economic Status, Controlling for Belief in Discrimination Against Low-Income Whites and Against Self.

	Disc. Low Inc. White		Disc. Self	
	Yes	No	Yes	No
Occ.:				
B-C	37%	35%	38%	38%
C & Sales	55	44	40	53
M'gerial	56	47	62	49
Prof.	59	38	59	45
	(141)	(88)	(78)	(173)
	p<.20 $\gamma = -.26$	p=NS $\gamma = -.09$	p=NS $\gamma = -.27$	p=NS $\gamma = -.10$
Edu.:				
0-11 Yrs.	44%	42%	41%	45%
H.S. Grad.	39	42	43	40
Some Col.	57	45	59	52
Col. Grad.	65	32	69	46
	(276)	(198)	(138)	(384)
	p<.02 $\gamma = -.27$	p=NS $\gamma = -.04$	p<.10 $\gamma = -.32$	p=NS $\gamma = -.07$
Inc.:				
Low	45%	40%	50%	40%
Mid.	49	45	55	46
High	55	39	58	46
	(280)	(209)	(141)	(395)
	p=NS $\gamma = -.13$	p=NS $\gamma = -.04$	p=NS $\gamma = -.10$	p=NS $\gamma = -.05$

However, a very different picture emerges at the high end of the SES scale. Here subjective common cause has a noteworthy effect—respondents much more often giving protest definitions if they believe they have been discriminated against or that low income whites are discriminated against. The findings thus appear to indicate that subjective common cause affects protest definition only among those who are in a clearly more advantaged position than the protestors. From another perspective, the results tentatively suggest that the definition of a given event as protest may stem in part from a broader perception and experience of social injustice, which predisposes the individual to be sympathetic to appeals for amelioration

from a potentially wide variety of groups.

#### APPEAL, THREAT, AND CONCILIATION

The remaining hypotheses deal with communication and interaction processes. In the communication process, an optimal combination of appeal and threat messages maximizes the chance that a disturbance will be interpreted as social protest. We have no way with the present data to compare *degrees* of appeal and threat, so that we cannot assay the optimal balance hypothesis. But we can examine some indirect indicators of appeal and threat in the message received by our sample of Los Angeles whites.

*Appeal.* A public fails to identify a disturbance as protest when the appeal—the identification of grievances and the call for help—is too soft to be heard, or when the threat to the hearer is so loud that it drowns out the appeal. Our data do not tell us what the persons interviewed actually heard, but we shall make inferences from what our subjects say other people heard.

Eighty-two percent of all the persons interviewed thought the disturbances had made whites more aware of Negro problems; so we can assume that minimally effective communication of the appeal component took place on a wide scale. (1) Protest definitions were somewhat more common among person who stated that whites were now more *aware* of Negro problems than among persons answering "less aware" or "no change" ( $\gamma = .27$ ,  $p < .05$ , 2 df). (2) As predicted, the largest proportion of protest definitions (50%) is found among persons who believe whites are more *sympathetic* than before, and the lowest proportion (39%) is among persons who say whites are less sympathetic, with "no change" taking the intermediate position (48%) ( $\gamma = .28$ ,  $p < .001$ , 2 df). (3) The question, "Do you think the riot, etc., increased or decreased the gap between the races?" has a moderately substantial and highly significant relationship with IPD ( $\gamma = .44$ ,  $p < .001$ ), but the difference is entirely between those who believe there is negative change and those who see either favorable change or no change. (4) In answer to the query, "Do you think the riot, etc. helped or hurt the Negro's cause?" 79%

said it hurt their cause. This reply showed a substantial and significant relationship to IPD, with 77% of persons who said it helped the cause and only 39% of persons who said it hurt the cause, defining the disturbance as protest ( $\gamma = .68$ ,  $p < .001$ , 1 df).

When attitude toward blacks is held constant, the weak relationship between IPD and the awareness question drops below the minimum significance criterion, though the magnitude of the relationship as measured by *gamma* is not greatly reduced. The relationship between IPD and the sympathy question drops to a trivial level among persons who are antagonistic toward blacks, but remains moderate and significant among those who are favorable in their attitude ( $\gamma = .31$ ,  $p < .01$ , 1 df). For the other two questions, relationships remain substantial and significant. Hence most of the support we have found for the appeal hypothesis remains when we employ a dichotomous control for attitude toward blacks.

*Threat.* The threat hypothesis says that too little threat fails to win attention for the appeal and too much threat diverts attention away from the appeal. We have assumed that the magnitude of the Watts disorder was sufficient to insure the necessary minimum of threat for most white citizens. Hence the hypothesis is simplified to state that persons who experience excessive threat are less likely to define the events as social protest than those who experience more moderate threat.

(1) A fairly direct measure of the sense of threat comes from the question, "During the time of the riot, etc., did you feel fear for yourself or your family's safety?" A small difference in protest definition in the predicted direction does not come close to minimum significance standards.<sup>7</sup> (2) The questions—"Did you, at any time, consider using firearms to protect yourself or your family?" and "Do you approve of white people buying guns to protect themselves

during the riots, etc.?"—were moderately but significantly related to IPD in the predicted direction ( $\gamma = .33$ ,  $.23$ ;  $p < .001$ ,  $< .01$ ; 1 df). Controls show that among males who are antagonistic toward blacks the negative relationship between IPD and approving of firearms is stronger ( $\gamma = .44$ ,  $p < .05$ , 1 df), while it is reduced in all other cells. Perhaps the cultural expectation for males to protect home and family, combined with a preexisting negative image of the potential aggressor, is necessary to predispose the individual who feels threatened to exclude any thought of legitimacy which might interfere with resolute action. These findings cannot be said to enhance the plausibility of the view that experiencing threat, insofar as it is independent of other factors, decreases the likelihood of defining a major disturbance as social protest.

*Conciliation.* If protest definition is seen as an act of conciliation, it is made with the expectation that an appropriate response will be forthcoming. If it is not, the gesture is likely to be withdrawn. Thus, if blacks do not respond by softening their attacks on whites, the white is likely to redefine the disturbances—not as social protest but as evil behavior of some sort.

Three questions enable us to explore the conciliation hypothesis indirectly at a moment in time. The most directly relevant question asks interviewees to agree or disagree that "Most Negroes are ungrateful." If defining blacks' actions as protest is a generous gesture, then a preconception that blacks are ungrateful would discourage one from inviting rebuff. Eighty-one percent of the interviewees disagree with the statement, but the predicted relationship with IPD is highly significant ( $\gamma = .37$ ,  $p < .001$ , 1 df).

Two other questions bear on this hypothesis, but somewhat tangentially. Interviewees were also asked to agree or disagree that: "Generally speaking, Negroes are violent people"; and "The majority of the Negroes in Los Angeles have respect for law and order." The violence question produces a weak relationship of borderline significance ( $\gamma = .28$ ,  $p < .05$ , 1 df), and the law and order statement produces an apparent relationship with IPD in the predicted direction that does not meet minimum criteria of significance.

<sup>7</sup> Sex is moderately related to IPD, strongly related to both fear and opinions regarding the use of firearms. For these reasons cross-tabulations pertaining to the threat hypothesis are controlled for sex as well as attitude towards blacks. The relationship between IPD and fear is not improved when these controls are used.

When we control for attitude toward blacks,<sup>8</sup> the apparent degree of relationship between the more crucial "ungrateful" question and the IPD drops very little, but the relationship is now of only borderline significance among persons showing high social distance ( $\gamma = .34$ ,  $p < .05$ , 1 df) and nonsignificant ( $\gamma = .29$ ) among persons showing low social distance. The relationships between IPD and the violence and law and order questions are no longer significant. Thus the hypothesis of a relationship between perception of the likelihood that conciliation will be met with counter-conciliation and protest definition, independent of a subjective measure of social distance, is neither clearly supported nor clearly negated.

#### CONCLUSIONS

Our findings are most consistent and strongest in the case of credibility theory involving a sensitizing experience or predisposing ideology. The modest support for comparison theory—when objective criteria of position are used—and the opposing common cause theory—when subjective measures are used—suggests an important area of future investigation. Many of the other relationships might have been clearer if the items had been designed as indicators of the basic theoretical propositions.

The problem of causality is pervasive. Which comes first, a belief in discrimination against blacks in Los Angeles and favorability toward the Civil Rights Movement or a protest definition of Watts? Theory and research in the area of personality structure (Allport, 1955:88-93; Festinger, 1957; Krech and Crutchfield, 1948:60-70; Rokeach, 1968:13-33; Smith, 1947:507-523; 1969:97-116; Woodruff and DiVesta, 1948:645-659; Rosenberg, 1956: 367-372; Carlson, 1956: 256-261; Osgood, 1960:341-365; Postman *et al.*, 1948:142-154) indicate that the more fundamental and general dispositions provide focal points for the organization of the more specific and less stable aspects of personality. Similarly, the existing structure

is a major determinant of how new events will be interpreted. Tentatively, it is reasonable to regard attitudes toward the Civil Rights Movement and beliefs about discrimination against blacks in Los Angeles as both prior to and somewhat more generalized than interpretations of the particular event of Watts.

Finally, it is important to consider what contribution our study has made to a better understanding of how those interested in improving the conditions of blacks and other disadvantaged groups may most effectively pursue their attempts to attract and mobilize the good will of what is usually an apathetic and often hostile majority. If the assumption that support of needed social reforms is closely linked to perceiving a collective disturbance as social protest is correct, then an understanding of the reasons for such a definition can be of considerable value. Our findings indicate that the previous recognition of existing injustices is of central importance. For example, even among those whose basic attitudes towards blacks are relatively antagonistic, almost half (48%) see the Watts riot as a protest if they believe discrimination exists in some degree, as compared to only 36% of respondents seeing the disturbance as a protest among those who are favorable in their basic attitudes toward blacks but believe there is very little or no discrimination. Thus credibility due to awareness of injustice is more important than a basically favorable attitude toward the protesting group in its effect upon a protest definition.<sup>9</sup> Consequently, devising ways to intensify public awareness of the extent of prejudice and discrimination continues to be a promising strategy toward reducing racial inequality in the United States.

<sup>9</sup> The application of Coleman's (1964:189-240) multivariate technique shows that prejudice accounts for .160 of the variation in protest definition; belief in discrimination, .275.

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<sup>8</sup> Because the three items were included in the generalized measure of attitude toward blacks, we have used only the dichotomized five-item measure of social distance as a control for these conciliation hypothesis relationships.

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## VARIATION IN COUNTY SIZE: A THEORY OF SEGMENTAL GROWTH

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*American Sociological Review* 1971, Vol. 36 (June):451-461

*Durkheim argued that improvements in transportation or further increases in the density of a population would lead to increased division of labor and to the effacement of segmental (territorial) types of organization. This paper develops the thesis that segmental growth—an increase in the number of segmental units—is the result of expansion of the settlement area under constant conditions of transportation. Two recent models of geopolitical structure, developed by Boulding and Stinchcombe, are shown to be related to this thesis and are made the basis for a formal representation of it. The thesis is then used to explain a particular case of segmental growth—the historical process of dividing states into county jurisdictions. Existing variation in county size is explained by the termination of segmental growth following the introduction of the automobile.*

COUNTIES in the United States vary considerably in size. The largest, San Bernardino County in California, covers 20,131 square miles; the smallest, New York County, covers only 22 square miles. The so-called "independent cities" in Virginia, which have the legal status of counties, are as small as one square mile each. The average size county in the United States is 961 square miles; median size is about 600 square miles, and the mode is about 500 square miles (Anderson, 1945:24). The

purpose of the research reported here was to investigate this tremendous variation in county size and to explain it.

It is appropriate to ask whether or not an explanation of county size variations has any significance in terms of sociological theory. In fact, the explanation developed here is directly related to the theory of social change developed by Durkheim (1933) in his classic work, *The Division of Labor in Society*. In that work, he distinguishes two fundamental types of social

organization, the "segmental type" and the "organized type." The segmental type consists of a "system of segments homogeneous and similar to each other" (1933:181), and there are two subtypes: those based upon consanguineous groupings (e.g., clans, tribes) and those based upon territorial units (e.g., counties, provinces, etc.). Durkheim's main interest, however, seems to lie with the latter—"The bulk of the population is no longer divided according to relations of consanguinity, real or fictive, but according to the division of territory. The segments are no longer familial aggregates, but territorial circumscriptions" (1933:185). Different levels of territorial units coexist in an area, and their combination constitutes society: "Thus it is that all peoples who have passed beyond the clan-stage are organized in territorial districts (counties, communes, etc.) which . . . connected themselves with other districts of similar nature, . . . which, in their turn, are often enveloped by others, still more extensive (shire, province, department), whose union formed the society" (1933:186).

The primary argument of Durkheim's work is that this system of territorial units is gradually being replaced by the organized type, based on the division of labor. "It is a general law," writes Durkheim (1933:187), "that partial aggregates which participate in a larger aggregate see their individuality becoming less and less distinct." The reason for this is that functions originally carried out within the partial aggregates begin to be carried out at the level of the larger aggregate, under pressure of competition. In the process, each of the functions becomes more specialized as the reoriented set of functions becomes more interdependent. "Territorial divisions are thus less and less grounded in the nature of things, and consequently, lose their significance" (1933:187). The end result of this process is the "organized type," constituted "not by a repetition of similar, homogeneous segments, but by a system of different organs, each of which has a special role, and which are themselves formed of differentiated parts" (1933:181). The "progressive preponderance of organic solidarity" results in and from the "effacement of the segmental type." Social change thus con-

sists essentially of an increased division of labor with greater functional interdependence as territorial divisions become increasingly artificial.

### *Segmental Growth*

Social change, in this form, has received considerable attention from sociologists; it is practically the definition of social change for many of us. But social change in the form of the development of the segmental type itself—"segmental growth": adding more units similar in structure to those already in existence—to my knowledge has received very little attention. The significance of this lack of attention can be seen in Nisbet's comment on the relevance of the segmental type to Durkheim's other work: ". . . Durkheim never went back, in later studies, to any utilization of the distinction between the two types of solidarity, nor to the division of labor as a form of cohesion. . . ." (Nisbet, 1966:86).

That counties are segmental structures is evident from Durkheim's own consideration of them in the passages quoted above. That current variation in county size may be related to the phenomenon of segmental growth, however, may not be so evident. The connection arises from the fact that *current* variation in county size is the product of a *historical* process—the process of dividing states into county jurisdictions over time, in almost every case by dividing larger counties into smaller ones. A compilation of the dates of creation of counties in the United States (e.g., Kane, 1962) shows that no state divided its jurisdiction into its present-day counties all at once. The first counties in the U.S. were created in Virginia in 1634; the last one added to that state was created in 1880. The same is essentially true for the other states: in the beginning a set of small counties is created in part of the state, the remainder of the state consisting of much larger counties. In time, the larger ones are subdivided, until the terminal number is reached. This division process—adding more counties to those already in existence within a state—constitutes segmental growth. The same division process, by going more or less to completion, produced the currently observable variation in county size.



The historical process is illustrated in Table 1, which shows the accumulation of counties in each state from 1790 to the present. The table was prepared by counting, in the census volumes, the number of counties in each state or territory during each census year. The states (indicated by their abbreviations) are arranged in the order in which they entered the Union; the census year by which statehood had been achieved is indicated by the line running through the table. It is evident that some counties were formed by colonial and territorial gov-

ernments; some present-day states, in fact, were at one time merely counties of other states. Vacant cells indicate that no further change in the number of counties was indicated, that the division process of segmental growth had come to an end. It may be noted that two counties in Georgia were, in 1932, consolidated with a third to reduce the number in that state.

It may be argued that the existing variation in county size no longer needs explanation, or rather that it has just been explained as the product of the historical

Table 1. Accumulated Number of Counties, by State and Census Year, in Order of Admission (Indicated by the Line Through the Table).

State	Census Year (1790-1950)																		
	90	00	10	20	30	40	50	60	70	80	90	00	10	20	30	40	50		
Del.	3																		
Pa.	22	35	42	51	51	54	63	65	66	67									
N.J.	13	13	13	13	14	18	20	21											
Ga.	11	24	38	47	76	93	95	132	132	137	138	138	147	155	161	159			
Conn.	8																		
Mass.	11	12	12	13															
Md.	19	19	19	19	19	20	20	21	22	23									
S.C.	20	25	28	28	29	29	29	30	31	33	35	40	43	46					
N.H.	5	5	6	6	8	8	10												
Va.	71	75	77	78	79	88	92	96	97	98									
N.Y.	15	30	43	50	56	58	59	60	60	60	60	61	61	62					
N.C.	54	59	61	61	63	67	78	86	90	94	96	97	98	100					
R.I.	5																		
Vt.	7	11	12	13	13	14													
Ky.	9	42	54	67	82	89	100	109	115	117	119	119	119	120					
Tenn.	8	18	38	52	62	71	79	84	85	94	95								
Ohio		7	36	59	73	79	87	88											
La.			20	26	31	38	47	48	53	58	59	59	60	64					
Ind.		2	4	35	64	87	91	92											
Miss.		3	11	17	26	56	59	60	65	74	75	75	79	82					
Ill.		3	2	19	51	87	99	102											
Ala.		1	3	24	36	49	52	52	65	66	66	66	67						
Maine	5	6	8	9	10	13	13	16											
Mo.		5	15	32	62	100	113	114											
Ark.		1	7	23	39	51	55	61	74	75									
Mich.		1	4	5	13	32	43	62	76	77	82	83							
Fla.				16	20	28	37	39	39	45	45	47	54	67					
Texas						80	133	144	210	238	244	246	253	254					
Iowa					18	49	97	98	99										
Wisc.				2	3	22	31	58	63	68	70	71							
Calif.						27	44	50	52	53	57	58							
Minn.						9	64	72	78	80	82	86	86	87					
Ore.						8	19	21	23	31	33	34	36						
Kansas							41	64	103	105									
W. Va.	10	13	16	21	23	28	42	50	53	54	55								
Nevada								3	14	15	14	14	15	17					
Neb.								36	55	70	90	90	92	93					
Colo.									21	31	55	57	60	63					
N. Dak.										21	41	39	49	53					
S. Dak.										13	48	56	56	64					
Mont.										11	16	24	28	51	56				
Wash.							2	19	23	25	34	36	38	39					
Idaho									9	13	16	21	23	44					
Wyo.									5	7	12	13	14	21	23				
Utah								7	17	21	23	25	27	29					
Okla.											8	75	76	77					
N. Mex.							7	10	13	12	14	19	26	29	31	31	32		
Ariz.								1	4	7	10	13	13	14					

process of jurisdictional division. Explanations of this type, referred to as "genetic explanations" by Brown (1963:47-58), are essentially "descriptive"—they explain the present by describing a development which occurred in the past. As Clark (1954:71) puts it: "The genetic approach focuses attention on processes, for whatever interests us in the contemporary scene is to be understood only in terms of the processes at work to produce it." But such explanations often only serve as the basis for restating the original question in historical terms. Granted that present variation is the product of an historical division process, we may ask why the process occurred as it did—why did segmental growth take place? And, in addition, why did the process terminate so abruptly, throughout the country, between 1920 and 1930? The only county created in the United States after 1925 was Los Alamos County, New Mexico, in 1949 (Kane, 1962:332). Regardless of the differences between states with respect to the beginning dates of territorial subdivision, and regardless of the extent to which states had subdivided their larger counties into smaller ones, they all terminated the process during the 1920s.

### *Causes and Conditions*

Book I of *The Division of Labor* by Durkheim is primarily concerned with genetic explanation. He explains specialization and interdependence in modern society as the product of twin historical processes: the progressive preponderance of organic solidarity and the simultaneous effacement of the segmental type. If we ask why these historical processes occurred, we must turn to Book II for Durkheim's answer. Further development of the division of labor (further effacement of the segmental type) was due, he argued, to further *concentration of the population* and/or to further *improvements in transportation or communication*. If, then, we ask why segmental growth should occur, the answer must be in terms precisely the opposite of those which Durkheim used to explain the effacement of the segmental type, namely, to the *dispersal* of population under *constant* conditions of transportation and communication.

Applying this general explanation of segmental growth to the specific phenomenon of subdividing states into counties, it would follow that new counties would be created as the settlement area within a state or territory expanded. How much expansion of the settlement area would be required in order to necessitate the creation of new counties? It is here that the connection between the settlement area and the transportation technology becomes important. Assuming with Hawley (1950:302) that "The diurnal cycle . . . constitutes the primary unit in the rhythm of activity"—that people prefer to return every evening to their place of residence—a county's boundaries would have to be such that its citizens would live close enough to their county seat in order to conduct whatever business they might have there and still be able to return home in the evening. The distance involved here would depend directly on the system of transportation which people had available to them. When a sufficient number of people had settled in an area lying beyond the limit so imposed, provision of county government services would require creation of a new county. Expansion of the settlement area, then, under constant conditions of transportation, could have led to the creation of new counties; and, since new counties could only be created by subdividing counties with nominally large jurisdictions, the same conditions would have led to a reduction in county size variation (i.e., to a reduction in the number of large counties in a state).

As to the termination of the process of segmental growth, we would expect this to occur, generally, whenever there was no further dispersal of population or when improvements in transportation could offset whatever dispersal might subsequently occur. It will be recalled that the process of further dividing states into counties came to an end throughout the United States by 1930. The coincidence of this date with the period during which the automobile was widely introduced suggests that it was a sufficient improvement in the system of transportation to terminate the process of further subdividing whatever large counties remained at that time. We get some idea of the impact of the automobile, with respect to the areal size of the field of daily

movement, in Hawley's (1950:352) comparison between the velocity of human or horse-drawn transportation systems and that of motor-driven systems: 15–20 miles per day for the former, 250–600 miles per day for the latter. For the extremes, time-cost is reduced by a factor of forty with the newer technology. The automobile, then, seems to have been the agent which “froze” county boundaries where they were in the 1930s, revealing through the distribution of large and small counties the extent of population movement up to that time.

The argument thus far has tended to view counties from the perspective of the dispersed citizens and their ability to conduct business at the county seat under conditions imposed by the diurnal cycle and the transportation technology available to them. Equally important, particularly during early periods of settlement, was the ability of the government at the county seat to enforce its regulations throughout its jurisdiction (e.g., the ability of the assessor to visit property within the county, or the ability of the sheriff to enforce payment of taxes within the county). Presumably, the same diurnal cycle, which—under the given transportation conditions—limited the mobility of citizens, put limits on the movement of these officials as well. As the settlement area of a nominally large county expanded beyond these limits, it would be to the advantage of the county officials to recommend creation of new counties to serve the outlying settlements. There is evidence (Porter, 1947; Updyke, 1913; James, 1921; Fairlie and Kneier, 1930) that the earliest counties—those created during the colonial period and which formed the basis for all subsequent development of county government—were created precisely because of the difficulty of administering justice from the General Courts of the Colonial capitals under existing conditions of transportation.

#### *A Formal Model of Governmental Effectiveness*

The problem of administering justice over a large area under limiting conditions of transportation has recently been given some attention by Boulding (1968:1111–1123) and Stinchcombe (1968:158–163 & 216–231). Their ideas are substantially iden-

tical though their terminologies vary somewhat. Together, their notions provided the basis for construction of the following model of governmental effectiveness—a model which gives graphic representation to the theory of segmental growth and which, at the same time, affords an opportunity to view counties as *governmental* forms of social organization.

A government is effective to the extent that it can control the behavior of its citizens in all relevant respects, that is, to the extent that it can enforce its regulations within its jurisdiction, over local opposition if need be. It should be noted “effectiveness” does not refer to the adequacy—moral or otherwise—of governmental regulations; the only reference here is to the government’s ability to enforce its regulations, whatever they may be. The probability that a regulation will be backed up by force if necessary, according to Stinchcombe (1968: 162), is a measure of the *legitimacy* of that regulation. Boulding (1968:1112) referred to a government’s ability to thus “legitimize” its regulation as the *threat capability* of the government; the degree to which people believe in a government’s threat capability he called its *threat credibility*. The importance of this last concept lies in the fact that, in order to regulate the behavior of individuals under normal circumstances, a government’s threat capability must enter into individual decision-making as a subjective belief.

Threat capability, Boulding argues, has a cost of transport which increases as a function of the distance over which that capability must be moved. Thus threat capability diminishes with distance from the origin of the threat. And, since threat capability and credibility are related (Boulding, 1968: 1123n), threat credibility also diminishes with distance from the origin of the threat. This decline in threat credibility per unit of distance from the origin represents the government’s *loss of strength gradient*. At a certain distance threat credibility will decline to the point where it can no longer control behavior. The amount of threat credibility which is just sufficient to control behavior for governmental purposes may be called its *critical value*. Stinchcombe (1968: 226) refers to those areas in which threat credibility (his word is “vulnerability”) is

lower than its critical value as *bush, wilderness, or frontier*.

Figure 1A is my attempt to give graphic representation to the Boulding-Stinchcombe model. In the Figure, S is the distance from O, the origin of the threat, T is the threat credibility of the government located at O. The critical value M occurs on the loss of strength gradient QR at the distance ON. It has been assumed that there is some distance PQ within which threat credibility does not decline, reflecting the possibility that the origin of the threat may be an area rather than a point. If we let L represent the nominal boundary of the government at O, then Figure 1A describes the situation in which a government claims jurisdiction over areas which it cannot govern, namely, the frontier NL. It may be noted that although the Figure is constructed on the basis of linear distance, it can be made to represent two-dimensional areal patterns by rotating the S-axis about the origin (and with it, PQR, L, M, and N).

Now suppose that the population, which

is nominally under the jurisdiction of the government at O, gradually expands its settlement area into the frontier NL. Under the assumed conditions, only two alternatives are available for providing effective government in the frontier. Either the seat of government must be relocated, or a second government will have to be created for those in the frontier. As Pierson (1968: 51) has suggested, however, institutions do not move easily; and, in any event, there are limits to the application of this alternative—the frontier absorbed by the government would be matched by the frontier created in the area newly removed from governmental control (i.e., on the “negative side” of the S-axis, not shown). It seems reasonable to expect, then, that the second alternative will be the more probable: a new government will be created as the population moves into the frontier.

Figure 1B illustrates this condition.  $Q_1R_1$  is the loss of strength gradient for the government located at  $O_1$  as in Figure 1A.  $Q_2R_2$  is the loss of strength gradient for the newly created government located at  $O_2$  (it rotates, for areal patterns, about the  $O_2P_2$  axis). The area represented by  $N_2N_1$  would be what Stinchcombe (1968:226) called *disputed border territory*, except that the boundary  $L_{12}$  has been created. To the extent that the boundary is legitimate, in the sense indicated above, it has the effect of reducing threat credibility to zero, regardless of the overlapping of objective threat capabilities. This can be seen by comparing  $Q_1R_1$  with  $Q_1KL_{12}$ . The original boundary L in Figure 1A has now become the boundary  $L_2$  for the government at  $O_2$ . The originally large jurisdiction  $O_1L_2$  has been reduced to the small jurisdiction  $O_1L_{12}$  by the creation of a second government at  $O_2$  following the movement of population into the new settlement area.

Returning to Figure 1A, suppose that something happens which reduces the cost (particularly the time-cost) of transporting threat capability. The effect of such a reduction would be to make the loss of strength gradient less steep, so that the critical value of threat credibility would lie further from the origin of the threat than it did when transport costs were higher. This condition is illustrated in Figure 1C, in

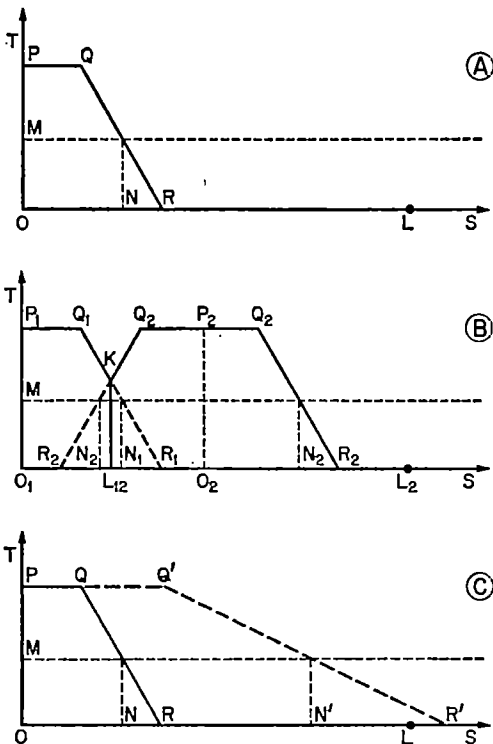


FIG. 1: Graphic Representation of the Boulding-Stinchcombe Model of Governmental Effectiveness.

which  $Q'R'$  represents the new loss of strength gradient. The distance  $ON'$  defines the area which can now be governed effectively from the origin  $O$ , and the area defined by  $NN'$  has been subtracted from the frontier. Thus, the movement of population into  $NN'$  would no longer necessitate the creation of a new government.

We can illustrate the Durkheim-Boulding-Stinchcombe model for a hypothetical case. Figure 2A shows a hypothetical state boundary with several smaller counties already formed in the southeastern corner of the state. County seats are indicated by "+" marks; settled areas are indicated by shading. At this point in time there is considerable variation in county size, produced by the very large counties to the north and west. In Figure 2B, the settlement area has expanded into the frontier, beyond the range of governmental effectiveness of existing counties; those living in the outlying settlement area, though nominally citizens of existing counties, are without an effective county government. This situation is remedied in Figure 2C with the creation of new counties in the outlying area. As population expands further, still more counties will be created, unless improvements in transportation no longer make this necessary. The result will be more or less variation in county size, depending upon the extent of settlement and the date of transportation improvement.

#### *Report of Research Findings*

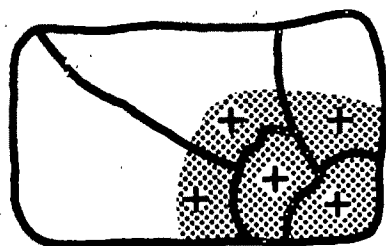
We turn from the hypothetical case to actual historical cases with this general hypothesis: variation in county size within a state (i. e., the presence of large counties in addition to the more typical small counties) is a function of the extent to which a state was settled prior to the general improvement in transportation which occurred with the introduction of the automobile around 1930. If the area of a state was completely settled prior to 1930, no large counties will remain; the state's counties will be uniformly small. If the state was but partially settled by that time, considerable variation in county size, produced by the remaining large counties, will be present. Furthermore, prior to 1930, where variation in county size is present within a state, the

smaller counties will be in the areas of earlier settlement.

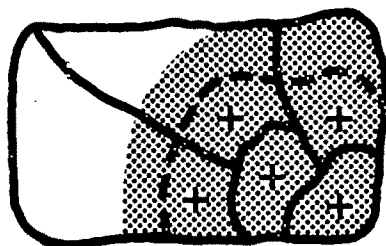
Testing the empirical accuracy of these expectations was relatively simple, compared to the task of reporting the results. Ideally, such a report would consist of a series of maps (a filmstrip?) showing all changes in the location of county boundaries and all changes in the distribution of population, from the earliest days of the colonial period up to the present. Obviously, such a series of maps, even if they were available, cannot be reproduced here (though a number of such maps are shown in Stephan, 1970a:61-92). For this reason, and for the reason that the requisite data are not fully available, the present report will fall far short of the ideal.

Data on the distribution of population

### A. INITIAL CONDITIONS



### B. SETTLEMENT EXPANSION



### C. SEGMENTAL GROWTH

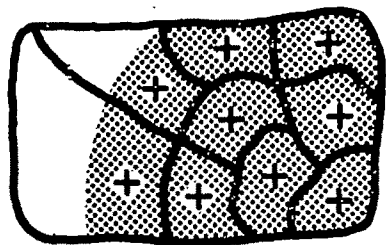


FIG. 2. Illustration of the Boulding-Stinchcombe model applied to a Hypothetical Case

during the colonial period, from 1625-1790, are available (Friis, 1968), but the earliest year for which there is a map of county boundaries in the United States (cf. Friis, 1968) is 1790. This map, based upon those produced by Rossiter (1909), is reproduced in a slightly modified form in Figure 3. It was modified to this extent: for the state of South Carolina, parish boundaries have been used, instead of county boundaries. The reason for this is that South Carolina had no counties, nominally, until the Reconstruction Period. In 1790, parishes performed the functions which elsewhere were performed by counties; hence, it seemed appropriate to indicate their boundaries (from Wallace, 1961:166) in Figure 3.

From 1790 to the present, data on population distribution has, of course, been made available by the Bureau of the Census (convenient compilations in cartographic form may be found in Paullin, 1932, or Lord and Lord, 1953). But maps of county boundaries in the United States are not available between 1790 and 1840. For this period one must rely on the maps of individual states, maps which lie scattered throughout numerous journals of state and local history

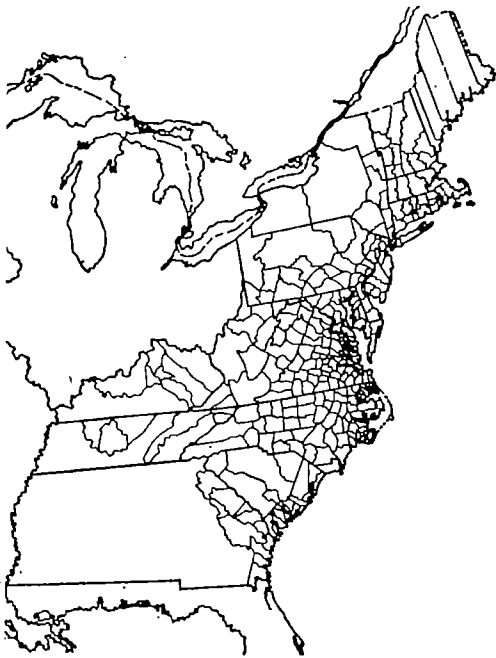


FIG. 3: Location of County Boundaries in the United States in 1790 (Adapted from Friis, 1968).

and which, together, still do not form a complete set. From 1840 to the present, however, maps are available for every census year, by special order from the Census Bureau.

The maps thus obtained, limited as they are, afford ample opportunity to test the adequacy of the hypotheses derived from the Durkheim-Boulding-Stinchcombe model. Referring again to Figure 3, the population maps (which cannot be reproduced here) make it clear that the heaviest concentrations of population lie in the areas along the Atlantic Coast with the smaller counties. Moving westward, the counties become larger as the population becomes less concentrated. The two smaller counties in the center of Kentucky are located at the site of a new settlement nucleus; the large counties in northwestern Pennsylvania and New York, and in northern Vermont and New Hampshire, reflect an almost total lack of settlement; Maine is settled only along the coast. By 1840, almost all of this area has been settled and very few larger counties remain. The process of subdivision has spread into the south and midwest, reflecting again the hypothesized relation between county size and population concentration. By 1870, smaller counties have been formed in the settled areas of Washington, Oregon, and California; around the settlement nuclei of Salt Lake City, Denver, and Santa Fe; and in the settled areas of northern Florida, southern Minnesota, eastern Kansas, Nebraska, and Texas. By 1900 all the states show some areas of population concentration coinciding with their areas of smaller counties; and those states which are not fully settled show larger counties in the areas which remain unsettled.

The situation in 1930, at the time the automobile became widely available, is described in the summary shown in Table 2. For purposes of tabulation, a "large" county was defined as any county of more than 2,000 square miles (see Anderson, 1945: 24). Similarly, "settlement" was defined by a population concentration of at least six persons per square mile. Any state which, in 1930, showed *no* unsettled area under this definition was classified as "completely settled." Those which showed *any* unsettled areas within their boundaries were classified

Table 2. Extent of Settlement (6 or More Persons per Square Mile) As a Determinant of County Size Variation.

Large Counties (2,000 or More Square Miles)	Extent of Settlement	
	Complete	Partial
Some Present	0	20
None Present	25	3

as "partially settled." The latter category included Arizona, California, Colorado, Florida, Idaho, Kansas, Louisiana, Maine, Michigan, Minnesota, Montana, Nebraska, Nevada, New Mexico, New York, North Dakota, Oklahoma, Oregon, South Dakota, Texas, Utah, Washington and Wyoming. Of these, only three—Kansas, Louisiana, and Michigan—did not have "large" counties (though each of them had at least ten counties of 1,000 square miles or more).

In the years following 1930, the area of "settlement" continued to expand (some previously settled areas, incidentally, became "unsettled" through rural depopulation). Yet, as Table 1 makes clear, the process of division, which until 1930 had produced new counties as new areas were settled, came to an end. The relation between population distribution and county size—a relation which seems to have obtained for three centuries—broke down under the influence of the automobile. If change in a social system can be described in terms of the creation, transformation, or extinction of *relationships* between social variables (Stephan, 1970b: 225), then the case reported here is certainly a striking instance of it.

#### *Reconsideration of Durkheim*

But is this the kind of change which Durkheim anticipated in his *Division of Labor*? Has the introduction of the automobile led to the "effacement of the segmental type"—to the elimination of counties as a form of social organization? In two states, Connecticut and Rhode Island, apparently it has; they have abolished counties as units of local government (United States, 1967:15), although county *areas* remain for purposes of statistical enumeration.

This amounts to consolidation at the state level. In two analogous units of segmental organization—the parishes of rural churches and the districts of rural school systems—there has been considerable territorial consolidation attributed, respectively, to the automobile and the school bus (Rogers, 1960:212 and 235). And there evidently has been considerable *pressure* for county consolidation since the introduction of the automobile (cf. Porter, 1922; Kilpatrick, 1930; Bromage, 1933; Lancaster, 1937; Anderson, 1945; Wager, 1950; Rogers, 1960). Whether or not this pressure will be successful in the future is not deducible from the model developed here (it does not, for example, take into account the political self-interest of county officials and employees, or the inertia of beliefs and values which Ogburn labelled "cultural lag").

What is clear, however, is that there are alternatives to the "effacement of the segmental type"—alternatives which Durkheim did not specify in his theoretical formulation. For one, there is simply the *termination of segmental growth* (the condition holding most generally throughout the United States since the 1930s). For another, there is *enlargement of the segmental units* (which would be the case if small counties were simply consolidated into larger counties). Neither of these alternatives necessarily imply "effacement of the segmental type." Neither do they result in the development of functional specialization—division of governmental labor—at the state level. Under either alternative, counties remain as social realities in the collective consciousness.

#### *Summary and Conclusions*

The research reported here was intended to explain county size variation in the United States. The basis of the explanation was Durkheim's theory of segmental growth. Implicit in his *Division of Labor*, the theory was given explicit formulation in this report; it was also shown to be related to certain geopolitical theories developed by Boulding and Stinchcombe, theories which suggested the model of governmental effectiveness developed here. The assumptions underlying application of the Durkheim-Boulding-Stinchcombe model seem to be: (1) that the

functions of county government (which remained essentially the same from the beginning—cf. Porter, 1947:1) occasionally required the *movement* of people between the county seat and their residences in the county; (2) that the diurnal cycle imposed *time* limits on such movement; (3) that the existing horse-and-buggy transportation technology imposed limits on the *velocity* of such movement; and (4) that these time and velocity limits determined the *distance* over which such movement could occur, thereby setting limits on the *area* which a county seat could serve. The resulting hypothesis was that new counties would be created as the settlement area expanded beyond the range of governmental effectiveness of existing counties—until the introduction of the automobile terminated the division process, producing the variation in county size which is presently observable. The data reported (and referred to) confirmed this expectation, and the theory of segmental growth appears to have been supported.

It is interesting to note in conclusion that, at the outset of this research, there was no assurance that the segmental growth theory would hold. For example, in writing to the Census Bureau for the requisite county outline maps, the author mentioned that he hoped to develop a "demographic-ecological" explanation of county size variations. The Chief of the Geography Division (in a letter to me dated April 4, 1969) expressed the following opinion: "For whatever it is worth to you, it is our opinion that the explanation of county boundaries may be more political than demographic-ecologic." He was referring, of course, to a fact well-attested in the state history journals—that the *precise* location of county boundaries has often been an object of political manipulation (for example, in the interest of prominent landowners, or for purposes of representation in state legislatures). This difference of opinion actually reflects a more fundamental difference of perspective. To the historian, or the geographer perhaps, the object of investigation may properly be to determine the unique set of circumstances which surrounded the precise location of a particular boundary (e.g., the political anecdote which explains its location). The sociological perspective, on the other hand, is oriented

specifically toward asking questions about *generalized patterns* of human behavior. In the present case, the pattern involved over 3,000 counties and took some 300 years to unfold; it encompassed a vast area comprising considerable local variation in physical and social milieu. It is a particular advantage of the sociological perspective that it enables us to ask questions which cut through variations in local conditions, to discover the general patterns which lie hidden under the masks of geographic and historical accident.

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## BUREAUCRATIC MAN: A PORTRAIT AND AN INTERPRETATION<sup>1</sup>

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*American Sociological Review* 1971, Vol. 36 (June):461-474

*There is a small but consistent tendency for men who work in bureaucratic organizations to be more intellectually flexible, more open to new experience, and more self-directed in their values than are men who work in nonbureaucratic organizations. This may in part result from bureaucracies' drawing on a more educated work force. In larger part, though, it appears to be a consequence of occupational conditions attendant on bureaucratization—notably, far greater job protections, somewhat higher income, and substantively more complex work.*

It is often asserted that bureaucracy makes for unthinking, literalistic conformism. So self-evidently correct does this view seem that Webster's Third New International Dictionary defines bureaucracy as, among other things, "a system of administration marked by . . . lack of initiative

and flexibility, by indifference to human needs or public opinion, and by a tendency to defer decisions to superiors or to impede action with red tape." Moreover, there is plausible theoretical reason why bureaucracy should have such effects. As Merton (1952) pointed out, the social psychological corollary of the efficiency, rationality, and predictability that Weber prized in bureaucratic organizational practice must be a certain "overconformity" in the behavior of bureaucrats.

But does working in a bureaucracy merely

<sup>1</sup> I am indebted for critical advice and essential help to my associates: Carmi Schooler, Lindsley Williams, Elizabeth Howell, Margaret Renfors, Carrie Schoenbach, and John Westine; and, for carrying out the survey on which the research is based, to Paul Sheatsley, Eve Weinberg, and the staff of the National Opinion Research Center.

make automatons of men, or are there compensating features that encourage individualistic qualities? Surprisingly, there has been little empirical study of how bureaucracy affects those who spend their working hours in its employ.<sup>2</sup> One objective of this paper is to ascertain whether "bureaucrats" really are conformist in their values and in their appraisal of social reality, resistant to innovation and change, literalistic in their moral judgments, and inflexible in their thinking.

A further and more important objective is to discover *how* bureaucratization exerts its social psychological impact, whatever that impact may be. The overall structure of the organization can matter, for those who work there, only through its impact on occupational conditions that bear directly on men's lives—"proximate" occupational conditions, such as closeness of supervision, time pressure, and the substantive complexity of the work. Bureaucratization affects many of these conditions, but most discussions seem to have arbitrarily focused on only one. In this inquiry, we attempt a systematic examination of the many occupational concomitants of bureaucratization to see which ones contribute to its impact.

We are not here concerned with the efficiency of bureaucratic organization for getting things done, with the effect of the system on outsiders who must deal with it, nor even with the adequacy of bureaucrats' performance in their occupational roles. We deliberately limit our attention to how (and why) the experience of working in a bureaucracy affects men's values, social orientation, and intellectual functioning.

#### RESEARCH METHODS

The research is based on interviews with 3101 men, representative of all men through-

<sup>2</sup> The few relevant studies give apparently contradictory results. Whyte (1956) argued that large corporations co-opt junior executives, to the detriment of their individuality; and Miller and Swanson (1958) found that bureaucratization leads parents to de-emphasize self-reliance in favor of accommodation as values for children; but Blau (1955:183-200) found that the job securities provided to the employees of a government bureaucracy generate favorable attitudes toward change; and Bonjean and Grimes (1970) failed to find any consistent evidence to support the hypothesized relationship between bureaucracy and alienation.

out the United States employed in civilian occupations. These interviews were conducted for us by the National Opinion Research Center in the spring and summer of 1964 (cf. Sudman and Feldman, 1965, for a general description of the sampling methods). About half the interview questions were directed to job, occupation, and career, the remainder to background information, values, and orientation.<sup>3</sup>

#### *Bureaucracy: Definition and Index*

Our conception of bureaucracy is derived from Weber's classic analysis (cf. Gerth and Mills, 1946:196-244; Weber, 1947:329-341). As summarized by Merton (1952:362), the main characteristics of bureaucratic structure are that: "[B]ureaucracy involves a clear-cut division of integrated activities which are regarded as duties inherent in the office. A system of differentiated controls and sanctions is stated in the regulations. The assignment of roles occurs on the basis of technical qualifications which are ascertained through formalized, impersonal procedures (e.g., examinations). Within the structure of hierarchically arranged authority, the activities of 'trained and salaried experts' are governed by general, abstract, clearly defined rules which preclude the necessity for the issuance of specific instructions for each specific case."

In this study, we are able to index only one of these several dimensions of bureaucracy, the hierarchical organization of authority. This limitation is the price we pay for a sample sufficiently large and diverse to permit a systematic assessment of the occupational conditions attendant on bureaucratization. In studying a multitude of organizations, we cannot assess their structure at firsthand, but must rely on reports by the men who work there, some of whom know little about their employing organization except insofar as it directly impinges on them. The one facet of organizational structure that necessarily impinges on all men is authority.

How much is lost by our inability to index other dimensions is uncertain, because there

<sup>3</sup> For the full interview schedule, and further information on sample and research design, cf. Kohn, 1969:235-264.

is contradictory evidence as to whether or not bureaucracy's several dimensions are highly intercorrelated (cf. Hall, 1963; Hall and Tittle, 1966; Miller, 1970). But even the research that finds low intercorrelations concludes that the hierarchical organization of authority "may be the central dimension . . . of the overall degree of bureaucratization" (Hall, 1963:37). Moreover, hierarchy of authority is conceptually central: bureaucracies can operate with greater or lesser specialization, with more or less impersonality, with or without a multiplicity of codified rules and procedures, but it is basic to the very idea of bureaucracy that authority be hierarchically organized (cf. Gerth and Mills, 1946:197; Rheinstein, 1954:336-337; Blau, 1970:203).

We treat the hierarchical organization of authority as operationally equivalent to the number of formal levels of supervision. Although many men do not have a comprehensive view of the authority structure of the firm or organization in which they work, even the man at the bottom of the hierarchy knows whether his boss has a boss and whether that man is the ultimate boss. So we asked the respondents: "Is this [an organization] where everyone is supervised directly by the same man, where there is one level of supervision between the people at the bottom and the top, or where there are two or more levels of supervision between the people at the bottom and the top?"<sup>4</sup>

To distinguish further, we assume that when an organization reaches at least three levels of formal authority, greater differentiation of structure is roughly proportional to size, at least in organizations of about 100 to 1,000 employees—an assumption given some empirical support by Blau's (1970:205) recent work. (We cannot make the same assumption about organizations of

more than 1,000 employees; nor would we even trust respondents' estimates of size when the number surpasses 1,000.) Thus, the index of bureaucratization is as follows: (1) One level of supervision; (2) two levels of supervision; (3) three or more levels of supervision, fewer than 100 employees; (4) three or more levels of supervision, 100-999 employees; and (5) three or more levels of supervision, 1,000 or more employees.

This index makes explicit what is in any case implicit: that it is impossible to index hierarchical structure without also indexing size—their correlation (0.71) is too great. For now, we simply accept as empirical fact that bureaucratization implies large size; later we shall attempt to separate the consequences of bureaucratic structure, as such, from those of size.

#### *Indices of Values, Social Orientation, and Intellectual Functioning*

*Valuation of Conformity.* By values, we mean standards of desirability—criteria of preference (cf. Williams, 1960:402-403). Specifically pertinent here, because of the assertion that bureaucracy breeds conformism, is men's relative valuation of self-direction or of conformity to external authority. The index is based on a factor analysis of the men's rankings of the relative desirability of a number of generally valued characteristics.<sup>5</sup> Self-direction, as thus indexed, means regarding as most desirable such characteristics as curiosity, good sense and sound judgment, and the ability to face facts squarely; valuing conformity means giving priority to respectability.

*Social Orientation.* At issue here is whether bureaucratization is conducive to intolerance of nonconformity, literalism in moral positions, and resistance to innovation and change. We attempt to measure all three, by means of the following indices:<sup>6</sup>

<sup>4</sup> Men who told us that they work in a firm or organization employing fewer than ten people were not asked about supervisory structure, because pretest interviews had shown these men to take it so completely for granted that there can be only one boss that they find such questions baffling. We simply assume that such firms have only one level of supervision. Correspondingly, we assume that firms or organizations employing as many as five hundred people must have at least three levels of formal authority.

<sup>5</sup> For the derivation of this index, cf. Kohn, 1969:73-75, or Kohn and Schooler, 1969:664-666. This and subsequent factor analyses are orthogonal principal component factor analyses, rotated to simple structure through the varimax procedure, based on the computer program developed by Clyde *et al.* (1966:15-16). The procedure used for calculating factor scores (cf. Ryder, 1965) preserves the orthogonality of the factors.

<sup>6</sup> The indices that follow are based on a factor

(1) "Authoritarian conservatism," that is, men's definition of what is socially acceptable—at one extreme, rigid conformance to the dictates of authority and intolerance of nonconformity; at the other extreme, open-mindedness. It is indexed by agreement or disagreement with such assertions as: The most important thing to teach children is absolute obedience to their parents. Young people should not be allowed to read books that are likely to confuse them. There are two kinds of people in the world: the weak and the strong. People who question the old and accepted ways of doing things usually just end up causing trouble.

(2) "Criteria of morality," by which we mean a continuum of moral positions, from believing that morality consists of strict adherence to the letter of the law, to holding personally responsible moral standards. This dimension is indexed by answers to the question, "Do you believe that it's all right to do whatever the law allows, or are there some things that are wrong even if they are legal?" and by agreement or disagreement with such assertions as: It's all right to do anything you want as long as you stay out of trouble. If something works, it doesn't matter whether it's right or wrong. It's all right to get around the law as long as you don't actually break it.

(3) "Stance toward change," that is, men's receptiveness or resistance to innovation and change. It is indexed by responses to the questions: "Are you generally one of the first people to try out something new, or do you wait until you see how it's worked out for other people? Are you the sort of person who takes life as it comes, or are you working toward some definite goal?" and by agreement or disagreement with: It generally works out best to keep on doing things the way they have been done before.

*Intellectual Functioning.* The most serious charge against bureaucracy is that it inhibits men's readiness to think for themselves. As one test of this assertion, we have measured men's intellectual flexibility, evidenced in several appraisals of actual performance deliberately built into the inter-

view. These include cognitive problems that require weighing both sides of an economic or a social issue; a test involving the differentiation of figure from ground in complex color designs; and a test of men's ability to draw a recognizably human figure whose parts fit together in a meaningful whole.<sup>7</sup> We also asked the interviewers to evaluate each respondent's "intelligence"; and we did a simple count of the respondent's propensity to agree with agree-disagree questions. All these we take to reflect, in some substantial part, intellectual flexibility.

As a single index, we use scores based on a factor analysis of these diverse measures. We also extracted from these same data two rotated factors, which provide measures of two distinct aspects of intellectual functioning.<sup>8</sup> One is "perceptual," based primarily on inferences from the figure-drawing and form-perception tests. The other is "idea-

<sup>7</sup> Specifically, we asked: "Suppose you wanted to open a hamburger stand and there were two locations available. What questions would you consider in deciding which of the two locations offers a better business opportunity?" and "What are all the arguments you can think of for and against allowing cigarette commercials on TV? First, can you think of arguments for allowing cigarette commercials on TV? And can you think of arguments against allowing cigarette commercials on TV?" The perceptual test consists of a portion of Witkin's (1962) Embedded Figures Test, selected by Witkin. The Figure-drawing Test (cf. Witkin 1962:117-129) consists simply of asking the respondent to draw a figure of a man on a standard-size card with a standard pencil. Respondents are reassured that artistic ability is not required. The meaningful coherence of the figure is what we appraise.

<sup>8</sup> The factor loadings for the single index are: hamburger-stand problem, 0.37; cigarette-commercials problem, 0.41; Embedded Figures Test, 0.67; Draw-A-Man Test: summary score, 0.75; Goodenough estimate of intelligence, based on the Draw-A-Man Test (cf. Witkin, 1962:127-129), 0.78; interviewer's rating of respondent's intelligence, 0.60; "agree" score, 0.52. In the two-factor solution, the first factor is based primarily on the Draw-A-Man Test (summary score, -0.91; Goodenough estimate of intelligence, -0.91) and the Embedded Figures Test (-0.43). The second factor is based on the interviewer's appraisal of the respondent's intelligence (0.69), the cigarette-commercials problem (0.61), the respondent's "agree" score (-0.56), the hamburger-stand problem (0.54), and the Embedded Figures Test (0.52).

analysis of a set of 57 questions, mainly of the "agree-disagree" and "how often?" types. For the derivation of these indices, cf. Kohn, 1969:265-269, or Kohn and Schooler, 1969:666-669.

solving and in impressing the interviewer as being an intelligent person.

One final index examines the demands men put on their intellectual resources, no matter how great or limited those resources may be. This index, based on a factor analysis of questions about a wide range of leisure-time activities, focuses on how intellectually demanding are those activities. The relevant factor contrasts spending a large amount of one's leisure time watching TV and reading popular magazines, with engaging in such intellectually active pursuits as going to museums and plays, reading books, and working on hobbies.<sup>9</sup> Some of the latter activities are facilitated by education and income; that will be taken into account in our analyses.

#### THE SOCIAL PSYCHOLOGICAL CONCOMITANTS OF BUREAUCRATIZATION

The most difficult problem in assessing the social psychological impact of bureaucratization is deciding who to compare with whom. Who are bureaucrats? The narrowest definition would be that they are the higher, nonelective officialdom of government. But there is reason and ample precedent to expand that definition to include all employees of all organizations that are bureaucratic in structure—blue-collar as well as white-collar workers, employees of profit-making firms and nonprofit organizations as well as government. To whom should bureaucrats be compared—entrepreneurs, employees of nonbureaucratic organizations, or both? Again, the answer is not self-evident.

Rather than make *a priori* decisions, we prefer to deal with these questions empirically. We begin at the simplest descriptive level, seeing what relationship there may be between bureaucratization, as we have indexed it, and those aspects of values, orientation, and intellectual functioning on which it has been thought to bear.<sup>10</sup> No man employed in a civilian occupation is excluded

from this analysis, whether he is employee or entrepreneur, whether he works in a profit-making firm, a nonprofit organization, or a governmental agency.

The correlations<sup>11</sup> of bureaucratization with values, orientation, and intellectual functioning are small, ranging from only 0.05 to 0.17; they are nonetheless impressive, because they consistently contradict preconception (Table 1). Men who work in bureaucratic firms or organizations tend to value, not conformity, but self-direction. They are more open-minded, have more personally responsible standards of morality, and are more receptive to change than are men who work in nonbureaucratic organizations. They show greater flexibility in dealing both with perceptual and with ideational problems. They spend their leisure time in more intellectually demanding activities. In short, the findings belie critics' assertions.

Now we can consider what difference it makes if the definitions of bureaucrat and nonbureaucrat be altered.

*Ownership.* Many discussions of bureaucracy assume or assert that the antithesis of the bureaucrat is the entrepreneur. We find, to the contrary, that entrepreneurs are remarkably similar to bureaucrats, particularly to bureaucrats of comparable occupa-

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by Cramer. The test of statistical significance is the F-ratio. We are treating all dependent variables in these analyses as interval scales (for justification, cf. Blalock, 1964:94; Cohen, 1965; Labovitz, 1967).

For more general discussions of the logical bases of the statistical procedures we have used, cf. Blalock, 1960, Chapters 15–21; Blalock, 1964; Cohen, 1968.

<sup>11</sup> The measure of association that we use is *eta*, the square root of the ratio of the "between groups" sum of squares to the total sum of squares, as calculated in an analysis of variance. When one deals with the linear component of the independent variable, *eta* is identical to the product-moment correlation coefficient, except that its sign is always positive—the direction of relationships can be determined only by an examination of the means. (Cf. Blalock, 1960:266–269; Cohen, 1965; Peters and Van Voorhis, 1940:312–324 and 353–357.)

It is apparent in Table 1 (and has been confirmed in more detailed analyses) that the *etas* for the linear component of bureaucratization—in its relationships with the several aspects of values, orientation, and intellectual functioning that we have investigated—are nearly as large as those for all components. It is therefore appropriate to treat our index of bureaucratization as essentially linear.

<sup>9</sup> The factor loadings are: frequency of visits to plays, concerts, and museums (–0.60); number of books read in the past six months (–0.54); time spent working on hobbies (–0.35); amount of magazine reading (0.61); and time spent watching television (0.35).

<sup>10</sup> This and subsequent analyses of variance are based on the computer program initially developed by Clyde *et al.* (1966:20–28) and further developed

Table 1. Mean Scores for Values, Social Orientation, and Intellectual Functioning, by Index of Bureaucracy (Total Sample).

Index of Bureaucracy	Values and Orientation					Intellectual Functioning				
	Valuation of self-direction/conformity (+conformist)	Authoritarian conservatism (+authoritarian)	Criteria of morality (+personally responsible)	Stance toward change (+receptive)	Perceptual component of intellectual flexibility (+flexible)	Ideational component of intellectual flexibility (+flexible)	Overall index of intellectual flexibility (+flexible)	Intellectual demand-ness of leisure-time activities (+demanding)	Canonical Correlation <sup>a</sup>	Number of cases <sup>b</sup>
One level of supervision	1.01	0.91	-0.59	-0.57	-1.47	-0.74	-2.17	-0.49		(840)
Two levels	-0.12	0.16	-0.63	0.01	-0.69	-1.06	-2.34	-1.22		(121)
Three+:										
1-99 <sup>c</sup>	0.23	0.12	0.50	0.33	0.68	-0.03	0.15	-0.32		(307)
100-999	-0.97	-0.36	0.08	0.13	0.26	1.07	0.54	-0.07		(456)
1000+	-0.61	-1.29	0.66	0.63	1.20	1.82	1.78	1.35		(1023)
Correlation Ratio ( $\eta^2$ ):										
Linear component	.07**	.09**	.05**	.05*	.11**	.11**	.17**	.08**	.17**	
All components	.08**	.09**	.06	.05	.11**	.12**	.17**	.09**	.17**	

<sup>a</sup>The canonical correlation excludes the overall index of intellectual flexibility, because it is a linear function of the two component indices.

<sup>b</sup>354 respondents for whom data are incomplete are excluded from this and subsequent tables.

<sup>c</sup>Employees

\* $p < .05$ , \*\* $p < .01$ .

tional status. (The one notable difference is that bureaucrats are more intellectually flexible—another refutation of stereotype.) The real contrast is not between bureaucrats and entrepreneurs, but between both these groups and the employees of nonbureaucratic organizations.

To properly assess the effects of bureaucratization, we must limit the analysis to employees, comparing the employees of bureaucratized organizations to those of nonbureaucratic organizations. Thus limiting the analysis strengthens the contrast between bureaucrats and nonbureaucrats (Table 2). Most of the correlations are increased in magnitude, the overall (canonical)<sup>12</sup> cor-

relation increasing by one-third. The picture of the bureaucrat as self-directed and intellectually flexible becomes a little more sharply etched.

*Sector of the Economy.* Just as the entrepreneur has been thought to be the antithesis of the bureaucrat, the government official is usually thought to be its prototype. In fact, employees of government (and of nonprofit organizations) do exemplify the social psychological characteristics associated with bureaucratization: they are more tol-

eration of one or a set of independent variables with a set of dependent variables. More precisely, it is the maximum correlation between linear functions of the two sets of variables (cf. Cooley and Lohnes, 1962:35).

<sup>12</sup> The canonical correlation is a multiple cor-

Table 2. Correlations of Bureaucracy with Values, Social Orientation, and Intellectual Functioning, Selected Samples.

Correlations <sup>a</sup> of Bureaucracy and			Employees of Profit-making Firms				
	Total Sample	Em- ployees	All	White- Collar	Blue- Collar	Controlling Ed. Work force <sup>c</sup>	
Values and orientation:							
Valuation of self-direction/conformity	.07**	.09**	.08**	.07*	.09**	.07**	.06*
Authoritarian conservatism	.09**	.09**	.07**	.08*	.00	.00	.01
Criteria of morality	.05**	.11**	.11**	.00	.15**	.09**	.07**
Stance toward change	.05*	.09**	.10**	.07*	.09**	.07**	.06*
Intellectual functioning:							
Perceptual component of intellectual flexibility	.11**	.09**	.03**	.08*	.06*	.05	.03
Ideational component	.11**	.17**	.13**	.14**	.14**	.11**	.10**
Overall index of intellectual flexibility	.17**	.19**	.19**	.17**	.13**	.11**	.09**
Intellectual demandingness of leisure-time activities	.08**	.15**	.15**	.07*	.13**	.08**	.09**
Canonical correlation <sup>b</sup>	.17**	.23**	.24**	.19**	.23**	.17**	.16**
Number of cases	(2747)	(2268)	(1855)	(702)	(1140)	(1855)	(1807)

<sup>a</sup>Eta or the canonical correlation for the linear component of bureaucracy.

<sup>b</sup>The canonical correlation excludes the overall index of intellectual flexibility (see note to Table 1).

<sup>c</sup>Education, region, size of community, race, national background, religion, and rurality of childhood.

\* $p < .05$ , \*\* $p < .01$ .

Note: Mean scores are not presented in this table. The direction of all relationships is the same as indicated by the mean scores in Table 1.

erant of nonconformity, have more personally responsible moral standards, evidence greater flexibility in dealing with ideational problems, and make more intellectually demanding use of their leisure time than do employees of equally bureaucratized profit-making firms.

To examine the impact of bureaucratization separately from that stemming from employment in the public or the private sphere, we further limit the analysis to profit-making firms—the one sector of the economy where there is any substantial variation in conditions of bureaucratization. We find (Table 2) the correlations of bureaucracy with values, orientation, and intellectual functioning to be nearly the same for

employees of profit-making firms as for all employees.<sup>18</sup> Thus, our earlier findings do not simply reflect the bureaucratization of the public sector of the economy, for bureaucracy's influence extends to the private sector as well.

<sup>18</sup>It could hardly be otherwise, since employees of profit-making firms constitute two-thirds of the total sample. For the same reason, all conclusions drawn from analyses of this subsample apply as well to the sample as a whole. We focus on employees of profit-making firms, not to secure different empirical findings, but for conceptual clarity. There is no advantage in further focusing the analysis on particular types of industries, for bureaucracy has essentially the same social psychological correlates in all types of industry. Moreover, excluding nonurban occupations from the analysis has no noticeable effect.

*Occupational Position.* Many discussions of bureaucracy have been addressed only to its salaried, its white-collar, or its professional staff. It is therefore necessary to see if bureaucratization bears the same relationship to values, orientation, and intellectual functioning for blue-collar as for white-collar employees. To do this, we examine the two groups separately, recognizing that in so doing we partially control variables correlated with occupational position, such as education, the substantive complexity of the work, and the job protections attendant on unionization. Even so (Table 2), bureaucracy's social psychological correlates are similar for blue-collar and for white-collar workers.<sup>14</sup> Moreover, the canonical correlation is as strong for blue-collar as for white-collar workers. Thus, any explanation of the social psychological impact of bureaucratization must apply to the entire work force, not just to the white-collar portion. It is true, of course, that the explanation need not be the same for both groups.

We conclude that bureaucratization bears essentially the same relationship to values, orientation, and intellectual functioning wherever and to whomever it occurs. Why? What is there about working in a bureaucratic organization that makes men more self-directed, open to change, and intellectually flexible?

We must also ask: why are the correlations so small? The comparable correlations for several other aspects of occupation are much stronger (cf. Kohn, 1969:165-182). Does the small size of bureaucracy's correlations imply that we have used an inadequate index, or does bureaucratization have a weaker impact than had been supposed?

#### EXPLAINING BUREAUCRACY'S IMPACT

Bureaucrats and nonbureaucrats are drawn from rather different segments of the population. Bureaucrats necessarily live

where large firms are located: disproportionately in big cities along the Great Lakes and in the Northeastern and Pacific states. Not only are they now urban, but they grew up in urban places. Their forebears are more likely to have come from northern or western Europe than from southern or eastern Europe. Few are black; few are Jews. They are disproportionately Catholic. Those who are Protestant are a little more likely to be members of large, established denominations than of smaller sects. Most notable of all, bureaucrats are more highly educated than are nonbureaucrats.

Of all these differences in the composition of bureaucratic and nonbureaucratic work forces, only education seems to matter for explaining why bureaucrats differ from nonbureaucrats in values, orientation, and intellectual functioning (Table 2). That is, statistically controlling education markedly reduces bureaucracy's impact: the canonical correlation is reduced by nearly 30% and the correlations with authoritarian conservatism and the specifically perceptual component of intellectual flexibility are reduced to statistical nonsignificance. Additionally controlling any or all of the other measures of social background reduces the correlations little more than does controlling education alone. Thus, education is clearly implicated in explaining why bureaucrats differ from nonbureaucrats; but aside from bureaucracies' employing more educated men, the composition of the work force appears to have little explanatory relevance.

Important though education may be, it can provide only a partial explanation of the differences between bureaucrats and nonbureaucrats. That is, even with education statistically controlled, bureaucrats are found to value self-direction more highly than do nonbureaucrats, to have more personally demanding moral standards, to be more receptive to change, to be intellectually more flexible (especially in dealing with ideational problems), and to spend their leisure time in more intellectually demanding activities. The explanation of these differences must lie either in bureaucracies' somehow recruiting more self-directed, intellectually flexible people (a possibility to which we shall return), or in bureaucracies' subjecting their employees to occupational conditions

<sup>14</sup> The index of occupational position is taken from Hollingshead and Redlich, 1958:387-397. For validation of the index, cf. Kohn, 1969:13-15.

It should also be noted that the social psychological correlates of bureaucracy are essentially the same regardless of men's own positions in the supervisory hierarchy (as measured by the number of subordinates they have).



that foster these social psychological attributes.

Bureaucratization does make for wide-spread, and in some instances substantial, differences in the conditions of occupational life, few of them attributable to educational disparities and most of them applicable (although not always in equal degree) to both the white-collar and blue-collar work forces (Table 3). These are, principally, that the employees of bureaucracies tend to work at substantively more complex jobs than do other men of comparable educational level,<sup>15</sup> but under conditions of somewhat closer supervision;<sup>16</sup> to work under an externally-imposed pressure of time that results in their having to think faster; to work a shorter week; to work in company of, but not necessarily in harness with, co-workers; to face greater competition; to enjoy much greater job protections;<sup>17</sup> and to earn more than other men of similar educational background

(even when in jobs of comparable occupational status).

In assessing the possible explanatory relevance of these occupational conditions, we statistically control education throughout<sup>18</sup> and limit the analysis to those social psychological correlates of bureaucracy that remain significant even with education controlled. From one point of view, this procedure gives undue weight to education, which may matter primarily because it is a precondition for certain types of jobs—substantively complex jobs, for example. But since most men come to their jobs only after completing their educations, it is incumbent on us to show that occupational conditions matter above and beyond any effect that might be attributed to education.

Three of the occupational concomitants of bureaucracy—job protections, income, and substantive complexity—prove to have substantial pertinence (Table 4). The combined effect of controlling all three is to reduce the canonical correlation of bureaucracy with values, orientation, and intellectual functioning by two-thirds and to render this and all the individual correlations statistically nonsignificant.

Further analysis, using multiple regression techniques,<sup>19</sup> shows that all three occupational conditions contribute independently to bureaucracy's social psychological impact (Table 4). Job protections contribute notably to the relationship between bureaucracy and men's orientation to morality and to change, and even to the relationship between

<sup>15</sup> The index of substantive complexity is a linear combination of seven constituent indices, which measure the complexity of the man's work with data, with things, and with people, the overall complexity of his job, and the amount of time he spends working with data, with things, and with people. The first three of these indices are modeled on those of the third edition of the *Dictionary of Occupational Titles* (U. S. Department of Labor, 1965). For a discussion of the concept and index of substantive complexity, cf. Kohn, 1969:139-140, 153-155, 271-276.

<sup>16</sup> Closeness of supervision is indexed by a Guttman Scale based on five questions about how much latitude men's supervisors allow and how supervisory control is exercised (cf. Kohn, 1969:153). Note, too, that many employees of bureaucracies are effectively supervised by men who are one or two steps removed from them in formal position.

<sup>17</sup> In particular, bureaucrats are more likely to enjoy the basic job protections given by tenure or by contractual guarantees based on seniority; by the existence of formal grievance procedures; and by sick pay. These correlations are more pronounced for blue-collar than for white-collar workers, and are linked to the greater unionization of the blue-collar work force. Even for non-union and for white-collar workers, though, job protections are decidedly greater in bureaucratic firms. (And they prove to be as important for younger as for older men.)

It is noteworthy that the critical difference bureaucratization makes is not in the *risks* to which men are subject but in the *protections* that their jobs afford. Bureaucrats are no less exposed to occupational risk than are nonbureaucrats, but they are better protected, should those eventualities occur.

<sup>18</sup> Since bureaucrats are more educated than are nonbureaucrats, they are necessarily higher in social class position, too. But we regard education, not class, as the appropriate variable to control in these analyses, because education is what men bring to the job. Other components of class, notably occupational position, are conferred by the job. (In fact, if education is controlled, occupational position has little importance.)

<sup>19</sup> The multiple regression program employed here is that by Nie *et al.* (1968:Chap. 15). To do this analysis, we constructed a single index of job protections by adding the dichotomized scores for job security, grievance procedures, and sick pay, giving each equal weight. For a single index of substantive complexity, we used scores based on a factor analysis of the seven constituent elements. The results shown in Table 4 are based on the standardized beta-coefficients for bureaucracy; analyses using the unstandardized beta-coefficients yield essentially the same conclusions.

Table 3. Correlations of Occupational Conditions and Bureaucracy (Limited to Employees of Profit-making Firms).†

Occupational Conditions	Total <sup>a</sup>	Control- ling Ed.	White Collar	Blue Collar
1. Substantive complexity	.40**	.31**	.37**	.33**
2. Supervision				
a. Closeness of supervision	.02	.05*	.04	.04
b. Positional disparity between man and his effective supervisor	.17**	.18**	.13**	.18**
3. Time-pressure				
a. Frequency of time-pressure	.10**	.05	.10**	.06*
b. Determinants of time-pressure: work-flow, not own volition	.10**	.13**	.12*	.19**
c. Consequences of time-pressure: think faster, not make faster movements or work longer hours	.19**	.14**	.21**	.16**
4. Average hours worked per week	-.28**	-.29**	-.20**	-.29**
5. Interpersonal setting				
a. Time spent in the company of five or more co-workers	.22**	.24**	.11**	.27**
b. Time spent alone	-.12**	-.13**	-.03	-.15**
c. Participation in coordinated work-team	.02	.04	.01	.04
6. Amount of competition	.17**	.13**	.19**	.11**
7. Job protections				
a. Tenure, or seniority guaranteed by union contract	.37**	.42**	.16**	.56**
b. Formal grievance procedures	.39**	.45**	.20**	.57**
c. Sick pay	.33**	.30**	.25**	.34**
d. Job protection: overall index	.50**	.52**	.30**	.60**
8. Income and income fluctuations				
a. Job income	.27**	.21**	.19**	.31**
b. Fluctuations in income	-.04*	-.02	-.07*	.02
9. Routinization of work	.08**	.09**	-.18**	.15**

†N's vary from 1826 to 2059 for the sample of all employees of profit-making firms, from 646 to 749 for the white-collar subsample, and from 1169 to 1289 for the blue-collar subsample.

<sup>a</sup>Eta, or (in the case of 1, 3b, 3c, 7d, and 9) the canonical correlation, for the linear component of bureaucracy. In Column 2, a partial correlation, controlled on education, is reported. Columns 3 and 4 report total correlations, computed for the appropriate subgroups. In lieu of presenting a multitude of mean scores, we have arbitrarily used signs to indicate the direction of relationships.

\*p<.05, \*\*p<.01.

bureaucracy and intellectual flexibility. It thus appears that men who are protected from some of the dangers that change might bring are less fearful of the new and the different, are better able to accept personal responsibility for their acts, and are even able to make fuller use of their intellectual talents. Substantive complexity is more specifically pertinent for explaining bureaucrats' flexibility in dealing with *ideational* problems and their making intellectually demanding use of leisure time. The experience of working at substantively complex jobs

thus appears to have direct carry-over to the use of one's intellectual resources in non-occupational endeavors. And job income is most pertinent for explaining bureaucrats' high valuation of self-direction; the higher income enjoyed by employees of bureaucracies appears to facilitate their feeling sufficiently in control of their lives to think self-direction an attainable goal.

We conclude, then, that job protections, substantive complexity, and income all contribute to, and together may largely explain, the social psychological impact of bureau-

Table 4. Statistical Effects of Controlling the Occupational Correlates of Bureaucracy (Limited to Employees of Profit-Making Firms).

	Partial correlation with bureaucracy, education controlled <sup>a</sup>	Uncontrolled coefficient <sup>b</sup> for bureaucracy	Controlled on educa- tion	Controlled on ed. & all relevant occ. conditions <sup>c</sup>	Job protections	Job income	Substantive complexity (when all other relevant occupational conditions are controlled)	Amount of competition	Time pressure	Hours of work	Interpersonal setting	Closeness of supervision	Job protections, income, & sub- stantive complexity
Values, orientation, & intellectual functioning													
Valuation of self- direction/conformity	.07**	.074	.051	.027	17 (.004)	20 (.011)	14 (.001)	-.06 (-.008)	.08 (.000)	20 (.012)	-.03 (-.005)	-.06 (-.004)	44
Criteria of morality	.09**	.109	.083	.010	50 (.021)	24 (.005)	25 (.008)	-.02 (-.005)	.05 (.000)	12 (.003)	.00 (-.006)	-.08 (-.012)	85
Stance toward change	.07**	.099	.063	.003	39 (.030)	19 (.000)	.08 (.016)	.21 (.009)	.10 (-.001)	-.19 (-.004)	.10 (.004)	.12 (-.006)	80
Ideational component of intellectual flexibility	.11**	.181	.085	.041	24 (.012)	21 (.004)	.29 (.022)	.05 (.002)	.03 (.000)	.01 (.004)	-.06 (-.008)	-.03 (-.005)	64
Overall index of intellec- tual flexibility	.11**	.185	.091	.056	32 (.018)	12 (.002)	.16 (.012)	.05 (.003)	-.05 (-.001)	-.07 (.000)	-.01 (-.007)	-.05 (-.002)	66
Intellectual demandingness of leisure-time activities	.08**	.138	.059	.009	11 (.007)	35 (.011)	.33 (.024)	.00 (-.003)	.05 (-.001)	.01 (.005)	-.01 (-.002)	.05 (.000)	71
Canonical correlation	.17**				29	21	.18	.03	.02	.00	.00	-.01	66
Number of cases	(1855)				(1774)	(1662)	(1823)	(1829)	(1846)	(1762)	(1761)	(1804)	(1569)

<sup>a</sup> From Table 2. In the computations that follow, these partial correlations (etas) have in each instance been recomputed for all respondents for whom the relevant occupational data are known.

<sup>b</sup> Signs omitted.

<sup>c</sup> That is, controlled on all occupational variables whose inclusion in the equation reduces the beta-coefficient for bureaucracy.

\*  $p < .05$ , \*\*  $p < .01$ .

cratization. Separate analyses of the white-collar and blue-collar work forces do indicate, though, that job protections contribute more to bureaucracy's impact on blue-collar workers; substantive complexity, to its impact on white-collar workers. It would seem

that the job protections afforded by bureaucracy matter most for men in occupations that do not already enjoy a substantial measure of security; substantive complexity comes to the fore only when some degree of security has been attained.

## DISCUSSION

Observers of bureaucracy, impressed by its need to coordinate many people's activities, have assumed that a primary effect of bureaucratization must be to suppress employees' individuality. We have found, to the contrary, that bureaucratization is consistently, albeit not strongly, associated with greater intellectual flexibility, higher valuation of self-direction, greater openness to new experience, and more personally responsible moral standards. In part, this seems to result from bureaucracies' drawing on a more educated work force. In larger part, it seems to be a consequence of the occupational conditions attendant on bureaucratization—far greater job protections, somewhat higher levels of income, and substantively more complex work. Job protections matter particularly for bureaucracy's impact on blue-collar workers; substantive complexity, for its impact on white-collar workers.

There are four issues that our data do not fully resolve. The first is whether the effects we have attributed to the experience of working in a bureaucratic organization might really be an artifact of what types of people bureaucracies recruit. We have found that the educational disparities between bureaucratic and nonbureaucratic work forces cannot provide a sufficient explanation, and that other disparities in the social and demographic compositions of the work forces have little relevance. But there remains the possibility that bureaucracies may hold a special attraction for self-directed, intellectually flexible men who are receptive to innovation and change. Perhaps, for example, intellectually active people seek jobs in bureaucratic organizations because that is where challenging work is to be found.

We cannot test this interpretation, because we have no information about the men's values, orientation, and intellectual functioning prior to their employment in bureaucratic (or nonbureaucratic) organizations.<sup>20</sup> There are, however, two reasons to

doubt that "self-selection" can explain our findings. The interpretation assumes that men have more complete and accurate knowledge of working conditions in bureaucratic organizations, before starting to work there, than is usually the case—especially in light of widely-held stereotypes about bureaucracy. The interpretation also assumes that men have a fuller range of choice in deciding on jobs than is usually the case—particularly when one remembers that our findings apply to men of all educational and skill levels and that many types of jobs can be found only in bureaucratic or only in nonbureaucratic settings. It would be more consonant both with our broader knowledge of occupational realities and with the specific data of this study to conclude that bureaucrats differ from nonbureaucrats primarily because they have experienced different conditions of occupational life.

The second issue is why the correlations are so small. We may have underestimated their size by limiting this investigation to one dimension of bureaucracy or by employing an inadequate index of that dimension. Our data, however, suggest an alternative explanation: that bureaucracy really does have a smaller social psychological impact than had been assumed. The psychologically most potent occupational conditions are those that maximize men's opportunities for self-direction in their work: freedom from close supervision, substantively complex work, and a varied array of activities (cf. Kohn, 1969:139-167). Bureaucracies do provide substantively complex work. But they conspicuously fail to provide freedom from close supervision. And although they provide a wide variety of complexly inter-related jobs to their white-collar workers, they tend to entrap their blue-collar workers in a routinized flow of simply-organized tasks. It is ironic and probably self-defeating that bureaucracies hire educated men, give them complex jobs to perform, and then fail to give them as much opportunity for

<sup>20</sup> We do find, though, that the correlations of bureaucracy with values, orientation, and intellectual functioning gradually increase in size, as men have worked for their present employer for greater

and greater lengths of time (at least until approximately six years, when the correlations begin to decline). The picture is the same whether or not age and education are controlled. But this evidence is hardly definitive, for most of the men have had previous jobs, and we do not know whether these jobs were in bureaucratic or nonbureaucratic organizations.

occupational self-direction as their educational attainments and the needs of the work allow.

The third issue is whether our findings reflect bureaucratization or only size. Two of the three occupational conditions that have come to the fore in our analyses—income and substantive complexity—may be only ancillary features of bureaucracy. It is not intrinsic to bureaucratic organization that it pay its employees more or that its work be substantively more complex. These are, instead, products of the very conditions that give rise to bureaucracy itself—large size, technology, the need for highly skilled employees, and the problems of coordination, planning, and record-keeping that result from size and technology. Job protections, though, are an essential feature of bureaucracy. As Weber long ago recognized (cf. Gerth and Mills, 1946:202–203), it is necessary to bureaucratic organization that its authority be circumscribed. The principal findings of this research—that job protections are central to the impact of bureaucratization on its employees—cannot be attributed to size alone; they reflect the structural essentials of bureaucracy.<sup>21</sup> (It is perhaps ironic that these structural essentials may be more important for the blue-collar work force, whom theories of bureaucracy have largely ignored, than for the white-collar work force.)

Last, and most important, is the issue of whether our focus on one dimension of bureaucratic structure—the hierarchical organization of authority—produces so partial a picture as to be misleading. Had we also studied such facets of bureaucracy as impersonality of procedures or specificity of rules, proximate occupational conditions other than (or in addition to) job protections, income, and substantive complexity might have come to the fore. The picture we have presented may be seriously incomplete.

Even if this be true, our findings are so

<sup>21</sup> It is hardly crucial to the argument (but nonetheless reassuring) that the correlations of bureaucracy with all our principal indices of values, orientation, and intellectual functioning remain statistically significant not only when we employ a truncated version of the bureaucracy index that makes no use of information about the size of the organization, but even when we impose the extreme restriction of statistically controlling size of organization.

at variance with common presuppositions as to require a rethinking of how bureaucracy impinges on its employees. Many writings about bureaucracy assert that a system based on the hierarchical organization of authority necessarily imposes tight discipline, leaving little leeway for initiative. Our data do indicate a tendency in this direction—employees of bureaucratic firms are supervised a little more closely than are other men of their educational levels, and close supervision does have the constricting effects ascribed to it. But the propensity of bureaucracies to supervise employees too closely is more than offset by the protections it affords from the arbitrary actions of superordinates.

Bureaucracies must ensure that superordinate officials are limited in what facets of their subordinates' behavior they are allowed to control and how they may exercise that control; superordinates cannot dismiss subordinates at will, and questionable actions can be appealed to adjudicatory agencies. The power of nonbureaucratic organizations over their employees is more complete and may be more capricious. Thus, the alternative to bureaucracy's circumscribed authority is likely to be, not less authority, but personal, potentially arbitrary authority. What is notable about bureaucratic practice is not how closely authority is exercised but how effectively it is circumscribed.

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# FLOW OF COMMUNICATIONS, EXPERT QUALIFICATIONS AND ORGANIZATIONAL AUTHORITY STRUCTURES \*

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American Sociological Review 1971, Vol. 36 (June):475-484

*This paper is concerned with some of the general issues of the relationship between comparative research and the case study and where one can complement the other, showing specifically how this applies to recent research dealing with the expert qualifications of operating personnel, the ratio of managers to operating personnel, and the flow of hierarchical communications in formal organizations. Inferences about the flow of communications within organizations in which superiors have narrow spans of control and supervise subordinates who are highly qualified are drawn from comparative research on the formal structures of organizations and tested with observational data from case studies on the interaction of superiors and subordinates. The findings from two of these case studies are generally consistent with the argument that narrow spans of control serve to facilitate upward communication where experts are employed as subordinates. However, the analysis of a third deviant case suggests that the degree and type of differentiation of hierarchical work roles importantly affect both the needs and opportunities for communication and through them the relationship between hierarchy and expertness.*

## INTRODUCTION

It is well recognized that the actual social relations which make up the daily operations of organizations cannot be accurately inferred from knowledge of formal structures alone. Styles of supervision, for example, cannot be predicted very well from knowledge of spans of control (Bell, 1967). These social relations must be studied directly. Yet, despite intellectual agreement in the matter, sociological research is frequently conducted as though these two levels of organizational analysis did not require independent empirical investigation. While most theories of organization include concepts referring to both levels (on multiple-level studies, see Lipset *et al.*, 1956:421-425), most research designs and methods of data collection tap only one level, or at least one is tapped more adequately than the other (on research methods appropriate to different levels of organizational analysis, see Blau, 1965). The bulk of research consists either of comparative studies of large samples of organizations with data from a few informants in each organization and from organizational records, or of case stud-

ies or small sample studies with data collected through interviews and observation. The two types of research are only rarely combined, although the first type is clearly best suited to the study of formal structure and the second to the study of the social relations. Putting multiple-level theory and single-level research together impedes the construction of verifiable theory in several ways. These obstacles to verification will be explored below, and it will be argued that case studies and comparative studies can complement one another to overcome them. To this latter end, data on the social interaction of superiors and subordinates that were collected in case studies of two business organizations (Brewer, 1970) are analyzed here in the light of recent comparative research (Blau *et al.*, 1966; Blau, 1968) in which inferences about hierarchical communication processes were made without the data to study them directly.

## *Case Studies, Comparative Studies, and Bureaucratic Theory*

It is obvious that case studies cannot establish the associations among organizational characteristics which are required to develop a theory of formal organization structure. But it is not necessarily true that case studies are highly relevant in other important respects. Associations among struc-

\* I am grateful to my colleagues Vernon K. Dibble and Hubert J. O'Gorman for their helpful comments, and particularly grateful to Peter M. Blau and Philip H. Ennis for their very extensive remarks on an earlier draft of the paper.

tural variables that are discovered in comparative studies are generally explained by reference to internal social processes. For example, in functionalist schemes of explanation, the explanation is partially provided by referring to the problems that one element of structure raises for participants in the social organization and the solution that other elements provide for these problems. However, the constraint of collecting data from many organizations in comparative research frequently makes it impossible to collect the very data describing actual social relationships that would verify these explanations. So long as they remain unverified, they are subject to continuous *ad hoc* revision to bring them into line with current structural findings, and structural studies are thereby deprived of a firm theoretical base from which to make predictions. It is a fundamental requirement of any theory of bureaucracy that the social processes which underlie associations among structural characteristics be established empirically.

By itself, the study of a single case can do little to specify the influence of formal structure on internal processes. The lack of variation in formal structure in one case provides no basis for comparison. But the significance of a case changes as soon as sufficient comparative knowledge accumulates, so that the researcher is not forced to see his conclusions either as unique or as applicable to all organizations (on dilemmas of over- and under-generalization, see Etzioni, 1961:xii-xiv). Comparative studies of structural variation can provide criteria for classifying individual cases in terms of their formal structural characteristics and, when these comparative studies make explicit their assumptions about intervening internal processes, can also lead to explicit differential predictions about the nature of internal social processes under the different structural conditions. This obviously aids in interpreting divergent case study findings (for an example of this type of analysis, see Dubin, 1965). But more importantly for the purposes of this paper, accumulated comparative knowledge increases the leverage that one may exert upon theory by initiating studies of single cases.

It becomes possible to locate in advance

what might be called *strategic cases*. These are the predictable deviant cases, the cases in which, if a given structural theory is correct, internal social processes should be different from what would be expected on the basis of most current evidence or pre-supposition. Studies of strategic cases serve two major theoretical purposes: (1) They serve to set explicit limits on the universality of propositions concerning social relations within organizations by looking at the deviant cases and the conditions under which they occur; (2) they provide direct tests of the propositions in theories of organizational structure which refer to internal social processes and thereby provide indirect tests of the structural propositions in these theories as well. The identification of such strategic cases for research provides a potentially productive way of integrating case and comparative analysis to deal with multiple-level theories of organization.

#### *Expertness, Hierarchy, and Flow of Communications*

There is good hunting for hypotheses to be tested in strategic case studies among reconceptualizations that are introduced to accommodate new structural findings. These reconceptualizations highlight particular cases for investigation by suggesting that under special conditions a particular element of organizational structure has a different significance for organizational processes than has generally been either observed or thought. Blau's research provides an excellent example.

Expertly qualified officials and hierarchically organized structures of authority are among the most important characteristics of bureaucracy (Weber, 1947). Weber's analysis implies that these two characteristics are functionally interdependent in producing a high level of organizational effectiveness. By contrast, later treatments of bureaucracy (Gouldner, 1954; Stinchcomb, 1959; Udy, 1959; Scott, 1966) suggest (1) that there are many incompatibilities and potentials for conflict between authority based upon hierarchical discipline and authority based upon expert knowledge; (2) that expertness and hierarchically organized authority are in fact functionally alternative forms of administration; and (3) that the two charac-



teristics are unlikely, as a result, to be found together. However, Blau and his associates have more recently reported research findings which support Weber. In two samples of organizations, one of 156 public personnel agencies and the other of 254 finance departments of state and local governments, they unexpectedly found that high technical qualifications of the operating staff (indicated by either the requirement of a college degree, or the requirement of a degree with a specified major) tended to be associated with more developed hierarchical structures of authority (indicated by a relatively high ratio of managerial to nonsupervisory personnel).

In interpreting his surprising finding that hierarchy and expertness are positively associated, Blau accepts the commonly held assumption that heavy downward communication is dysfunctional in important respects where the subordinates are experts. To do so, he is compelled to reject the equally common assumption that increased hierarchy implies greater downward communication where experts are employed. He argues, instead, that an operating staff with high expert qualifications creates an organizational need for upward communication. The narrower spans of control that are made possible by a high managerial ratio increase the opportunities for supervisors to consult with subordinates rather than to supervise them closely. This is important "... not alone because experts tend to be more alienated by one-sided directives but because they make greater contributions through feedback than persons with poorer qualifications" (Blau, 1968:458-459). This argument, of course, also contradicts the more usual assumptions that hierarchies make poor channels for upward communications (on distortions in hierarchical communications, see Florence, 1961:153; Kelley, 1951; Read, 1962; Torrance, 1955; Wilensky, 1967:42-48 especially) and that a high ratio of managers to operating personnel instead increased hierarchical control and downward communication (Worthy, 1950; Whyte, 1961:88-93; Blau and Scott, 1962:161-172). However, Blau dismisses as unlikely the more conventional interpretation that a higher ratio of managers might mean closer control by administrative officials where personnel are more qualified.

Although he has no data describing the interaction between superiors and subordinates in the organizations he studied, his argument is supported by the findings that managers in organizations with more qualified personnel have more responsibilities delegated to them by higher management and report spending less time on supervision and more time on professional work of their own. The reconceptualization is therefore plausible.

### *Hierarchical Status and Interaction*

Case studies cannot establish the relationships between structure and process, but they can test hypotheses which predict the ways in which different components of internal social process are related to one another in particular structural situations (on the method of internal analysis in the case study, see Lipset *et al.*, 1956:425-427). Hypotheses of this sort are essential to testing the validity of reconceptualizations. Blau's research suggests the hypothesis that in organizational work groups in which the superior has a narrow span of control and his subordinates have high expert qualifications, formal hierarchical status and rates of participation in superior-subordinate interaction will be either inversely related, or unrelated.

There is little doubt that narrower spans of control increase the volume of upward communication in an organization. If only by increasing the duration and frequency of contacts in which superiors and subordinates interact, narrower spans of control would accomplish this. However, increased interaction obviously increased the volume of downward communication as well. Moreover, the existing evidence suggests that social relations in organizational hierarchies are such that an increase in the total volume of hierarchical communication would in fact result in a net increase in downward communication. It has generally been found that formal hierarchical status and rates of participation in superior-subordinate interaction are directly related (Caudill, 1958:243-252, 295-296; Blau and Scott, 1962:98-99; Hurwitz, *et al.*, 1960). Where this pattern holds, any formal structural change which increased interaction between superiors and subordinates would tend to produce the benefits of increased upward communication at the cost

of a still greater increase in downward communication. If this is so, it makes little sense to explain the association between hierarchy and expertness primarily in terms of the upward communication functions served by an expanded hierarchy. The validity of Blau's explanation rests then not only upon the proposition that upward communications can be increased by increasing hierarchy, but also upon the additional and more questionable proposition that increased hierarchy can be introduced without increasing downward, more than upward, communication. If this second proposition is not true, then it is necessary to examine the constraints which prevent alternative structures with better balances of functional to dysfunctional consequences from emerging (on alternatives to hierarchy see Wilensky, 1967:45-48; Florence, 1961:149-162), or to reconsider the possible functional significance of hierarchy and downward communication in relation to expertness.

Data from case studies in two organizations can be used to test the hypothesis stated above. The data in each case were collected by direct observation of the interaction of superiors and subordinates at work in the organizations using Bales' (1950:37-38) units of interaction and a considerably modified version of his category system (for further discussion of the method, see Brewer, 1970). The first case, Department A, is a home-office underwriting department in a large insurance company. Using Blau's criteria, the insurance underwriting subordinates would be classified as having high expert qualifications and the underwriting superiors would be classified as having narrow spans of control. The subordinates, and superiors as well, were required to have college degrees, Blau's measure of expert qualifications. In addition, the underwriting subordinates were required to complete a curriculum of specialized courses on insurance given by the company's education and training department. Superiors could be assigned from two to four subordinates and were usually assigned three. Blau classified first-line supervisors with a span of control of less than six as having narrow spans.

The second case is from the electrical construction departments of a light and power company. The roving foremen, so called be-

cause they travelled from one job site to another, were a middle level of supervision in the departmental hierarchies. They supervised the crew leaders who provided first-line supervision for the construction crews that carried out the basic work operations of the departments. Each foreman had approximately four crews under his control. College degrees were not required and presumably would not be a relevant indicator of technical qualifications for this type of work.<sup>1</sup> However, positions were assigned to operating personnel only after extensive company training both on and off the job, and promotions to the major operating job classifications of lineman and electrical mechanic required that the candidate pass both written and practical examinations on the technical aspects of the work.

The direction of association between formal hierarchical status and participation in

<sup>1</sup> What sort of expertness does a college degree, even one with a specified major, indicate? Blau does not discuss the significance of his measure. It seems clear that college degrees do not provide expertness in the same sense as professional degrees, craft training, or organizational training programs. However, Weber (1947:339) distinguished two types of bureaucratic expertness. The first is gained through formal training and is brought to the organizational position from without by the bureaucrat. The second is developed or acquired while performing the duties associated with the position. The first type of expertness is formally recognized and signified by degrees, certificates, examinations passed, etc. The second type of expertness is neither so easily recognized, nor so likely to be signified formally. Yet the possession of either type can make subordinates' contributions valuable to managers. Positional expertness, the second type described above, is often cited as a key source of informal power and status for lower participants in bureaucracies and for bureaucrats in relation to their various publics and controlling agencies. But how can it be assured that managers will recognize the importance of this positional expertness among their subordinates or that subordinates will have the confidence to assert their positional knowledge in dealings with superiors? It may be argued that college degrees serve this function. By raising the general social status of subordinates, degrees increase the presumption of their general competence and therefore the presumption by both themselves and others that they will have something worthwhile to say about operating problems. This presumption will most likely be true if their jobs are ones in which positional expertness is developed and is necessary for organizational operations. Consequently, those employees with degrees will come to be regarded as more "qualified" than those without.

superior-subordinate interaction can be measured by the proportion of the total interaction that is initiated by the superior, more than 50% indicating a direct relationship and less than 50% indicating an inverse relationship. In Insurance Department A, a total of 5944 acts were recorded for six insurance superintendents interacting with their subordinates. The mean percentage of acts initiated by the superintendents was 42.7. The range was from 34.5% to 50.4%. It is clear that status and participation were not directly related in this case. In the construction departments, a total of 6238 acts were recorded for seven foremen and their subordinates. The mean percentage of these acts initiated by the foremen was 48.8. Four initiated less than 50.0% of the interaction (42.9, 46.0, 46.1, 47.6) and three initiated more (51.5, 53.6, 54.0). Again it appears that there is no direct relationship between status and participation, but support for the hypothesis is less clear here than in the preceding case.

It might be that the difference in support for the hypothesis is due to the difference between the usual spans of control of the foremen and the insurance supervisors, but the slight difference in spans of control makes this unlikely. However, superiors can modify the effects of a given span of control if they are free to adjust their contacts with subordinates to fit the demands of current operating problems and the demands of particular subordinates; rarely do all subordinates and all problems require equal attention. The insurance supervisors were free to schedule contacts as they thought best. The foremen did not have this flexibility. They were formally expected to contact each crew leader at his work site twice each day. Since the work sites were geographically dispersed, the travelling time alone, from one to the next, even if little time was spent at the site, could consume much of the foreman's workday. Few of the foremen tried to observe this rule very strictly. Most would try to visit each site at least once a day. But of the seven, two reported that they tried to comply with the twice-a-day rule, and they were in fact observed to make more contacts than other foremen. These two foremen initiated the highest proportions of interaction.

### *Formal Organization Structure and Social Process*

When a proposition about social processes in organizations does not hold universally, it is especially necessary to have theories which bridge the gap between organizational structure and organizational process, so that each new set of findings does not leave the researcher on the horns of what Bales (1950:117-123) has referred to as a "flip-flop dilemma"; for one is tempted to reverse the interpretation of the relationship between structure and process to fit the particular set of data at hand. To avoid flip-flop dilemmas and make determinate predictions possible, it is necessary to carefully specify the conditions under which the flip, rather than the flop, interpretation applies. Blau argues that the communication flow in organizational hierarchies is the combined result (1) of the structurally induced communication needs of managers and operating personnel and (2) of the opportunities that the organization structure provides for communication between them. The presence of an operating staff with high expert qualifications discourages downward communication both by reducing the managerial need to exercise close supervision and by increasing the operating staff's need for autonomy. At the same time, expertness encourages upward communication both by increasing the staff's need to exercise influence over their work situation and by increasing the value of their contributions to management. Organization members' abilities to actually satisfy these needs depend, however, upon the availability of opportunities for communication. The shape of the hierarchy determines these opportunities.

This argument suggests that status and participation will vary directly under the following conditions. (1) When communication opportunities are limited, as by wide spans of control, status and participation will be directly related. Considerations of administrative convenience will lead managers to rely heavily upon centrally formulated policies and directives. Consultation with subordinates will be seen by them as irrelevant and perhaps even disruptive; what will be important is that subordinates know, understand, and obey orders. (2) Even when

the organization structure provides ample opportunity for communication, status and participation will be directly related when other structural conditions, such as an operating staff with low expert qualifications, determine that: (a) the superior must closely supervise subordinates to satisfy relevant others with the performance of his role; (b) subordinates depend upon close supervision to perform their own roles satisfactorily; (c) the superior sees subordinates' information and advice concerning operating problems as unimportant to the satisfactory performance of his role; (d) subordinates do not see extensive upward communication as being essential or efficacious for influencing the conditions under which they perform their roles.

The usefulness of these propositions for further structural research depends upon the sociologist's ability to identify structural conditions—other than expertness and the span of control—which affect communication needs and opportunities. These conditions could obviously be inferred after the fact by examining the interrelations among formal structural characteristics in a large sample of organizations. But one need not wait. The case study method can help to predict as yet undiscovered structural relationships and to suggest new structural variables to be introduced into comparative analyses.

#### *The Hierarchical Differentiation of Work Roles*

One aspect of organizational hierarchies that receives only passing attention in Blau's analysis is the differentiation of superiors' and subordinates' work roles. Superiors and subordinates differ from one another not only in status and power but also in what they actually do on the job. At one extreme of *high differentiation* is the bureaucratic superior, who is busy with supervision and administration, and his subordinates, who are busy with production. At the other extreme of low differentiation is the working supervisor, known variously in different industries as the crew leader, crew chief, straw boss, group leader, or set-up man (Strauss, 1957), who performs supervisory and administrative duties with respect to his sub-

ordinates but spends much of his time at production work, working at the same site and doing either the same type of work, or taking part in an interdependent work process with his subordinates. Intermediate between these extremes is the situation, perhaps most common in professional organizations, in which both superior and subordinate are principally concerned with production but in which production work is graded by skill level so that different production functions are carried out at different hierarchical levels. Subordinates handle more routine matters and superiors do work that is seen as being more technically complex, more creative, as having more serious consequences of error, or in which the client is more demanding (for a description of the hierarchical division of labor in large law firms, see Smigel, 1969:141-160). For convenience and clarity, these variations will be referred to as low differentiation, professional differentiation, and bureaucratic differentiation.

The construction crews in the light and power company provide a third strategic case to be compared with the two preceding ones. The crew leaders, who are themselves supervised by the roving foremen, were the first line of supervision in the construction departments. Like the supervisors in the other two cases, they had narrow spans of control, frequently as few as two but never more than four subordinates. The construction workers who were their subordinates in the crews had the high technical qualifications described above. However, the interaction data suggest that in contrast to the other two cases, hierarchical status and participation in superior-subordinate interaction were directly related in the construction crews. A total of 8,007 acts were recorded for eleven crew leaders interacting with their subordinates. The mean percentage of interaction initiated by crew leaders was 58.2. The range was from 49.3% to 68.2%. It will be recalled that the comparable figure for the insurance superintendents was 42.7% (range from 34.5% to 50.4%) and for the foremen 48.8% (range from 42.9% to 54.0%). This case clearly contradicts the hypothesis stated earlier that status and participation are not directly related in work groups in which superiors have narrow

spans of control and their subordinates have expert training.

One of the most striking formal structural differences between this and the previously described cases is the relatively low degree of differentiation of superior-subordinate work roles in the crews. The crew leaders' formal duties include doing production work along with their men, as well as supervising and performing other purely administrative tasks. The foremen, in contrast to the crew leaders, were actually prohibited by company and union rules from performing any but supervisory and administrative tasks (bureaucratic differentiation). The insurance superintendents were not similarly forbidden to work with their subordinates, nor were the supervision and administration connected with subordinates' productive work, their major duties. They were expected to delegate routine work more or less completely and to spend the greater part of their time on the more important productive work of the department (e.g., evaluating insurance "risks" involving very high premiums, formulating general underwriting policy for the company's agencies, and dealing with particularly influential or troublesome insurance agents and brokers). Their subordinates, who were formally titled "assistant underwriters," were expected to assist the superintendents in their work by freeing them from more routine chores and by doing the more time-consuming research and "legwork" that the superintendents required to make decisions on particularly complex problems (professional differentiation).

The variation among the crew leaders in the percentages of interaction which they initiated with subordinates provides some validation for the importance of differentiation. As indicated by their own and their foremen's reports of their role performances, some crew leaders did indeed work regularly with their men on the job, but others spent nearly all of their time supervising, planning future work, ordering equipment, consulting with staff departments, etc. The six crew leaders who worked regularly with their men initiated a mean of 60.2% of the interaction with subordinates. The range is from 55.0% to 68.2%. The five crew leaders who deviated more from the role of working supervisor initiated a mean of 55.9% of inter-

action, with a range from 49.3% to 63.9%. Though status and participation in superior-subordinate interaction are directly related for work groups headed by both types of crew leaders, the relationship is obviously not as strong among the groups headed by crew leaders who had informally increased the differentiation of their own from their subordinates' work activities.

Blau's description of the nature of managerial role activities in the finance departments of his sample in which experts were employed suggests that superior-subordinate roles in these departments were highly differentiated and were differentiated along professional rather than bureaucratic lines. He found that managers in these departments spent relatively little of their time on supervision, planning, or administration, and more upon "professional work of their own." He theorizes that a manager's role activities affect his communications with his subordinates through their effects upon his familiarity with operating problems, which in turn influences his ability to make use of the opportunities for upward communication that narrow spans of control provide (Blau, 1958:461). It may be inferred from this that the upward flow of communication is greater in units in which superiors' and subordinates' work roles are differentiated professionally rather than bureaucratically. This inference is supported by the already reported finding that the upward flow of communication was slightly greater in the insurance department than it was between the foremen and their subordinate crew leaders. Low differentiation, even more than professional differentiation, increases the superior's familiarity with operating problems. But the relatively light flow of upward communication found in the construction crews, compared to the other units, does not bear out the additional inference that there is greater upward communication where there is low differentiation. To provide an explanation that accounts for this finding, it is necessary to consider the impact of differentiation upon communication needs as well as upon opportunities. It can be plausibly argued *post factum* that low differentiation reduces the upward communication needs that are associated with having expert subordinates and that it produces downward communi-

cation needs that do not result from either professional or bureaucratic differentiation.

For communication to occur at all, the communicators must share some measure of common ground. If it is to occur for reasons other than mutual support, there must also be differences between them. The superior who, like the crew leader, is directly involved in the same work operations as his subordinates, given that he is as expertly qualified as they are, will certainly have less need for their information, opinions, and suggestions concerning these operations than one who is more removed from the actual work of his unit. He can, in short, see matters for himself. The distance from production that is created by bureaucratic differentiation makes it difficult for the superior either to see for himself, or to see through his subordinates' eyes. Where there is professional differentiation, the superior's role activities give him the familiarity with operating problems *in general* to communicate with subordinates easily, and in such a way as to encourage further communication. Yet he does not have direct visibility of all the operations for which he is responsible. Hence, subordinates' observations are of greater value to him than to the working supervisor.

Low differentiation naturally reduces not only the distance between the superior and the work but also the distance between the superior and his subordinates. This result of low differentiation may alleviate the problem of alienation stemming from the greater needs for autonomy and influence among expert subordinates, which Blau identifies as yet another source of the need for extensive upward communication. Reduced social distance and increased physical accessibility of superiors to subordinates enable subordinates to exercise more direct and continuous influence upon their superiors. (For research on the effects of reduced distance between superiors and subordinates on alienation and influence from below, see Pearlin, 1962; Blau, 1963:207-228). Furthermore, it increases subordinates' visibility to superiors, allowing them to anticipate subordinates' demands. Extensive upward communication may only be necessary to avoid alienation where superiors are unable to anticipate subordinates' demands and where subordinates can exercise influence only intermit-

tently and circuitously through their reports of work operations.

A system of management in which there is low differentiation of hierarchical work roles may also create certain special problems which encourage heavy downward communication. One of these problems is the need to affirm the lines of formal authority. For example, because the crew leaders performed production as well as supervisory and administrative work, they were required by the terms of the collective bargaining agreement to be union members. The nature of their authority over their men was in consequence highly ambiguous. While management gave them the routine supervisory powers to give performance ratings and the right to enforce directives and rules, the union specifically prohibited them from making use of these same formal powers. One means used by management to strengthen and clarify the lines of formal authority was to channel, as much as possible, all communication to the crews through the crew leaders and to restrict the availability of non-hierarchical sources and channels. Printed work orders, blueprints, manuals of procedure, and books of specifications were, for instance, the exclusive property of the crew leader. He became in this way the primary source of job information to his men. The comparable impersonal and nonhierarchical sources of information and directives in the insurance office were made freely available to subordinates.

A second problem is that when the supervisor works along with his men, he tends to retain the primary operative as well as administrative responsibility for the work of his group. The insurance underwriting subordinates and the crew leaders (as subordinates of the foremen) were expected to continue at their work whether their superiors were actually present at the work site or not. By contrast, it was the crew leader's responsibility not only to supervise the results of his men's work and to see that it was done properly, but actually to lead them personally in getting it done. Continuous work performance required continuous downward communication. This situation was formally expressed in the provision in the company's rules that work could not continue without a crew leader (or a construction

worker who had been temporarily elevated to the status of crew leader) present at the work site. The rule was much more enthusiastically supported by the union than by the company management. The union argues that it was necessary to restrict the delegation of operative work responsibility at the level of the crews in order to preserve a hierarchy of management positions into which production workers could be promoted. Without this restriction on delegation, it was feared that the crew leaders, who were not management, would come to replace the foremen, who were. Thus, to those above, low differentiation constituted a threat to hierarchical control, while to those below, it threatened to close channels of social mobility within the company.

### *Implications for Bureaucratic Theory*

Organizational studies that concentrate upon formal structure are often criticized for presenting an overly rational picture of organizational development, one in which the purposes of managers are taken to represent organizational reality, and in which the functional requirements of organizations as means of achieving officially stated goals are given too high a priority relative to other determinants (see for example Gouldner, 1955). This is obviously not an empty criticism. Theories of organizational structure do indeed often sound disquieteningly similar to the explanations that managers provide to their shareholders. Naturally, for this reason one cannot dismiss these theories outright, anymore than one can automatically disbelieve the managers themselves. But one can be skeptical. It would not be surprising, for example, if the managers responsible for expensive and potentially unpopular organizational changes, such as the increased bureaucratization of the authority structure, were more likely to justify these changes in terms of the exigencies of the work than in terms of defects in the current system of management or the need to increase hierarchical control. Blau implies that the greater technical complexity of the tasks that are carried out in the organizations which employ experts is the ultimate source of the greater bureaucratization of authority structures in these organizations. (If the

tasks in these organizations are not more technically complex, why hire personnel with higher technical qualifications?) This explanation closely parallels the rationales that the insurance and electrical construction managers provided for narrow spans of control. The managers in both organizations stressed that their operating personnel were too technically competent to require very close supervision and that what was really required was increased communications to cope with the complex technical problems that faced their departments.

Blau's explanation and the managers' rationale are consistent with the data on communication patterns in the insurance department and at the foreman-crew leader level in the construction hierarchy, but they are not consistent with the data on communication in the construction crews. The sociologist's skepticism of official rationales for bureaucratic growth, or for sociological theories that closely parallel these rationales, can be profitably channelled into the study of deviant cases of this kind. In this instance, the case suggests that the structure of management itself may be an important factor in determining whether or not the presence of expert operating personnel creates a need for more upward communications and so for more managers. The need for upward communication appears to be high only when there is high differentiation of superior and subordinate roles which removes the superior from first-hand contact with operating problems and from close contact with his subordinates. The direct association between hierarchy and expertness that was reported by Blau, in so far as it is indeed based upon needs for upward communication, may therefore be dependent upon the presence of the third structural condition of highly differentiated hierarchical work roles. If a high ratio of managers to operating personnel is found in organizations with low differentiation, one would suspect this development to be the result of needs for downward communication that are unassociated with expertness. Hence, low differentiation should be found to be associated directly with the development of hierarchy whether expertness is present or not; but high differentiation should be as-

sociated with hierarchy only when expertness is present.

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# DECISIONS AND EXCHANGE \*

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*American Sociological Review* 1971, Vol. 36 (June):485-495

*A formal theory is developed to deal with some aspects of social exchange behavior. Values are defined as "givens," and the exchange of values is defined as "decision-making." Exchange decisions can follow one or more "exchange rules"—rationality, reciprocity, altruism, status consistency, or competition. Formal definitions are developed for these rules using concepts from game theory. From the definitions and the assumption that exchange rules operate as social norms, a number of hypotheses about the properties of exchange situations and behavior are derived for two-person exchanges.*

## INTRODUCTION

EXCHANGE theory is an approach to social behavior that has been used by a number of recent theorists (Homans, 1961; Blau, 1964; Thibaut and Kelley, 1959). To take an exchange approach means to view social behavior as "an exchange of acts, more or less rewarding or costly, between two or more people" (Homans, 1961). Taking this approach, one studies the relationships between two kinds of variables; the quantity and kinds of acts performed, and rewards and costs. This paper attempts to deal with a few of the theoretical problems that arise in an exchange approach. The central problem is how and whether to define "rationality" as a principle of social exchange, and the solution offered is to give the term rationality a somewhat narrower meaning than often is done, and to propose, offer formal definitions for, and examine theoretically some alternative principles.

### *The Premises of Social Exchange*

The basic assumption here is that human social behavior can be logically derived or predicted from premises held by the actor whose behavior is being predicted. These premises include (1) his values, (2) his perception of the alternative behaviors available to him, (3) his expectations of the consequences of these alternatives to himself and others (consequences including the prob-

able responses of others), and (4) a "decision rule," which is a kind of social norm telling him how the first three premises should be combined to yield a prescription for his behavior. The first three premises will be defined as "givens," and the analysis will concentrate on the decision rules.

Taking this approach requires that we be able to define values separately from behavior, both logically and empirically. A way of doing this is presented by Hamblin and Smith (1966). They define values as conditioned nonvoluntary emotional responses to stimuli just as perceptions are nonvoluntary responses to stimuli such as visual, auditory, etc. Values, although they are conditioned nonvoluntary responses, can be measured in the same way that perceptual responses can be, according to Hamblin and Smith. In a similar approach, Emerson (Forthcoming), also working with an exchange theory, uses concepts of operant conditioning to develop definitions of social values. Hamblin and Smith point out that viewing values as nonvoluntary does not imply that behavior is nonvoluntary, because people may hide the way they feel, or weigh one value against another. This distinction will be followed here: values are defined as nonvoluntary while behavior is defined as voluntary, that is, the result of *decisions* by an actor. It will also be assumed that values can be discovered without observing the decisions.

Similarly, perceptions of alternative behaviors, available at any given time, and expectations of their consequences may be defined as cognitive structures existing in the mind of a person before he acts, and

\* This work is part of a research project conducted at the University of Washington supported by National Science Foundation grant number GS-2318.

also discoverable independently of behavior. As a convenience in the ensuing discussion, values, perceptions and expectations will be lumped together as "values," meaning the evaluative and cognitive structures that are the premises of behavioral decision-making. They will be separated again in the formal definitions.

Speaking of behavior as decisions has the advantage of providing a logical point of articulation among sociological, psychological, and mathematical theories since both psychology and mathematics have branches that deal with decision-making. One common approach to the discussion of decision-making is to assume that the person who is faced with two or more mutually exclusive alternatives has different preferences for the alternatives, determined by his values, and that he will choose the one he values most. The question is whether, if we know what a person values, we can reasonably use the assumption that his decision is strictly determined by an effort to maximize his values.

### *Rationality and Its Alternatives*

This is a persistent question in social science. For example, Max Weber (1947: 115) proposes that people do not always pay attention to the expected consequences of an action. He distinguishes between four bases of decision: tradition, emotion, rational calculation of means and ends (*Zweckrationalität*; this corresponds to the definition of rationality used here), and evaluation of the means without regard to ends (*Wertrationalität*). A person who bases a decision on considerations of *Wertrationalität* will be rational in the sense of applying logic to figure out what the consequence of his decision will be and how he will evaluate that consequence, but may not choose to maximize his value because he sees the act itself as illegitimate. This sort of question has more recently been the subject of debate between economists and anthropologists, with respect to the appropriateness of assuming that all exchanges of goods in primitive societies are instances of optimizing behavior (Sahlins, 1962; Burling, 1962). The anthropologists tend to believe that it is more useful to think of exchanges of goods as reciprocating behavior, assuming that the criterion for

decision is the *balance* of exchange rather than the amount gained by one or the other party.

The problem of rationality has also played an important part in the theoretical work of Parsons, for example: "A role, then, may define certain areas of pursuit of private interests as legitimate and in other areas obligate the actor to pursuit of the common interests of the collectivity. The primacy of the former alternative may be called 'self orientation'; that of the latter 'collectivity orientation'" (Parsons, 1951:60).

These people have in common an idea that it is useful in describing human social behavior to recognize a difference between value maximizing behavior and behavior that is systematic, predictable, and results from conscious decisions, and/or behavior that is the result of logical reasoning, although all of these could be called rational. This is useful, for rationality can easily become tautological; if we predict that people choose what they value and find out what they value by observing what they choose, we have not accomplished much except to describe choice behavior.

On the other hand, there are reasons for making a rationality assumption. One is the observation that people do seem to choose things they say they like more than they choose things they say they dislike. Another reason is that it is very useful in constructing theories to have one simplifying assumption that applies to all situations instead of having to find a new one for each new situation. This is the reverse aspect of the tautological property of a universal assumption; if we assume that all behavior is rational in the sense of maximizing values, we are equipped with a way of finding out what people value. Many users of exchange theory have done this; much of Homans' (1961) book, for example, is devoted to this kind of analysis.

If rationality is useful but does not, unaided, explain all behavior, what alternatives do we have? An examination of sociological and social psychological literature reveals that a number of alternative assumptions have been proposed. Most of these have been assumed at least implicitly to apply to all social behavior.

One is reciprocity. For example, Gould-

ner (1960) asserts that there is a general norm of reciprocity, which requires that a person help someone who has helped him or at least not harm someone who has helped him. Similar to reciprocity is equity, according to which people try to get out of an exchange what they think they deserve on the basis of what they have put into it. Adams (1965), for example, reports an experiment in which subjects hired as interviewers were given the impression that they were either highly qualified or poorly qualified for the job. When the workers who thought they were poorly qualified were paid by the hour, they turned in more interviews than those who thought they were highly qualified, whereas when the "poorly qualified" workers were paid by the number of interviews, they turned in fewer interviews (i.e., they performed so as to be paid less) than the "highly qualified" workers. This finding is not predictable from a rational model but is consistent with an assumption of a tendency to equalize value given and value received, which Adams supports with an argument from dissonance theory.

Another variant of this principle is distributive justice (Homans, 1961), according to which a person with higher investments deserves higher rewards; investments include the values of acts, costs to the actor, and external status characteristics such as age and sex. Status consistency or rank equilibration is still another version of this principle, in which it is assumed that people try to distribute rewards proportionally to status on an external dimension, e.g., race or sex.

Other exchange principles include competition or rivalry (Thibaut and Kelley, 1959: 227), in which the object of the exchange is to get more rewards than another person gets even at an absolute cost; and altruism or social responsibility (e.g., Berkowitz and Daniels, 1963), in which the object is to help another person. Several principles or motivations could operate at once in an exchange; for example, Messick and McClintock (1968) hypothesize that the three motives of maximizing joint gain, relative gain (competition), and own gain are the possible bases for choice in two-person, mixed

motive games. They also show how it is logically impossible to separate the three motives in any one situation.

We could say, of course, that each of these principles is a value and that the person who reciprocates is being rational because he is maximizing the value of reciprocity. This merely postpones the problem because we will still want to know how to define reciprocity, whether we want to assume that it is a universal social value and, if not, what situations it applies to.

If we wanted to invoke any of these principles as a universal principle of exchange, we could use it in the same way rationality can be used—as a way of discovering what people value. Thus, the assumption that all behavior is reciprocating is a reminder to examine the value of what is gotten in return or the obligation created in an exchange. If we assume that all exchange is conducted on the principle of status consistency, and observe that one kind of person gets paid less than another for doing exactly the same work, we can infer that one person has higher status on some dimension and look for that dimension. If we assume that all exchange is altruistic, and observe that John takes something away from James, we can conclude that John thinks James would rather be without the object in question. This begins to sound absurd, just as trying to fit all social behavior into a strictly rational model can begin to seem strained when we are faced with someone who is obviously incurring a cost in order to follow some other principle of exchange. A reasonable goal for theorizing would thus seem to be a development of statements about when each of the principles, including rationality, can be expected to apply.

In order to proceed with this theorizing, more specific definitions of the exchange principles will be required.

### *Formal Definitions of Exchange Rules*

To begin with, the following elements of social exchange will be introduced as undefined terms: *persons* (the participants in the exchange), *acts* (items of behavior performed by the persons), and *values* of acts (the reward or reinforcement a person re-

ceives from an act). Values will be classified as *rewards* (positive value or positive reinforcement) and *costs* (negative value or reinforcement). Values can include material objects, physical states of a person, affection, deference, conformity, etc. Opportunity costs (that is, value of a thing foregone in order to do something else) will be included as a kind of cost. For the present, only two-person exchanges are considered and the two participants are called P and O, in keeping with conventional notation. The perceptions of acts and values are defined from the point of view of one person, P.

*Definition 1:* A *social exchange* is a set of acts performed by P and O which have value for both P and O. Consider, for example, a committee composed of P and O which is assigned the task of drafting a set of recommendations for a larger group. O's acts will be rewarding to P if O has ability at this task and is willing to work at it. O's acts will not be rewarding to P if O has ability but is not willing to work. And if O is willing to work but has low ability, his acts may be costly because he will get in the way. Another instance in which O's acts will be costly to P is when O has ability and is willing to work, but the actions he would like to see recommended are significantly different from the actions P wants to recommend.

The actual value received by P from this exchange depends on (1) the acts available to each of the actors (e.g., their abilities), (2) the value each places on the acts, (3) the way the actors' values are structured in relation to each other (e.g., do they want the same thing or different things), and (4) the choices each makes among the acts available to him. In using these factors to define exchange principles, I shall make use of a set of analytic tools derived from the branch of mathematics called "game theory."

Game theory considers an individual (who may be a group acting as a unit) faced with a set of alternatives or acts. The acts have certain consequences—rewards and costs. A "game" is a situation in which the consequences of the alternatives are not only a result of the decision of the actor (and, possibly, chance) but also of the decisions of one or more other persons. Each person has preferences among the possible consequences of acts. Usually, a "game" is a conflict situ-

ation in the sense that the outcome most preferred by one actor is not the outcome most preferred by the other(s); but this is not necessary to the analysis. (More information about game theory may be found in Luce and Raiffa, 1957; Shubik, 1964, also has a useful description.)

The structure of a game may be described by a pay-off matrix, such as the one in Figure 1<sup>1</sup> (see Figure 1 on p. 491). This way of describing a situation of interdependence between two or more people has been used by a number of social psychologists; for example, Thibaut and Kelley (1959: especially pp. 13–21) and Deutsch (1958). Also, recently a great deal of experimental work has used game matrices as part of an experimental procedure; examples can be found in recent issues of *Journal of Conflict Resolution* and *Journal of Personality and Social Psychology*.

*Definition 2:* The *goal structure* of an exchange is the interdependency of rewards and costs. It is defined by P's values, his perceptions of the alternatives available to him, his expectations of the consequences of each alternative, and his perception of the same factors for O. A goal structure is *conflicting* if a reward to O implies a cost to P and vice versa. A zero-sum game is one example of a conflicting goal structure. A goal structure is *cooperative* if a reward to O implies a reward to P and vice versa. A goal structure is *mixed* if some alternatives are conflicting and some are cooperative.

*Definition 3:* The *decisions* of P and O are the acts each actually performs. Thus, an exchange is really a set of decisions. A decision by P is *helpful* if it results in a reward to O; it is *nonhelpful* if it results in a cost to O.

The matrix notation is useful in analyzing discrete decisions; however, since a social exchange is a set of decisions over a period of time, some definitions about the cumulation of decisions are required.

*Definition 4:* The *total pay-off* to P from an exchange is the total value P receives

<sup>1</sup> In a two-person game in matrix form, Player 1 (the row player) determines by his choice the row of the matrix, and Player 2 (the column player) determines by his choice the column; the unique cell thus determined contains a pay-off to each player, with the pay-off to Player 1 being written first.

from both his own and O's decisions over the course of the exchange, a fixed time period. Total pay-off at the moment will be defined as simply the sum of the values, to P, of all decisions made by him and by O, although it could be defined as a more complicated function of the number and value of decisions in which concepts like diminishing marginal utility are taken into account.

Total pay-off corresponds to the concept of profit (reward minus cost) as used by Homans (1960). It recognizes that, in addition to the rewards and costs he receives from O, P may receive rewards and costs from his own actions.

For purposes of this analysis, it will be assumed that the actors can communicate with each other prior to decision, that each has full knowledge of the goal structure, and that decisions may be made either simultaneously or sequentially. Some of the logical implications of these and alternative assumptions are discussed by Schelling (1960); psychological implications, by Thibaut and Kelley (1959).

In addition to listing the players, alternatives, and pay-offs, the mathematical analysis of a game requires stating a decision rule; that is, a way of deciding which alternative each player is to choose. The usual decision-rule for two-person, zero-sum games, in which decisions are to be made simultaneously, is the "minimax" rule which says to take the alternative that guarantees the lowest maximum cost no matter what choice the other player makes. This rule is adopted by game theorists because it satisfies a more general criterion for decision making; it *maximizes expected value*. This means that over the long run, decisions that follow the minimax rule will result in more gain than any other set of decisions. (See Luce and Raiffa, 1957, Chapter 4, for a more detailed description of the rationale for the minimax rule.) In some games, there is one choice that is clearly the best for a given player by the criterion of maximizing expected value; no matter what the other player does, this one choice will guarantee him the least loss. If one choice is always the most rational, it is said to *dominate* the other choices. In some games, no choice dominates for either player, and in others, while neither choice dominates, there is a

minimax solution which consists of both players choosing on a random basis. From the general criterion of maximization of expected value may be derived a strategy—minimax—from which in turn may be derived a specific choice in a specific game, e.g., "always choose 'A'," or "choose 'A' 30 percent of the time."

Game theory is usually dismissed by sociologists at this point because once a decision rule has been introduced, the theory is normative rather than descriptive; it says what one should do in order to maximize expected value rather than what people actually do. However, much of social behavior is normative; culture consists in large part of rules about what people should do in given situations. It ought to be possible to use the normative aspects of game theory to represent normative aspects of social behavior.

It should also be remembered that the minimax strategy is not the only rule that could be applied to a game; for example, one could use a decision rule that says to take the alternative that promises the highest reward, even though it might also result in a high cost. From this general rule, specific prescriptions for behavior may be derived just as from the minimax rule; however, the decisions thus prescribed will in general not maximize expected value. In fact, an interesting feature of the logical structure of games that has limited the usefulness of this approach for social scientists is that although the minimax strategy will always provide a basis for deriving a choice in a zero-sum game, it will not always provide a reasonable solution to other games, i.e., to cooperative and mixed goal structures. For these games, some other decision rule is required even for mathematical analysis.

The various exchange principles listed previously, such as reciprocity and altruism, may be thought of as alternative decision rules; each states a criterion for decision in terms of the pay-offs to each participant in an exchange. Each of them, if defined mathematically in terms of the criteria they should meet, may be used as a basis from which to derive a decision between alternatives in an exchange situation.

*Definition 5a:* An *exchange rule* is a rule assigning a pay-off to one or both of the participants in an exchange.

*Definition 5b: Rationality* is an exchange rule that assigns to P the outcome that maximizes his total pay-off.<sup>2</sup>

This means (1) making decisions that maximize his contribution to himself and (2) making decisions that are helpful to O, only if he expects (a) that O will not make decisions that are helpful to him if he does not, and (b) the decisions P makes are less costly to him than the helpful decisions O makes are rewarding.

If P follows a rational exchange rule, the consequences to O will depend on the structure of the game; P's following a rational rule in a conflicting goal structure will result in nonhelpful decisions, while following a rational rule in a cooperative goal structure will result in helpful decisions.

*Definition 5c: Altruism* is an exchange rule that assigns to O the outcome that maximizes his total pay-off.

*Definition 5d: Group-gain* is an exchange rule that assigns the maximum value to the sum of P's and O's total pay-offs. (This corresponds to Parsons' "collectivity orientation.")

*Definition 5e: Competition* is an exchange rule that assigns the maximum possible value to the difference between P's and O's total pay-offs, (provided P's is higher).

*Definition 5f: Status consistency* is an exchange rule that assigns the maximum value to the difference between P's and O's pay-offs, with P's higher than O's if P has higher status than O before the exchange begins, and lower if P has lower status, and the minimum difference between P's and O's pay-offs if they have equal status before the exchange begins.

*Definition 5g: Reciprocity* is a rule that assigns the minimum value to the difference between the amount that P's decisions have contributed to O's pay-off and the amount that O's decisions have contributed to P's pay-off.

Status consistency and reciprocity require more information than is contained in the description of the goal structure of an exchange; reciprocity requires that P know what O's contribution has been in the past, and status consistency requires that P know

what the relative status of the two actors is outside the exchange. Note that status consistency and reciprocity pay attention to different aspects of value; reciprocity is concerned only with contributions to the other person, while status consistency is concerned with total pay-offs. In this respect, reciprocity differs from all of the other rules; it concerns what P and O put into the exchange, rather than what they get out of it.

Status consistency for a higher status person is identical to competition; it is the opposite of competition for a lower status person.

Some comment should be made about the difference between reciprocity and bargaining since the two in many instances describe the same behavior. Bargaining will be defined as reciprocity when it is also rational to reciprocate. It will occur when both persons have a rational orientation and the goal structure is mixed. Reciprocity, as distinct from bargaining, may occur when the acts P performs for O are more costly to P than the value to him of the acts he receives.

These definitions are admittedly somewhat arbitrary; any of the rules could be defined in some slightly different way. For example, reciprocity could be defined in terms of pay-offs. Other rules could be added such as a rule that assigns the minimum value to O's pay-off, perhaps labeled "sadism." The definitions just presented seem to be the simplest forms of the most commonly proposed principles of exchange; also other reasonable definitions will in general have the properties (described below) that these exchange rules have.

Each of these rules, when applied to a specific goal structure, provides a prediction (or prescription) for a decision. For an example, consider the following goal structure.

If we take the point of view of Player 1 (P) in Figure 1 and apply each of the exchange rules, we find that:

(1) for rationality: P should always choose B, because he prefers B-X (which gives him 4) to A-X (which gives him 3) and he prefers B-Y (which gives him 3) to A-Y (which gives him 2).

(2) for altruism: P should choose A if Player 2 (O) chooses X, and B if O chooses Y.

<sup>2</sup> Remember that all definitions are from P's point of view.

		Player 2 (O) choice	
		X	Y
Player 1 (P) choice	A	3,2	2,2
	B	4,0	3,3

FIGURE 1

(3) for competition: P should choose B if O chooses X; if O chooses Y, P is indifferent. Thus B is his best choice.

(4) for group gain: P wants A if O chooses X, and B if O chooses Y.

(5) for reciprocity: P needs to know what O has done for him in the past. If O's previous decisions have resulted in P's receiving 2, then P should choose A no matter what O chooses.

(6) for status consistency: if P has higher status, his preferences are the same as for competition. If P and O are of equal status, P prefers either A-Y or B-Y to any other alternative, but he is indifferent between A-Y and B-Y. Note that in this case P's decision makes no difference to his ability to follow an exchange rule; it is all up to O. If P has lower status than O, he must try for A-Y or B-Y, although within the goal structure of this exchange, he cannot satisfy low-status consistency.

### Formal Properties of Exchange Rules

This example points out several features of the exchange rules; they are neither mutually exclusive nor exhaustive with regard to the choices they prescribe, nor does each necessarily lead to a unique outcome. It is possible for P to satisfy more than one at a time but it is not possible for him to satisfy all at once. The goal structure helps determine which rules are compatible with each other and which provide unique solutions, and P's knowledge of O's past actions and probable future behavior are factors that influence the decision derived from some exchange rules, but not all. In general, these things will be true for any goal structure, but *which* rules are compatible or contradictory or provide strategies dependent on O's choice will depend on the goal structure.

**Definition 6:** Two exchange rules are *mutually consistent* in a given goal structure if they both choose the same alternative. Two

exchange rules are *mutually inconsistent* in a given goal structure if one rule says to choose one alternative and the other says not to choose that alternative.

In the example just given, group gain and altruism are mutually consistent; rationality and reciprocity are mutually inconsistent.

**Hypothesis 1:**<sup>8</sup> For any pair of exchange rules there exists (a) some goal structure in which they are mutually consistent, and (b) some goal structure in which they are mutually inconsistent (except for competition and status consistency for high status P).

This hypothesis is derived from the logical properties of the exchange rules as defined.

Not only is it possible for several exchange rules to be consistent with a single choice, it may be that a given exchange rule provides no unambiguous prescription for a choice. Although the idea of dominance in game theory applies only to rational decision-making, it can be applied to the other exchange rules. If a rule, when applied to a given goal structure, points to a single alternative, no matter what choice the other player makes, or to a single strategy if the other player's exchange rule provides him with a single best choice, the exchange rule alone determines P's choice. If not, some other factors must enter the decision-making process.

**Definition 7:** An exchange rule has a *dominant choice* for P in a given goal structure, if (a) that choice will always result in an outcome more conforming with the rule than the outcome from any other choice, or (b) O's exchange rule has a dominant choice, and there is one choice for P that will always result in an outcome more conforming with P's exchange rule than the outcome from any other choice if O chooses his dominant choice. If an exchange rule in a given goal structure has no dominant choice for P, the exchange rule is *ambiguous* for P in that goal structure.

Note that dominance and ambiguity, like consistency and inconsistency, are specific

<sup>8</sup> In the formal part of this theory, statements derived from previous definitions or from assumptions are called hypotheses; statements not so derived are called assumptions.

to an exchange rule, a goal structure, and a participant.

*Hypothesis 2:* For any exchange rule, there is a dominant choice in some goal structures and it is ambiguous in some others.

In the example in Figure 1, rationality and reciprocity clearly have dominant choices; so does status consistency between equals (the choice is A). The other rules are ambiguous.

This set of definitions and the hypotheses derived from them can be related to sociological theory by the following assumption.

*Assumption 1:* A person in a social exchange always adopts at least one exchange rule as a social norm for that exchange. He will try to make decisions consistent with his exchange rule (or rules, if he adopts more than one).

It will also be assumed for the sake of simplicity that P and O adopt the same exchange rules, although the question of what happens when they do not is a pertinent one.

Some of the reasons that a person adopts one exchange rule rather than another as a norm are similar to factors affecting his adoption of other social norms; these include role expectation, mutual influence processes, cultural values and personality. In addition, there are reasons provided by the characteristics of the exchange rules themselves. In the remainder of this paper, a brief discussion will be devoted to each of these sets of factors.

### *Exchange Rules as Norms*

Exchange rules may operate as informal or formal norms. Statements about "fairness" or "justice," expressions of felt or perceived obligation, requests for and offers of help, and exchanges of gifts are clues to exchange norms. On the informal level, adoption of exchange norms may be supposed to be subject to processes such as imitation, pressures to conform to norms being followed by others, and degree of commitment to the exchange. A number of examples of these processes in informal groups are cited by Homans (1961) and Blau (1964), although both these analysts concentrate more on goal structures in the form of resources pos-

sessed by actors. On the formal level, role relationships may include, among other expectations attached to role positions, an expectation of which exchange rules are appropriate to the relationship.

Some empirical evidence for the assertion that exchange rules are part of role expectations can be found in experimental studies of helping behavior which indicate, for example, that a person is more likely to repay a favor when the favor is defined as appropriate to the role relationship (Schopler and Thompson, 1968), and that a person is more likely to engage in an "altruistic" act when he has observed a person of a similar social category to himself (a role model) performing the altruistic act than after observing a different type of person being altruistic (Bryan and Test, 1967). In a different sort of research, an interview study (Muir and Weinstein, 1962) found social class differences in norms of social obligation to role categories such as relatives, neighbors, and friends. These norms included things like the appropriateness of giving favors and the necessity of repaying dinner invitations.

One could hypothesize that in our culture friendship relationships tend to be characterized by reciprocity and status consistency between status equals, while business relations are characterized by rationality; and in bureaucracies, status consistency between status unequals is an important exchange norm. Parsons (1951) proposes that the professions are characterized by a group-gain rule (collectivity orientation), which is reinforced by their position in the institutional structure of society.

Exchange rules as social norms can also be predicted from more general values; for example, differences between cultures in propensity to compete or cooperate have long been noted by anthropologists (e.g., Benedict, 1935; Mead, 1937).

In addition to shared expectations attached to roles and cultural values, there may be individual differences that affect which exchange rule a person adopts. In experimental studies of two-person games (in which a pair of subjects in a laboratory setting is presented with a "game" structured according to some kind of mixed-motive principle and asked to make decisions) a



number of personality factors have been found to be associated with higher rates of "cooperation" (which in my terminology is a combination of altruism, group gain and rational bargaining). Low F scores (Deutsch, 1960), internationalism (McClintock *et al.*, 1963), flexible ethicality (Lutzker, 1960), and high need for achievement (Terhune, 1968) have been found to be associated with cooperation. Typically, though, the amount of variance in cooperative behavior explained by personality variables is relatively small (Terhune, 1968).

### *Some Implications of the Formal Properties of Exchange Rules*

The definitions of the exchange rules themselves lead to some hypotheses about the social circumstances under which they will be adopted.

*Hypothesis 3.* Status consistency can be adopted only when P and O know what their relative status is outside the exchange. Status consistency between unequals can be adopted only when P and O have unequal status outside the exchange, and status consistency between equals can be adopted only when they have equal status outside the exchange.

*Hypothesis 4.* Reciprocity can be followed only when the exchange has a history, although it may be adopted as an expectation at the beginning of an exchange.

This is a variation of Gouldner's (1960) hypothesis that social relationships develop histories because they are characterized by some degree of reciprocity.

Perhaps, more interestingly, the properties of consistency, inconsistency, ambiguity and dominance raise some theoretical questions that must be answered before the definitions and assumptions can be used to make predictions about behavior. (1) What happens when P has adopted two or more exchange rules, which are inconsistent in the goal structure in which he finds himself? (2) What happens when P's exchange rule is ambiguous?

To answer these questions, we need to reconsider the nature of rationality as compared to the other exchange rules. If a goal structure has a dominant rational choice, then any time P makes any other

choice he will experience a cost; that is, he will know he could have gotten more of something he wants if he had made another choice. This means that if P follows an exchange rule other than rationality and both that rule and rationality have dominant choices, then P must incur a cost in order to follow his exchange rule. To complicate matters, there are also goal structures in which rationality has a dominant choice, but being rational in the short run imposes costs in the long run. The well-known Prisoner's Dilemma is an example of such a situation. If the exchange rule P has adopted—whether it be rationality or not—is ambiguous, then coordination between P and O is required for either of them to follow their exchange rules. This is likely to produce costs for both P and O (as in the case of the two gentlemen who bump into each other as each tries to let the other through the door first).

The argument that follows says that at the same time that P tries to follow his exchange rules, he also prefers to avoid costs. When he cannot do both for one of the reasons given above, a tendency to change some aspect of the exchange may be hypothesized. Among the changes P could make are changes in the goal structure or in his evaluations of the outcomes, leaving the exchange, and adopting a different exchange rule. Since it has been assumed that goal structures and values are unchanging and that the exchange continues, only the last possibility will be considered.

*Assumption 2.* P will change from an exchange rule that imposes a cost to one that does not, or that imposes a lower cost.

This assumption has some specific consequences such as:

*Hypothesis 5.* If rationality has a dominant choice, P will tend to adopt rationality.

*Hypothesis 6.* P will change from an exchange rule without a dominant choice to one with a dominant choice.

*Hypothesis 7.* P will adopt a nonrational exchange rule that has a dominant choice if rationality does not have a dominant choice.

*Hypothesis 8.* P will adopt a nonrational exchange rule that has a dominant choice if rationality imposes a long-run cost even though rationality has a dominant choice.

*Hypothesis 9.* A goal structure in which

no exchange rule has a dominant choice will be unstable; no exchange rule will be permanently adopted.

And, in answer to the first of the two questions asked above:

*Hypothesis 10.* If P has adopted several exchange rules which are mutually inconsistent in a given goal structure, he will follow those rules that are consistent with rationality.

Although it is necessary to make some assumption about how people will handle inconsistency and ambiguity, this approach is not the only one possible. An alternative that has some intuitive appeal would be to assume that a person faced with costs in following an exchange rule, or faced with ambiguity, will engage in some kind of compromise between avoiding costs and following his exchange rule. This might take the form of weighting each rule and alternating between behavior consistent with one and with the other according to some pattern or, randomly, without ever changing the rules themselves. Empirical studies are required to choose between these approaches. One argument in support of the approach taken here is derived from dissonance theory (Festinger, 1957) which would predict that people would feel uncomfortable observing the lack of congruence between their norms and their behavior and change one or the other. Some support may also be inferred from results of experimental studies of two-person games (e.g., Rapaport and Chammah, 1965) in which a usual finding is that members of a pair of subjects faced with a conflict situation end up both cooperating or both competing rather than alternating between strategies. It is clear that empirical studies designed to explore this question must be observations of persons making a series of decisions within one goal structure; studying a number of persons making one decision only will not provide information about the processes of changing exchange rules.

#### SUMMARY AND CONCLUSIONS

The theory presented here consists of several discrete parts, linked together by definitions or assumptions. (1) The major theme is the use of a social exchange orientation,

which means that the variables to be considered are rewards and costs. A number of principles have been proposed as governing the transmission of rewards and costs in social exchange—rationality, reciprocity, equity, etc. The first section of the theory is an attempt to give these principles definitions that will make them comparable and useful for further theory construction. (2) One of the logical tools available to social scientists but not often used by sociologists is game theory; the second part of this theory is an attempt to adapt game theory analysis to sociological uses. The crucial links between exchange principles and game theory are the idea of a decision, the notion that exchange principles can be defined as decision rules, and the assumption that exchange rules are social norms. (3) Finally, some discussion is devoted to the problem of deciding when each exchange rule might be expected to apply and the factors influencing shifts from one rule to another. The links here are the properties of ambiguity and consistency and their opposites, derived from the definitions of exchange rules, and the assumption that there is a fundamental tension between avoiding costs and following norms in some situations, depending on the logical structure of the situation and the norms that have been adopted.

As a conclusion, I shall speculate on some of the ways this theory might be used for further theorizing and research. (1) The sets of definitions can be used as a technique of analysis; any social exchange can be thought of in terms of its goal structure, exchange rules, and decisions, and as with any logical system such analysis can help clarify, bring up implicit assumptions and unanswered questions, etc. The use of the notion of a goal structure provides a standard way of comparing exchange situations that retains some of the properties that are unique to each situation. (2) Some of the assumptions point to the need for empirical research; for example, do people actually resolve situations of conflict between norms in the way hypothesized? What exchange rules are characteristic of specific roles, groups, cultures, or personality types? (3) The restrictive initial assumptions that persons do not act to change goal structures, that both participants have complete knowl-

edge of the goal structure and of each other's values and that they agree on the exchange rules can be relaxed to develop theory about the effects of these variables. (4) The analysis can be extended to social exchanges among three or more persons.

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# MEASUREMENT OF RELATIVE VARIATION: SOCIOLOGICAL EXAMPLES

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American Sociological Review 1971, Vol. 36 (June):496-502

*There exists a variety of situations in which the dispersion of data, rather than their mean or other central value, is of interest. The coefficient of variation, which is a measure of dispersion divided by the appropriate measure of central tendency, is preferable to a raw measure of dispersion for this purpose. However, these measures do not have a constant range, and the common practice of dividing the variation value obtained by 100 and expressing the result as a per cent is particularly inappropriate, as all have maxima larger than unity for  $n > 2$ . The present paper provides standardization procedures for coefficients of variation. The resulting standardized coefficients (called S-measures) have a zero-to-one range, and the common practice of dividing the variation value obtained by 100 and deviation) for descriptive purposes is demonstrated. It is suggested that S-measures replace, or at least supplement, coefficients of variation and raw dispersion measures when the problem of interest is relative dispersion between groups.*

## VARIABLES MEASURED BY THEIR DISPERSION

A VARIETY of situations exists in sociological research in which we are interested in the relative dispersion of a data-set rather than in the particular values taken by the data. For instance, researchers interested in the homogeneity of attitudes within a particular subgroup, the extent of convergence or divergence in perception of physical phenomena, the relative balance or imbalance in power among group members, or the uniformity of distribution of income within a specified population, are all, implicitly or explicitly, concerned with relative degree of dispersion.

In such research situations most practitioners either rely simply on graphic techniques (cf. Sherif, 1936; Festinger and Thibaut, 1951) or present their data in the form of variances or standard deviations (cf. Leik, 1965). Recently, techniques have been devised which answer to the aforesaid questions (cf. Leik, 1966; Robinson, 1957), but problems remain in both communicating and interpreting the obtained results.

A measure of variation is required which allows comparison of data with different central tendencies. For example, income variation in a community which has a mean

income of \$10,000 and a variance of \$2,000 may not be compared directly with income variation in a community with a mean income of \$5,000 and variance of \$1,000—it would be incorrect to say that the *relative* variability of the first was twice that of the second. A measure of variability which is often suggested to solve this problem is that variously known as the "coefficient of variation," "coefficient of variability," and the "coefficient of relative variation" (cf. Blacklock, 1960:73-74; Clelland *et al.*, 1966:206; Cooke, 1936:43-45; Dornbush and Schmid, 1955:87-88; Kendall and Stuart, 1958:47; McCormick, 1941:129-131; Mueller and Schuessler, 1961:159-161; Parzen, 1960; Rosander, 1951:81-82; Rozeboom, 1966:46-47; Snedecor, 1956:44; Spiegel, 1961:73-84).

This measure, which we shall call  $V$ , is frequently found in statistical literature and is defined in one of the following ways;

$$V(s) = \sqrt{\frac{\sum(X - \bar{X})^2/n}{\bar{X}}} = \frac{s}{\bar{X}}, \quad (1)$$

$$V(D_{\bar{X}}) = \frac{\sum|X - \bar{X}|/n}{\bar{X}} = D_{\bar{X}}/\bar{X}, \quad (2)$$

$$V(D_{Md}) = \frac{\sum|X - Md|/n}{Md} = D_{Md}/Md, \quad (3)$$

where we have followed the convention of designating the sample standard deviation by  $s$ , the sample mean absolute deviation from the mean by  $D_{\bar{x}}$ ,<sup>1</sup> the sample mean absolute deviation from the median by  $D_{Ma}$ , and sample size by  $n$ .

These equations define the family of measures  $V(d)$ , where  $d$  may be any one of the measures of dispersion defined above. In each case the measure of dispersion is divided by the measure of central tendency about which it is computed, and the result is held to indicate the *relative* scatter of the data. This is certainly an improvement over simply using a dispersion measure without taking central tendency into account; a standard deviation of 5 about a mean of 25 indicates something different from the same standard deviation about a mean of 1000.

#### A STANDARDIZED MEASURE

Some sources (cf. Clelland *et al.*, 1966; Cooke, 1936; Dornbush and Schmid, 1955; Kendall and Stuart, 1958; McCormick, 1941; Mueller and Schuessler, 1961; Snedecor, 1956:62-64; Spiegel, 1961) multiply the right hand side of the equation for  $V(d)$  by 100, and express the result as a percent. This is seldom appropriate. Not only is the maximum of none of these measures equal to 100%, but the maximum of (1) and maximum of (2) are simple functions of  $n$ , while the maximum of (3) is infinite for all  $n > 2$ . A few sources note this difficulty (cf. McCormick, 1941; Parzen, 1960; Rosander, 1951; Rozeboom, 1966), but they do not treat it in detail.

As pointed out in Mueller and Schuessler (1961), and Rozeboom (1966:46-47), and Parzen (1960:213, 379), since interval scales have moveable zero points and ranges, the measures  $V(d)$  are appropriate only for absolute value ratio scale data (magnitude variables), i.e., non-negative real numbers with a meaningful zero. With such data all forms of  $V(d)$  reach their maxima where all cases have zero values save one, i.e., in a monopoly situation (cf. Dodd, 1952).

For (1) the maximum value of  $V(s)$  is

$$\begin{aligned} V_{\max}(s) &= \frac{\sqrt{\frac{1}{n} \left[ \left(k - \frac{k}{n}\right)^2 + (n-1) \left(\frac{k}{n}\right)^2 \right]}}{\frac{k}{n}} \\ &= \sqrt{\frac{\frac{1}{n} \left(k^2 - \frac{k^2}{n}\right)}{\frac{k}{n}}} = k \sqrt{\frac{\frac{1}{n} - \frac{1}{n^2}}{\frac{k}{n}}} = n \sqrt{\frac{n-1}{n^2}} \\ &= \sqrt{n-1}, \end{aligned} \quad (4)$$

where  $k$  is the value of the single non-zero case, and  $n$  is sample size.

For (2) the maximum value of  $V(D_{\bar{x}})$  is

$$\begin{aligned} V_{\max}(D_{\bar{x}}) &= \frac{\frac{1}{n} \left(k - \frac{k}{n}\right) + (n-1) \frac{k}{n}}{\frac{k}{n}} \\ &= \frac{k + (n-2) \frac{k}{n}}{k} = 1 + \frac{n-2}{n} \\ &= 2 \left(1 - \frac{1}{n}\right), \end{aligned} \quad (5)$$

where  $k$  is defined as above. It may also be shown that

$$\lim_{n \rightarrow \infty} 2 \left(1 - \frac{1}{n}\right) = 2, \quad (6)$$

which, as we shall show, gives (2) several advantages over (1), which lacks an asymptote.

The maximum of (3) for  $n > 2$  is infinity in the monopoly situation since the median is zero. [This measure will not be considered further here.] A *mean* of zero, on the other hand, would occur only in the trivial case in which all observations are zero, in which case all  $V(d)$ 's are undefined.

Thus if a researcher desires a measure of relative scatter which may be expressed as a proportion of its maximum, varies from zero to one, and which may be stated as a percent if desired, he may compute the measure  $S(d)$ , defined as

$$S(d) = \frac{V(d)}{V_{\max}(d)}, \quad (7)$$

where  $V_{\max}(d)$  is given by (4) or (5), as the case may be. Such a measure based on

<sup>1</sup> Some others (cf. Mueller and Schuessler, 1961; Zelditch, 1966) use A.D. for the mean absolute deviation.

(1) has been previously suggested by Dodd (1952), but we are unaware of the existence in the literature of its analogue for (2)

#### APPLICATIONS

##### *Norm Formation*

Sherif's (1936, 1958, 1965) classic study provides an example of quantitative data for which the measures discussed may be appropriate.  $V(d)$  gives a measure of the dispersion of the behaviors of group members (in this case, autokinetic judgments). However, the fact that  $V_{\max}$  is different for groups of two individuals than for groups of three is something of an inconvenience. The S-measures, being  $V$ 's set to a common range of zero to one, provide an index of dispersion which can be applied to groups of differing size. Convergence of autokinetic judgments (norm formation) should be seen in the reduction of S's. Table 1 shows the values of  $V(D_{\bar{x}})$  and  $S(D_{\bar{x}})$  for the "crucial" pairs of runs in Sherif's study—those runs where groups are changed from individual to group conditions and vice versa.

Table 1. Variabilities of "Crucial" Cells of the Sherif Experiment.

	(A) Group-Group-Group-Individual	
	Last Group Session	Individual Session
Mean $V$ , $n=2$	.01	.10
Mean $V$ , $n=3$	.04	.14
Mean $S$ , $n=2$	.01	.10
Mean $S$ , $n=3$	.03	.10
	(B) Individual-Group-Group-Group	
	Individual Session	First Group Session
Mean $V$ , $n=2$	.58	.09
Mean $V$ , $n=3$	.38	.14
Mean $S$ , $n=2$	.58	.09
Mean $S$ , $n=3$	.29	.11

In three of the four comparisons in Table 1, the mean value of  $S(D_{\bar{x}})$  for groups of size three is closer in magnitude to the mean

value of  $S(D_{\bar{x}})$  for groups of two than the mean value of  $V(D_{\bar{x}})$  for groups of three is to the mean value of  $V(D_{\bar{x}})$  for groups of two. This does not, of itself, validate the superiority of the S-measures, but if the same processes characterize groups of both sizes, it is reasonable to expect them to exhibit similar amounts of consensus. (Since each cell of the table represents four groups of two and four groups of three, it is not appropriate to demand that all four cells show this closer agreement of S's, just as it is not appropriate to make too much of the fact that three of them do.)

Examining two groups from the condition [B-individual session] with more similar values of  $V(D_{\bar{x}})$  may be of some interest, since this is the anomalous cell. A group of three produced the individual values: .7, 1.9, 7.4,  $V(D_{\bar{x}}) = .81$ ,  $S(D_{\bar{x}}) = .61$ . A group of two produced these individual values: .3, 3.5;  $V(D_{\bar{x}}) = .84$ ,  $S(D_{\bar{x}}) = .84$ . Note that the ratio of the extreme cases is slightly larger in the group of size two than in the group of size three; the value of  $V(D_{\bar{x}})$  for size 2 is likewise slightly larger than  $V(D_{\bar{x}})$  for size three. It seems reasonable, however, to consider that there is less variability in the group of three because there exists an intermediate case; this difference in variability is better reflected in the values of  $S(D_{\bar{x}})$ .

The data in Table 1 permit the tentative conclusion that divergence is taking place in the transition from group to individual conditions (Part A). Sherif's discussion gives no indication that he was aware of any such process; indeed, he has said recently that the group-norm perception is maintained under individual conditions (Sherif and Sherif, 1969:211). Quantification by means of these measures has permitted us to detect a pattern which was less apparent in the original graphical presentation.

##### *Geographic Mobility*

Nations vary in the extent to which their citizens move about during their lives. They may also vary in the extent to which these moves are concentrated in one or another part of the life span. Long (1970) presents data on geographic mobility for four nations, giving per cent of population mobile during

a one-year period, by age. Unfortunately for comparability, the age categories used by different countries are not identical even in their total number. Japan is of special interest here because it is the only non-Western country considered in Long's study.

A suitable measure of variability in mobility would be the dispersion of percent mobile over whatever age categories exist, provided that control for mean value and number of categories is included. The  $S$  measures would appear to be appropriate. Table 2 gives the mean percent mobile, standard deviation, mean deviation, and appropriate  $V$  and  $S$  measures for mobility in the United States, Japan, England and Wales, and Scotland.

The data indicate that both mean mobility and variability in mobility differ for the four countries. The United States has the highest level of mobility, and when raw variability measures are used, it appears to have the highest variability in mobility as well. When  $V$ -measures are computed, Japan moves from least variable to most variable, and the United States and Scotland are discovered to be the least variable. Correction for the mean also leads to the discovery of a negative relationship between the mean mobility and the extent to which mobility decreases with age. The young contribute much more to Japan's low mobility rate than to the United States' high rate (for details see Long, 1970:1197).

When  $S$ -measures are computed, correcting for both the mean mobility rate and the number of age categories, Japan remains the

most variable, but her margin of excess variability over that of England and Wales is seen to be small. In the case of  $S(s)$ , it virtually disappears:  $S(D_{\bar{x}})$  appears to represent the situation more appropriately, as Japan's maximum mobility rate is nearly as high, and her minimum mobility rate less than half as high, as that of England and Wales. (This holds even if Japan's three lowest-mobility age categories, all in old age, are considered, indicating that the effect of the larger number of categories, while present, is small. The correction applied by the use of  $S(D_{\bar{x}})$  is likewise small.)

### Homicide Rates

Gibbs (1968) shows that there is a negative association between homicide rates and the certainty and severity of punishments for homicide. A matter of interest to investigators of relationships such as these is the extent to which regions of the country vary in homicide rates. More generally, variability in homicide rates (like variability in many other indices such as mean income, family size, church membership and educational level) can be taken as an indicator of cultural heterogeneity. Table 3 shows the mean homicide rates for eight commonly-used regional divisions of the United States, and the variability in homicide rates for each of them.

The data indicate that there is variation in homicide rate by region. The regional means range from 1.25 to 9.91, an absolute range of variation greater than is found

Table 2. Variability in Percent Mobile, by Age and Sex, Four Countries.

Country	Mean	$s$	$V(s)$	$S(s)$	$D_{\bar{x}}$	$V(D_{\bar{x}})$	$S(D_{\bar{x}})$
U.S.A.:							
Males	20.60	7.64	.371	.166	7.06	.343	.205
Females	20.77	8.56	.412	.184	7.33	.353	.212
Japan:							
Males	8.12	6.15	.757	.252	5.58	.687	.383
Females	7.17	5.54	.772	.257	4.86	.678	.376
England & Wales:							
Males	10.10	6.35	.629	.281	5.20	.515	.309
Females	13.08	6.05	.462	.207	5.35	.409	.245
Scotland:							
Males	11.47	4.23	.369	.165	4.07	.354	.213
Females	11.70	4.90	.418	.187	4.37	.373	.224

Table 3. Variabilities of Homicide Rates by Region of the U.S.

Region	Mean	s	V(s)	S(s)	$D_{\bar{x}}$	$V(D_{\bar{x}})$	$S(D_{\bar{x}})$
New England	1.25	.263	.210	.094	.233	.186	.111
Mid-Atlantic	2.73	.238	.087	.062	.210	.077	.058
East North Central	4.02	.568	.141	.082	.425	.106	.071
Midwest	2.01	1.33	.661	.250	1.015	.502	.286
Border South*	6.00	1.65	.275	.123	1.30	.216	.129
Old South*	9.91	1.90	.190	.063	1.65	.165	.092
Rocky Mountain	4.60	2.10	.457	.172	1.775	.386	.221
West Coast	2.73	.695	.254	.180	.643	.236	.178

\*Distinguished by whether a state seceded from the Union during the period 1861-65.

within any one region. (Considering the resistance to fluctuation that the mean enjoys, this is rather clear evidence of regional differences.) Of greater interest to our present question of technique is the variation in variabilities. The greater the variability of crime rates within a region, the less homogeneous that region with respect to whatever are the causes of that crime.

There is relative but unequal homogeneity within regions, with the Midwest the least homogeneous region. The use of the V-measures may be seen to improve on the raw variability indices, with the S-measures providing further refinement.

When uncorrected s and  $D_{\bar{x}}$  are used, the South, the Midwest, and the Rocky Mountain states appear to be the least homogeneous regions. When V-measures are computed, the Old South no longer appears to be an especially variable region, while the West Coast approaches the Border South in variability. Correcting for the mean permits one to avoid the mistaken conclusion that the South is unusually heterogeneous, and reverses the order of homogeneity of the two most heterogeneous areas: the Midwest and the Rocky Mountain states. When S-measures are computed (correcting for n as well as for the mean), the Old South is seen to be one of the less-variable regions, while the Midwest, Rocky Mountain states, and the West Coast are seen to be the most variable. Considering the size and economic variability of these regions, and the differences that exist among the states within them, this seems reasonable. The homogeneity of the Old South is similarly in line with intuitive conceptions of this as a uniformly violent cultural area.

In this example, both  $S(D_{\bar{x}})$  and  $S(s)$  were computed. While essentially the same conclusions would be drawn using either base (squared or absolute deviations), the measures based on absolute deviations appear to us to be preferable. Using  $S(D_{\bar{x}})$ , we could conclude that the east North Central states are less variable than the Old South and that the West Coast is less variable than the Rocky Mountain region.<sup>2</sup> The use of  $S(s)$  would lead to the opposite conclusions. We are of the impression, though some may object, that the conclusions reached by the use of absolute-deviation measure are more appropriate.

$S(D_{\bar{x}})$  appears to be generally more satisfactory than  $S(s)$ , as the latter frequently gives minute values if n, the number of cases, is large. Data with different n and the same proportional form give highly similar values of  $S(D_{\bar{x}})$ , while they may give quite different values of  $S(s)$ . The usual advantage of the standard deviation (or the variance)—mathematical simplicity in dealing with probability questions—does not necessarily apply when the data of interest are dispersion measures, though such standard measures of variation may be of use in making probability statements about V-measures or S-measures. Since absolute deviation scores are themselves ratio scale data, their distribution about their own mean may be determined and used to test the sort of statistical questions likely to be asked in a context of interest in dispersion.

#### *Size Distribution of Free-Forming Groups*

The advantage of  $S(D_{\bar{x}})$  over  $S(s)$  is

<sup>2</sup> It should be noted that Alaska and Hawaii are not included in these 1960 data.



clearly illustrated by the skewed data in the following example. Coleman and James (1961) observed the sizes of free-forming groups. As presented, these data do not fulfill the criteria for a ratio scale, since there is no meaningful zero. (How does one observe groups of size zero?) However, by considering the number of "others" in a group (i.e., subtracting one from group size as reported and assigning the value zero to solitary individuals, in effect defining groups to have two or more individuals), these data can be transformed to fulfill ratio-scale criteria.

Examination of Table 4 should convince the reader that this distribution is highly skewed. This skew is reflected in the values of  $V(d)$ :  $V(s) = 1.47$ ,  $V(D_{\bar{x}}) = 1.23$ . Norming by (7) yields  $S(s) = .066$  and  $S(D_{\bar{x}}) = .617$ . The latter S-measure seems to be much more reasonable as an index of relative dispersion.

The vast difference between  $S(s)$  and  $S(D_{\bar{x}})$  stems from the non-asymptotic behavior of  $V_{\max}(s)$ . For the  $n$  reported by Coleman and James,  $V_{\max}(s)$  is 22.27, while  $V_{\max}(D_{\bar{x}})$  is 1.996. The extremely large maximum of  $V(s)$  results from squaring deviations, a procedure whose effect is not fully counteracted by the subsequent "un-squaring" of their sum.

For  $n$  greater than twenty, the effect of increasing  $n$  on  $S(D_{\bar{x}})$  is slight. Values of  $S(D_{\bar{x}})$  are essentially the same for distributions of the same proportional form if the smallest  $n$  is even moderately large. For example, suppose that a distribution of the same proportional form as that reported by Coleman and James was found with an  $n$

of 10,000.  $S(s)$  for this distribution would be .015, which is less than one-fourth the value of  $S(s)$  observed for the Coleman-James data.  $S(D_{\bar{x}})$  for the larger  $n$ , on the other hand, is .615, nearly the same as the value for the observed distribution. The advantage of using  $S(D_{\bar{x}})$  should be obvious.

#### SUMMARY

The V-measures improve upon the standard and traditional measures of dispersion by providing some control for maximum possible variability; they do this in a simple and clear manner by utilizing division by the appropriate measure of central tendency. However, they do it imperfectly in the sense that they do not have a constant range; indeed, the maximum of one of them is infinite, and the maximum of another,  $V(s)$ , approaches infinity as  $n$  increases without limit. S-measures have a range which is independent of  $n$  and thus are useful when different-sized samples or groups are being compared.

S-measures are undefined for average deviation about the median: the fact that this  $V$  has a maximum of infinity for all  $n$  leads us to conclude that variation should not be computed about the median for data which yield an interpretable mean. Comparing  $S(s)$  to  $S(D_{\bar{x}})$ , we find the latter preferable because it yields similar values for distributions of the same proportional form.

We suggest that researchers interested in questions of dispersion, whether their interests lie in norm formation, mobility, cultural homogeneity, etc., focus their attention specifically upon S-type-measures. These not only are useful for answering questions of dispersion, but, additionally, and perhaps most importantly, are easily communicated. Few measurement techniques in social science possess such clear and communicative properties as the measures discussed here.

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Table 4. Size Variability of Natural Play Groups in Eugene, Oregon (Coleman and James).

Number of Others in Group	Observed Number of Groups
0	306
1	132
2	47
3	10
4	2
	<u>497</u>

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### MANUSCRIPTS FOR THE ASA ROSE SOCIOLOGY SERIES

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## NOTES AND COMMENTS

### COMMENT ON "TOWARD A TEMPORAL SEQUENCE OF ADOLESCENT ACHIEVEMENT VARIABLES"

Richard A. Rehberg *et al.* (1970:34-48) have recently argued for a specific causal ordering of adolescent achievement variables.<sup>1</sup> Basically, they attempt to construe their data to support one particular causal ordering of the variables, or, in their words, to make one causal model more theoretically tenable than another. Although there exist other points of disagreement, basic criticisms must be raised regarding their attempt to determine causal ordering. (1) We believe the authors have misinterpreted earlier statements of writers in the area, particularly the theory and conclusions reached in an earlier study by Sewell, Haller and Strauss (1957). (2) The basic objective which they set for themselves cannot be attained in the manner they propose. (3) Their use of the Simon-Blalock approach and path analysis procedures in attempting to determine causal ordering represents a misapplication of the logic of the procedures.

Rehberg *et al.* (1970) are interested in the causal ordering of four variables: parental socioeconomic status, measured intelligence, educational expectations and mobility attitudes.<sup>2</sup> They present two causal orderings of these four variables (p. 35). In Model I mobility attitudes and

measured intelligence are not causally related, but are both directly determined by parental SES. Educational expectations is affected by mobility attitudes and measured intelligence but is not directly affected by SES. Model II is presented as the causal ordering which is "most consistent with the traditional perspective of sociology" (p. 34). Model II features a "reversal of the 'traditional' causal ordering" (p. 35) of the variables, which they credit to Turner (1964). The Rehberg *et al.* adaptation of this model is a fully recursive system in which educational expectations depend on SES; measured intelligence depends on SES and educational expectations; and mobility attitudes depend on all three of the prior variables. The major effort of Rehberg *et al.* is directed toward choosing between these two theoretical models on the basis of cross-sectional survey data.

#### THEORETICAL SUPPORT

Of the references cited for having "implicitly" suggested Model I to Rehberg *et al.*, only Sewell *et al.* (1957) explicitly include measured intelligence in their analysis.<sup>3</sup> Both models which Rehberg *et al.* present for analysis involve the regression of measured intelligence on SES; in Model I, Sewell *et al.* (1957) are incorrectly given credit for the formulation of this ordering. There is no support, implicit or explicit, in the Sewell *et al.* (1957) report for the Rehberg *et al.* causal representation of the relationship between SES and intelligence in Model I. Neither in this research nor in subsequent research by Sewell and his associates (Sewell, 1964; Sewell and Orenstein, 1965; Sewell and Haller, 1965; Sewell and Armer, 1966; Sewell, Armer and Shah, 1966; Sewell and Shah, 1967, 1968; Sewell, Haller and Portes, 1969; Wegner and Sewell, 1970) has measured intelligence been treated as intervening between socioeconomic background and achievement expectation variables. Rather, measured intelligence has been treated as a predetermined exogenous variable along with SES, and the causal nature of the SES; measured intelligence relationship has been left unanalyzed. This has frequently been

<sup>1</sup> The title of the Rehberg *et al.* (1970) article, in addition to references within the text of the article, indicates a concern with the "temporal" sequence of adolescent achievement variables. Yet, the data which they analyze involve variables which are apparently measured at a single point in the life cycle of their respondents. Since the variables are not separated in time, we prefer to use the term "causal" order or sequence, so as not to imply a temporal ordering in the data.

<sup>2</sup> Consistent with the Rehberg *et al.* Models I and II, we will represent mobility attitudes as one variable. However, since there are presumably four distinct mobility attitudes involved in the study, and since there is obvious covariation among them, neither Model I nor Model II is really appropriate. We believe that each of the models, the general one we will propose and the two presented by Rehberg *et al.*, should include the four mobility attitude variables. Even so, we find it expedient to discuss the Rehberg *et al.* report within the context in which it is phrased. Thus, our discussion of their Models I and II is in terms of four, rather than seven, variable models.

<sup>3</sup> Sewell *et al.* (1957) refer to their dependent variables as educational and occupational aspirations, but both variables are operationalized in terms of "plans," consistent with the notion of expectations.

the practice of analysts who have dealt with social background and mental ability variables, e.g., Duncan (1969), Bayer (1969) and Hauser (1969). While Hyman (1966:490, 492) and Strodbeck (1958:136, 159) discuss the relevance of ability variables in the determination of achievement related variables, their discussions do not provide, even implicitly, an ordering of measured intelligence with respect to SES and achievement expectations. Rosen does not even mention ability variables in his discussion of educational aspirations (1956:209).

While the Turner (1964) and Rehberg *et al.* notion regarding the effect of transient (SES-related) motivational factors on measured intelligence is perhaps appealing, it ignores the facts that by late adolescence intelligence is a very stable trait (Bloom, 1964; Duncan, 1968:8) and that SES and measured intelligence have common causes, both genetic and environmental, that produce a somewhat spurious correlation between them (Eckland, 1967: 181, 189-190; Jensen, 1968:15-18). Because of these common causes, the regression of measured intelligence on SES would amount to a misspecification of the causal connection which would lead to a slight upward bias in the estimate. We refer the interested reader to the literature which deals with the specification of the SES-intelligence relationship (Eckland, 1967; Jensen, 1968; Duncan, 1968).

In contrast to the case of measured intelligence, there is support in the writings of Hyman (1966), Rosen (1956), and Strodbeck (1958) for the assumption that mobility attitude variables do mediate the effect of SES on educational expectations. We question whether these writings justify the specification, in Model I, of no direct effect of SES on educational expectations.

The major conclusion of Sewell *et al.* (1957) is that both parental SES and measured intelligence have an independent direct effect on educational expectations. Sewell *et al.* do not explicitly include mobility values in their analysis, although they do associate "achievement values" with SES (1957:68, 73). However, their discussion on this point is brief and equivocal, and it is impossible to exclude the direct path from SES to educational expectations in light of their findings. In fact, Sewell and his associates have subsequently attempted to find variables which mediate the effect of SES on educational expectations, e.g., parental encouragement (Sewell and Shah, 1968), influence of significant others, and high school rank (Sewell, Haller, and Portes, 1969). Still, they continue to find a direct effect of SES on educational

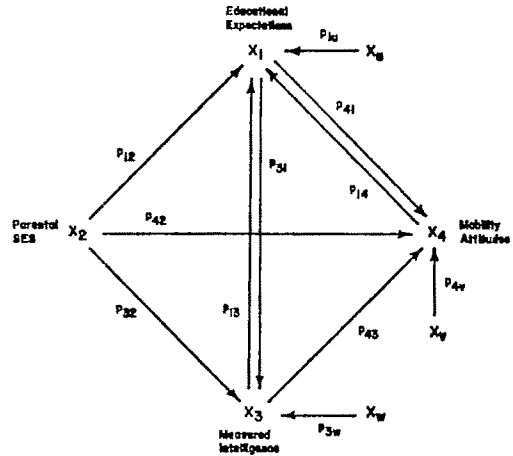


FIGURE 1. GENERAL MODEL WHICH COMBINES MODEL I AND MODEL II.

expectations (Sewell and Shah, 1968:567; Sewell, Haller and Portes, 1969:88).

#### THE LOGIC OF CAUSAL INFERENCE

Rehberg *et al.* (1970) state that their purpose is "... to focus recently collected survey data to each of these two temporal models in an effort to ascertain which is more congruent with the empirical territory (sic)" (p. 35). This essentially involves a determination of (1) whether mobility attitudes intervene between SES and educational expectations (Model I), or educational expectations intervene between SES and mobility attitudes (Model II); (2) whether measured intelligence directly causes educational expectations (Model I), or vice versa (Model II); (3) whether SES has a direct effect on educational expectations (Model II) or not (Model I); and (4) whether measured intelligence has a direct effect on mobility attitudes (Model II) or not (Model I).

The possibility of completing these four tasks may be conveniently discussed in terms of a general path model which merges the Rehberg *et al.* Models I and II while maintaining the initial form of each.<sup>4</sup> The path diagram in Figure 1 represents such a model. The nonzero paths of their Model I are denoted by  $p_{12}$ ,  $p_{14}$ ,  $p_{22}$ , and  $p_{24}$ , while the nonzero paths for Model II are  $p_{31}$ ,  $p_{32}$ ,  $p_{34}$ ,  $p_{41}$ ,  $p_{43}$ , and  $p_{44}$ . If one were to estimate this model and find that  $p_{12}$ ,  $p_{31}$ ,  $p_{41}$ , and  $p_{44}$  were near zero (within the limits of sampling error) and  $p_{13}$ ,  $p_{14}$ ,  $p_{22}$ , and  $p_{24}$  were nonzero, support for Model I would have been obtained.

<sup>4</sup> We are indebted to Professor Robert M. Hauser for suggesting the form of the argument presented here, as well as for other helpful suggestions throughout the entire comment.

Similarly, if one found  $p_{13}$  and  $p_{14}$  to be near zero and the remaining paths to be nonzero, Model II would have received support. But such results cannot be obtained because the model is underidentified; eight parameters are to be estimated while only six observed correlations are available. (See Chart 1.) In other words, there are six independent equations for solving eight

unknowns and no unique solution is possible.<sup>5</sup>

Our analysis up to this point reflects the need to separate the issue of the causal ordering of variables and the issue of the causal effect of one variable on another. These issues must be treated separately in a recursive model. The coefficients in a recursive system cannot be identified without assumptions regarding the causal ordering of the variables, and the only possible inference is that, under a given postulated ordering, a particular path might be zero, i.e., there is no causal effect. Advice in this issue is abundant in the sociological literature on causal models and path analysis (Blalock, 1968; Duncan, 1966; Heise, 1968; Duncan, Featherman and Duncan, 1968). In contrast, in a nonrecursive model, for pairs of variables with reciprocal effects, these two issues become one, i.e., the issue of causal order is reducible to the issue of whether one of the reciprocal paths equals zero.

Returning to the underidentified general model in Figure 1, we note that a solution is possible if restrictions in the form of specific assumptions are placed on this model. One type of assumption consists of setting some of the paths equal to zero, although other restrictions such as assuming that proportionalities hold among certain coefficients may be introduced. Either of these procedures will produce a large number of identifiable models. Rehberg *et al.* (1970) have generated two such models by assuming some of the paths to be equal to zero. However, it is important to emphasize that a large number of models may be equally consistent with one's data, and in order to estimate some models one must assume others to be incorrect (Duncan, 1969). In particular, to estimate Model I, Model II must be assumed to be incorrect, and vice versa. The estimation of one model offers no empirical "test" of the truth or falsity of the other.

We would not deny that *under certain conditions* it is possible to find support for one theoretical model in the rejection of a competing model. In particular, if one has two *strongly supported* theoretical models which differ *only* in the presence or absence of a specific path or paths, the rejection of one model would lend support for the other. By "rejection" we mean that certain paths predicted to be zero are not zero, and/or paths theorized to be nonzero are in fact zero. This is precisely the type of situation to which Blalock (1964) refers. However, this is not the situation for the two models presented by Rehberg *et al.* Their Model I and

CHART 1. STRUCTURAL EQUATIONS AND PATH ESTIMATION EQUATIONS

#### General Model

##### Structural Equations:

$$X_1 = p_{12}X_2 + p_{13}X_3 + p_{14}X_4 + p_{1u}X_u$$

$$X_2 = p_{22}X_2 + p_{21}X_1 + p_{2w}X_w$$

$$X_4 = p_{41}X_1 + p_{42}X_2 + p_{43}X_3 + p_{4v}X_v$$

##### Path Estimation Equations:

$$r_{12} = p_{12} + p_{12}r_{22} + p_{14}r_{24}$$

$$r_{22} = p_{22} + p_{21}r_{12}$$

$$r_{24} = p_{24}r_{12} + p_{12} + p_{24}r_{22}$$

$$r_{13} = p_{13}r_{23} + p_{12} + p_{14}r_{24}$$

$$r_{14} = p_{41}r_{12} + p_{21}$$

$$r_{14} = p_{14}r_{24} + p_{12}r_{24} + p_{14}$$

$$r_{14} = p_{41} + p_{43}r_{12} + p_{43}r_{22}$$

$$r_{12} = p_{41}r_{21} + p_{43}r_{22} + p_{43}$$

$$r_{11} = 1 = p_{12}r_{12} + p_{13}r_{13} + p_{14}r_{14} + p_{1u}^2$$

$$r_{22} = 1 = p_{22}r_{22} + p_{21}r_{21} + p_{2w}^2$$

$$r_{44} = 1 = p_{41}r_{41} + p_{42}r_{42} + p_{43}r_{43} + p_{4v}^2$$

#### Model I

##### Structural Equations:

$$X_4 = p_{42}X_2 + p_{4v}X_v$$

$$X_3 = p_{32}X_2 + p_{3w}X_w$$

$$X_1 = p_{12}X_2 + p_{13}X_3 + p_{1u}X_u$$

##### Path Estimation Equations:

$$r_{14} = p_{42}$$

$$r_{22} = p_{22}$$

$$r_{12} = p_{12}r_{22} + p_{14}r_{24}$$

$$r_{24} = p_{42}r_{22}$$

$$r_{13} = p_{12} + p_{14}r_{24}$$

$$r_{14} = p_{14} + p_{12}r_{24}$$

$$r_{11} = 1 = p_{12} + p_{13}r_{13} + p_{1u}^2$$

$$r_{22} = 1 = p_{22}r_{22} + p_{2w}^2$$

$$r_{44} = 1 = p_{42}r_{42} + p_{4v}^2$$

#### Model II

##### Structural Equations:

$$X_1 = p_{12}X_2 + p_{1u}X_u$$

$$X_3 = p_{32}X_2 + p_{31}X_1 + p_{3w}X_w$$

$$X_4 = p_{42}X_2 + p_{41}X_1 + p_{43}X_3 + p_{4v}X_v$$

##### Path Estimation Equations:

$$r_{12} = p_{12}$$

$$r_{13} = p_{31}r_{12} + p_{31}$$

$$r_{22} = p_{22} + p_{21}r_{12}$$

$$r_{24} = p_{42} + p_{41}r_{12} + p_{43}r_{22}$$

$$r_{14} = p_{42}r_{12} + p_{41} + p_{43}r_{22}$$

$$r_{24} = p_{42} + p_{43}r_{22} + p_{41}r_{12}$$

$$r_{11} = 1 = p_{12}r_{12} + p_{1u}^2$$

$$r_{22} = 1 = p_{22}r_{22} + p_{21}r_{21} + p_{2w}^2$$

$$r_{44} = 1 = p_{42}r_{42} + p_{41}r_{41} + p_{43}r_{43} + p_{4v}^2$$

<sup>5</sup> Although two correlations,  $r_{12}$  and  $r_{14}$ , are expressed in terms of two equations each, these equations are equal.

Model II are different, not only with respect to the presence or absence of specific paths, but also with respect to the causal ordering of variables. Finding, for example, that the two predicted zero paths in Model I are in fact nonzero does allow for the inference that Model I is inadequate. In no sense does this result confirm Model II. A plausible alternative suggested by these empirical findings would be the addition of the significant nonzero paths, which would not require us to reorder the variables. Only if one set of estimates leads to grossly improbable interpretations would one be led to reorder the variables.

#### THE EXAMINATION OF PARTIALS

While it is apparent that the objective which Rehberg *et al.* set for themselves is *not* attainable by the method they prescribe, we shall nonetheless review their misapplication of the Simon-Ballock approach. Since Model I is not fully recursive, and thereby overidentified, the Simon-Ballock procedure for examining partial correlations (Ballock, 1968:168) yields two statistical predictions: (1)  $r_{12.3} = 0$  or  $r_{23.1} = r_{34}$ , and (2)  $r_{12.4} = 0$ . Rehberg *et al.* never mention the first prediction, and only one of their several predictions for this model bears any resemblance to the second (see below). Model II represents a fully recursive set of equations and is therefore just-identified, given the assumption of uncorrelated errors. For such a model there is no "test" involving partial correlations in the Simon-Ballock sense. (Ballock, 1968:168; Heise, 1968:63).

Given the above statements, it is of interest to inquire about the source of the "predictions" which Rehberg *et al.* make purportedly on the basis of the Simon-Ballock approach. The following statement is used by Rehberg *et al.* as the basis for a number of their "predictions":

In systems involving relatively simple temporal sequences, the correlations between variables which are separated by one or more intervening variables will be less than the correlations (or average thereof) between variables which operate directly upon one another (1970:36),

referring the reader to Ballock (1964:68) and Turner (1964:51). Thus, for Model I they predict  $r_{12} < r_{13}$  and  $r_{12} < r_{23}$ , and  $r_{13} < r_{14}$  and  $r_{13} < r_{34}$ , and for Model II  $r_{23} < r_{13}$  and  $r_{34} < r_{12}$ , and  $r_{34} < r_{14}$  and  $r_{24} < r_{12}$ .<sup>6</sup> However, Ballock (1964:68) and Turner (1964:51) discuss simple causal chains of the form:  $X_2 \rightarrow X_3 \rightarrow X_1$ , in

which there is no direct path specified from  $X_2$  to  $X_1$ . For this model it is true that  $r_{12} = p_{21}p_{12} = r_{23}r_{13}$ , given the assumption of uncorrelated errors.

We assume this is the source of the Rehberg *et al.* predictions, since, if in Model I,  $r_{12} = r_{23}r_{13}$ , then it follows that  $r_{12} < r_{23}$  and  $r_{12} < r_{13}$ . But, this also requires that no other variable,  $X_4$ , intervene between  $X_2$  and  $X_1$ ; so it cannot also be true that  $r_{12} < r_{14}$  and  $r_{12} < r_{24}$ . This can be seen in the path estimation equations for Model I in Chart 1. If the simple causal chain fits the relations between  $X_2$ ,  $X_3$ , and  $X_1$ , then either  $p_{14}$  or  $p_{24}$ , or both, must by necessity equal zero (assuming all positive paths), in which case the original model (Model I) is no longer the one under consideration. In short, the reduction of Model I to a set of two three-variable causal chains is *reductio ad absurdum*; the truth of one three-variable chain makes necessary the falsity of the other.

Except in the simple causal chain model, the prediction,  $r_{12} < r_{23}$  and  $r_{12} < r_{13}$ , is inappropriate, since, in a more complex model, e.g., Model I or Model II, the value of  $r_{12}$  is not equal to the product  $r_{23}r_{13}$  (see Chart 1). It appears that Rehberg *et al.* have based most of their "predictions" (for the one exception, see below) on a consideration of two three-variable models for each of Models I and II. We assert that this is totally inappropriate. Basically, any prediction about the structure of the relationships among  $X_1$ ,  $X_2$ , and  $X_3$  in Model I must consider  $X_4$  as well. Any regarding  $X_1$ ,  $X_2$ , and  $X_4$  must consider  $X_3$ . We have already noted there are no such predictions for Model II.

The third set of predictions that Rehberg *et al.* make from Model I and Model II, namely  $r_{12.3} < r_{13}$  and  $r_{12.4} < r_{13}$  (Model I), and  $r_{23.1} < r_{23}$  and  $r_{34.1} < r_{13}$  (Model II), are interesting in that if the control variable in a given partial correlation correlates positively with either of the other two variables, the partial must be less than the zero-order. Thus, these "predictions" merely state what is obvious from the models anyway, and as discussed before, they have no place in a model (Model I) which posits two variables intervening between  $X_2$  and  $X_1$  (Ballock, 1964:67), or in a model (Model II) which is fully recursive (Ballock, 1968:168).

This brings us to the fourth prediction which Rehberg *et al.* make for Model I,  $r_{12.4} < r_{12}$ , the only one which is congruent with the Simon-Ballock procedure. However, even here, the approach would require the prediction to be stated in a more exact form:  $r_{12.4} = 0$ . Moreover, as noted regarding the first-order partial predictions, the use of the inequality sign in the prediction for the second-order partial does little more than state the obvious.

<sup>6</sup> We have been unable to find mention of this procedure of averaging in the Ballock and Turner references or elsewhere, nor can we think of any rationale for it.

As we noted earlier, because Model I is not fully recursive, it is possible to apply the Simon-Blalock approach. What, then, is the meaning one should attach to the results obtained in correctly "testing" the model? First, if  $r_{12.34} \neq 0$ , given the specification of the model, one might conclude that SES has a direct effect on educational expectations, i.e.,  $p_{12} \neq 0$ . Second, if  $r_{24.13} \neq 0$ , again given the ordering of the variables, i.e., no specification errors, this is indicative of a direct causal relationship between  $X_2$  and  $X_4$ , or, if one is unwilling to make a determination as to the direction of causation, the situation can be handled by specifying a correlation between  $X_2$  and  $X_4$  (see Duncan *et al.*, 1968:39, Figure 2.3.2(d)).

To conclude from a significant partial that the causal ordering of the variables is necessarily incorrect is entirely inappropriate and unjustified. In order to estimate a model of this sort, one is forced to make "causal ordering" assumptions. Then, given those specifications, the failure of a specific partial correlation to disappear might indicate, among other things: (1) the presence of a direct path in the true model, (2) the presence of an unmeasured intervening variable in the true model, (3) the presence of an unmeasured common cause in the true model, or (4) the presence of random measurement error in the control variables. Now, it is possible that one's causal assumptions are wrong, but ordinarily one will want to argue this with outside information, e.g. relevant theory or experimental evidence.<sup>7</sup>

Rehberg *et al.* present the results of a path analysis for Model I as a means of substantiating the inferences they draw from their application of the Simon-Blalock procedure. With regard to "causal inference," there is no difference between path analysis and the Simon-Blalock procedure for examining partial correlations; if a path is zero, the relevant partial correlation is also zero (see Linn and Werts, 1969). Thus for their purposes the additional use of path analysis is superfluous. But again the technique is misapplied. In their Table 4, for each different mobility attitude, the path coefficients for two fully recursive models are presented. Yet, it is clear that Model I is not fully recursive. In particular, it is interesting that  $p_{23}$  is estimated, yet neither in their theoretical discussion nor in their Figure 1 is there any reference to any direct causal connection between  $X_2$  and  $X_4$  in Model I. In order to

compute the coefficients presented in the left panel of Table 4, Rehberg *et al.* have thus implicitly specified some causal assumptions not made explicit in their Figure 1. The appropriate path estimation equations for Model I are presented in our Chart 1.

Further, in discussing the results of the path analysis for their Model I, they note: "The key path is between expectations and status,  $p_{12}$ . The coefficient for this path represents the direct influence upon expectations of status after the influence of intelligence and the particular mobility attitude has been removed" (p. 39). This "finding" is presented as evidence for the rejection of a model attributed in part to Sewell *et al.* (1957), yet, as we noted earlier, this nonzero path is merely evidence of what the Sewell *et al.* findings would have suggested as a specification of the model.

In conclusion, we suggest that instead of setting up what is essentially a "straw man" (Model I) and engaging in a specious argument that their data indicate another model to be more correct, Rehberg *et al.* would have been better advised to simply estimate and interpret Model II, after having justified it on theoretical grounds. This makes sense because they obviously prefer Model II, and because Model I has not been seriously proposed elsewhere. Their estimation of Model II, and subsequent interpretation of the results, would have been more compelling in the absence of a prior presentation of Model I and the needless use, or misuse, of the Simon-Blalock procedures.

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<sup>7</sup> It seems reasonable to assume that the Rehberg *et al.* use of partial correlations to infer causal ordering was influenced by Turner who utilized the same logic in his analysis (1964:51-52, 95-96, 106-107).

\* Both authors are supported by the National Institute of General Medical Sciences grant No. GMO-5526.

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#### RESPONSE TO "COMMENT ON 'TOWARD A TEMPORAL SEQUENCE OF ADOLESCENT ACHIEVEMENT VARIABLES'"

It is impossible to take issue with Alwin and Mueller's critique of our statistical procedures, and their observations serve to codify much that the authors have learned since the publication of the paper. There are, however, several points raised by the critique toward which we wish to direct brief comments.

First, with respect to the construct of intelligence, Alwin and Mueller take us to task for: (1) incorrectly giving credit to Sewell *et al.* (1957) for the formulation of the ordering of the variables in Model I; (2) ignoring the stability of intelligence during adolescence; and (3) regarding IQ as an endogenous rather than as an exogenous variable. While we agree with Alwin and Mueller that, in publications subsequent to 1957, Sewell and his associates have



been unambiguous in their placement of intelligence as a correlate of status rather than as a variable intervening between achievement expectations and status, we cannot so easily agree that in the 1957 Sewell *et al.* paper "there is no support, implicit or explicit, . . . for the causal representation of the SES, measured intelligence relationship in the Rehberg *et al.* Model I." We suggest that the text of the 1957 paper does indeed leave unanswered the question of whether intelligence is to be regarded as an exogenous or endogenous variable in achievement systems. A quotation from that paper is appropriate:

In particular, a number of studies have shown that educational and occupational aspiration levels are positively correlated with intelligence, and others have shown that intelligence is similarly related to social status. On the basis of such studies, it may be reasoned that the apparent effects of parental social status on the youth's levels of aspiration may be due to the common relationship of these variables to intelligence. The democratic ethos notwithstanding, careful studies have shown that in general those of lower intelligence tend to be disproportionately concentrated in the lower social classes; and those of lower intelligence have been shown to have lower levels of educational and occupational aspiration. Thus, the apparent relationship of the youth's educational and occupational prestige aspirations to the values of his social status situation may in reality be a simple reflection of differences in intelligence. (Sewell *et al.*, 1957:68)

With respect to the stability of intelligence during adolescence, we argue with no one. Our discussion of "ambition" as a plausible source of variation in intelligence, while making reference to Turner's (1964) causal reasoning of IQ in his adolescent sample, focused primarily on childhood, a fact apparent from our reference to Moss and Kagan's (1961) longitudinal study of their sample cohort from childhood to adolescence, and to our citations of the third to sixth grade studies of Hoehn (1963) and Davidson and Lang (1960).

Moreover, we have been persuaded by the cogent reasoning of Eckland (1967), Duncan *et al.* (1968), and by Jensen's provocative paper (published after "Toward a Temporal Sequence . . ." was in press) of the merit in regarding intelligence as a predetermined exogenous variable correlated with but not "determined by" status in achievement systems.

The second issue we wish to address is the apparent misconstruction Alwin and Mueller have accorded what we had thought was the rather moderate tone in which our paper had been cast. Although Alwin and Mueller employ several times throughout their critique the term "test" in quotes, thereby seemingly attributing

the use of that term to us, we did not state our objective as being that of "testing" each of the two models. Rather, our clearly stated purpose was "to evaluate the 'goodness of fit' of the data to each model." And, while we concur with Alwin and Mueller that such a purpose was ill-conceived with the procedures we employed, we do register our disappointment with their attribution to the paper of such a categorical phrase as "in no sense does this result *confirm* Model II" (emphasis ours), and their imputation that we concluded "from a significant partial that the causal ordering of the variables is *necessarily incorrect*. . . ." (emphasis ours). No place in the paper did we ever state that a model was "confirmed" or rejected. And no place did we ever conclude that a causal ordering was "necessarily incorrect." Perhaps the use of such categorical language is their "straw man" whose existence is rendered spurious when judged by the tone of the original paper.

The third point which draws our comment is the frequent reference by Alwin and Mueller to the continued persistence of a residual or direct effect between achievement expectation and socioeconomic level, a persistence which (they note) itself persists in the writings of Sewell *et al.* We are well aware that, in most studies of adolescent achievement (and adult achievement as well), there remains a residual relationship between the measure of achievement and status after the effects of plausible intervening variables have been partialled out. But, to label that residual as a "direct effect" of status on achievement without further amplification is to us uninformative. Such a direct effect, we suggest, is most appropriately understood as a quantitative coefficient of our collective ignorance, an ignorance of those variables in addition to the several already identified by Sewell *et al.*, by Duncan *et al.*, and by ourselves and others which "intervene" between achievement and status, or, in the more recent words of Rosenberg (1968), variables which are the "components" of status. When such intervening or component variables have been identified and operationalized, then we may anticipate that their inclusion in a model will virtually eliminate any residual or direct relationship between achievement and status. Such a complete interpretation, of course, will provide sociologists with the knowledge necessary for an understanding of the "how and why" higher family status levels come to be translated initially into higher adolescent achievement intentions and subsequently into higher adult achievement behavior. We would like to think that our original paper, the comments of Alwin and Mueller, and this

response may serve to advance that objective both methodologically and substantively.

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#### COMMENT ON "RESPONSE BIASES IN FIELD STUDIES OF MENTAL ILLNESS"

Phillips and Clancy (1970) deal with a number of important issues in their recent article "Response Biases in Field Studies of Mental Illness." The authors suggest that two forms of response bias, social desirability responding

and naysaying, gravely distort scores on the Langner mental health inventory (Langner, 1962). They suggest that such distortions may be nonrandomly distributed across levels of SES, and thus explain numerous findings of negative correlations between social class and mental illness. Their concern with response bias in such studies is exceedingly important. Such biases, if they can be shown to exist, must be identified, understood, and controlled, either statistically or by modifying the instruments in question. I will argue, however, that the above article fails to illustrate any contribution of response bias or faking to scores on the Langner inventory. This failure stems from several weaknesses in approach and method.

First, the authors do not clearly define "response set" or the general range of implications they might draw from correlations between mental health and response set indicators. Rather, the *a priori* position is taken that association means error. Not considered are alternate explanations: [1] that the criterion variable might be a cause of the response set; [2] that the response set is contaminated with the criterion variable; or [3] that the response set and criterion variable may both be influenced by some third factor. Their orientation precludes substantive interpretation of correlations as reflections of relationships between mental health and other, unspecified personality characteristics measured by the response set indices.

Second, the authors apply their social desirability index to a problem which it cannot resolve, that of estimating bias in actual responses. Social desirability responding results from anxiety produced in the interview. Respondents are asked many questions which may arouse shame or defensiveness, and as a result some respondents "fake good." Such faking may take many forms. A person may erroneously report voting in a recent election. A respondent may overestimate his parents' education. Or, in a field study of mental illness, a respondent may underestimate the occurrence of certain stress symptoms. These factors, however, may be partly controlled by the researchers, a fact which undermines the validity of social desirability response set indicators.

Researchers conducting field studies of mental health, or other surveys, recognize that certain precautions (for example, assurances of anonymity, explanations about the study, and careful planning of the interview) will greatly reduce response error. Response error is also reduced by strong interviewer-respondent rapport. Social desirability is therefore a *dynamic* product of the interview situation. However, social desirability response set indicators, such

as that used by Phillips and Clancy, give a more *static*, stable datum: a respondent's *attitude* toward a question stimulus. While a social desirability index gives static predictions of response tendencies, the individual's social desirability response behavior may vary considerably. A nonsituational, static indicator of social desirability responding, therefore, cannot be reliable or valid. Moreover, such an index, as Edwards has noted, cannot be used to distinguish true from distorted responses: "Whether a subject's answer to a personality item is dishonest or inaccurate can be determined only by means of some criterion by which the truthfulness of the response may be evaluated" (Edwards, 1967:70).

Third, the authors present a "social desirability" index which is of questionable validity. The authors state that: "Social desirability of the items was measured by asking respondents to rate each of the 22 items on a 9-point desirability scale. Respondents were asked to look at the numbers 1 through 9 on their telephone dials. The more desirable they thought a given symptom was, the higher the number they were asked to give it. The less desirable they thought it, the lower the number they were asked to give each symptom. This is a procedure somewhat similar to that developed by Edwards. . . ." (Phillips and Clancy 1970:507) The authors, however, fail to consider the differences between desirability and social desirability. Is an index of *desirability* the same as *social desirability*? Different attitudinal orderings and behavioral consequences seem to be implied by the two terms.

The "social desirability" index created by Phillips and Clancy is, they suggest, "somewhat similar to that developed by Edwards" (1970:507), yet their index is a radical departure from Edwards'. According to Edwards, "The SD [social desirability] score is based upon the number of socially desirable responses a subject gives in self-description" (1957:28). The Phillips and Clancy index, however, indicates nothing about the respondent's actual response behavior. Thus individuals with high "social desirability" scores might or might not give socially desirable responses. There is no way the reader (or the authors) *could* know on the basis of the data given.

The index described by Phillips and Clancy assesses only the *desirability* of the questions. Such data may be important in its own right, but not as indicative of social desirability responding, or any other methodological artifact. Such an index is purely evaluative: an index of attitudes towards symptoms of mental ill-

ness. Had they presented the more conservative view—that attitude toward mental health is a good indicator of social desirability response *potential*—their conclusions might have been more soundly based. Factor analysis of items of the Langner scale and the "social desirability" scale would have been an appropriate and useful approach to validating the social desirability index. Loadings of the 44 items on similar factors might have left the social desirability explanation "in the running," whereas distinct patterns might have allowed its rejection. This, however, would still leave the more basic question unresolved: does a correlation between the two scores imply that attitude toward mental health affects mental health, or the reverse that mental health affects attitude toward mental health? The latter interpretation, consistent with cognitive dissonance theory, seems more plausible. An individual experiencing symptoms of psychological stress is likely to rationalize these symptoms, to decide that having an upset stomach or severe headache *isn't* so "bad," or even "unusual."

Fourth, interpretation of the yaesaying-naysaying response set data is clouded by the questionable response set index. As Phillips and Clancy point out: "It seems apparent from these results that people who are 'naysayers' are either in very much better mental health than others or, . . . because they are naysayers deny symptoms which they may actually experience" (1970:510). Naysaying is composed of "no" or "never" responses to items such as: "I have had at least one cold during the last 10 years." "Have you ever been bothered by an upset stomach?" or "Have you ever felt grouchy or irritable?" Yaesaying is measured by "yes" or "always" responses to items such as: "My teeth sometimes itch," "Sometimes my lungs feel empty," and "My heart sometimes stops beating for a few minutes."

Many people might answer the first question with "no" or "never" after careful and honest reflection. Colloquially, "no" may mean "mostly not," and "never," "hardly ever." The naysaying index is therefore simply another index of mental health. Yaesaying, on the other hand, is much more than acquiescence. There are people who respond "yes" honestly and thoughtfully when asked if their lungs feel empty or if their teeth itch or if their heart ever stops beating. But this index reflects more than yaesaying, and even more than mild psychological stress—it may indicate severe psychological impairment. Again, factor analysis of the yaesaying-naysaying items might have illustrated the dependence of these items on gen-

eral mental health. This researcher is confident that the yaesaying-naysaying items would load highly on a general factor of mental health.

My conclusion is that Phillips and Clancy cannot explain the relationship between SES and mental health as a result of methodological artifacts. They have not measured such artifacts and are thus unable to test the hypotheses that response set tendencies are nonrandomly related to SES and mental health.

Response sets present an important problem for field studies of mental health and survey research in general. Yet most response set research has been based on highly contaminated or unreliable response set indicators. Block's (1965) work with the MMPI, for example, suggests that Edward's social desirability interpretation of MMPI and other tests is erroneous. Other researchers (Rorer, 1965; Rokeach, 1967) have also rejected what has been called "the great response style myth."

Three prerequisites must be dealt with before response set studies such as that reported above can provide credible findings: (1) Indices of response set tendency must measure what they intend [social desirability responding, for example, and not attitudes toward symptoms of stress]. (2) Such research must consider the dynamics of response set behavior. Response sets are situational: sometimes they are elicited and sometimes they are not. Static measures which are item-oriented rather than situation-oriented [and which ignore the importance of real-time causal sequences] will not reflect response set tendencies. This suggests that valid indices of response set tendencies can only be developed in experimental research. (3) Much research suggests that response sets comprise interesting and possibly important psychological data. In the study discussed above, for example, a correlation between a valid indicator of social desirability responding and mental health would have added greatly to our understanding of both phenomena. When they are validly measured, response sets must be accorded a place in psychological theory commensurate with their demonstrated predictive and explanatory significance.

Until such rigor characterizes response set research, and until such research indicates otherwise, researchers conducting field studies of mental health and other social-psychological phenomena must proceed with a cautious but unconvinced attitude regarding the effects of response bias on measures such as the Langner mental health inventory.

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## REPLY TO HARVEY

We appreciate the opportunity to respond to Harvey's comments on our article, "Response biases in field studies of mental illness" (Phillips and Clancy, 1970). Most of his criticisms, however, concern issues which we raised in an earlier draft of the paper (Clancy and Phillips, 1968) and in more recent work (Clancy, 1971; Clancy and Garsen, 1970; Phillips, 1971; Phillips and Clancy, 1971a, 1971b). Since these materials were not available to Harvey, some introductory remarks are called for.

The study under scrutiny here was the first of a series of investigations we have been conducting to explore bias and invalidity in survey research. As is probably the case with most published articles, the paper that was published in the ASR is a considerably abbreviated version of the 65-page manuscript that was originally submitted (Clancy and Phillips, 1968). Not surprisingly, we were asked to cut its length and resubmit it. We shortened it to about 35 pages, eliminating much of the theoretical rationale for our research and speculations (including competing hypotheses) regarding our findings. Later we were asked to be even more brief, and we complied. We mention this not as a criticism of the ASR editors (whose requests for abbreviation we freely chose to follow) but rather because we were

aware of at least some of the problems that Harvey raises.

It is also important to note that our paper was not intended to answer all questions concerning response biases. Rather our purpose as outlined on pp. 505-506 was to extend the work of Dohrenwend, who in his provocative 1966 ASR article raised the spectre of response biases in field studies of psychiatric disorder. Finding that "social desirability" and "acquiescence" were directly related to scores on Langer's 22-item screening inventory and inversely related to socioeconomic status, Dohrenwend questioned whether the frequently found inverse relationship between symptom scores and socioeconomic status was simply a spurious relationship, brought about by a systematic response bias. Our primary concern, then, was to test this hypothesis, not to dwell at length on the response biases themselves, a topic which Harvey would seemingly have preferred.

Now let us turn to Harvey's comments. He points to four weaknesses in approach and method in our research. We will list them and attend to each in turn.

(1) We did "not clearly define 'response set' or the general range of implications (we) might draw from correlations between mental health and response set indicators." With regard to "response set," he is correct. We did not provide a clear definition. While we regret this omission from our article, we would today choose not to speak at all of response set. As the term is used by psychologists, it suggests some relatively enduring characteristics of individuals. It is our view, and apparently Harvey's as well, that such potential sources of bias as acquiescence and trait desirability (which will be discussed shortly) are largely dependent on a complex mixture of factors in social science research, including: the research setting, the topic of inquiry, attributes of the interviewer and respondent (age, sex, race, etc.), and a whole host of other factors (for an enumeration of these, see Phillips, 1971). That is, we feel that people "fake good," "lie," or "distort" their responses more because of assessments of the social situation (and all that it entails) in which they find themselves than because of some enduring personality characteristic.

However, Harvey is incorrect when he claims that we have taken the *a priori* position that association (correlation) means error. A closer inspection of our paper will reveal, for example, that it was concerned with testing the two hypotheses outlined by Dohrenwend (1966) regarding the correlation between "social desirability" and symptom scores. The first hypothesis posits that association may mean error, while

the second hypothesis views social desirability and symptom scores as indicative of actual rates of disorder. Moreover, in our earlier version (Clancy and Phillips, 1968) and in subsequent work (particularly Clancy, 1971, and Phillips and Clancy, 1971b), several explanations for the relationship between response biases and survey responses are discussed.

(2) We "apply (our) social desirability index to a problem which it cannot resolve, that of estimating bias in actual responses." We agree with Harvey that a social desirability index cannot estimate bias. Rather, as discussed in some detail in Clancy (1971), it estimates potential bias. Second, we are also in basic agreement with Harvey that response biases must be considered as part of the gestalt of the interview. As outlined above and in our original paper (Clancy and Phillips, 1968), we view an interview as being a "miniature social situation" (Hyman, 1949). However, we fail to see where it has been demonstrated in the literature that the sociologist's "control" over the interview situation reduces "socially desirable" responding and thus reduces error. Harvey states, for example, that response error is reduced by strong interviewer-respondent rapport. Yet Weiss' (1968) findings suggest precisely the opposite: the better the rapport between interviewer and respondent, the greater the proportion of biased responses.

(3) We "present a 'social desirability' index which is of questionable validity." If by validity, Harvey means the index's ability to measure dissimulation directly, we agree. For as indicated above, we believe, as he does, that the index measures potential bias.<sup>1</sup> We agree with Harvey, though, that it is important to distinguish between "social desirability" and the desirability of traits or symptoms, and in subsequent writings (Clancy, 1971; Phillips and Clancy, 1971b) have considered two components of social desirability: "need for social

<sup>1</sup> However, we have attempted to validate a desirability index elsewhere (Phillips and Clancy, 1971b). We obtained from respondents their assessments as to the desirability of (using the same 9-point scale as in the study under discussion): being the kind of persons who try new products, read all the latest books, know something about the latest television programs, and have seen all the newest movies. We also asked questions regarding their utilization of several new products, books, television programs, and movies—all of which were actually *nonexistent*. As expected, those people who perceived the traits as most desirable scored highest in "overclaiming" (i.e., dissimulation), therefore suggesting that measures of trait desirability may be of use in isolating potential bias.

approval" (Crowne and Marlowe, 1960, 1964), which is considered to be a characteristic of the individual, and "trait desirability," which refers to people's assessments of the desirability of traits or symptoms in an inventory. It is our position, however, that "trait desirability" assessments are for the most part culturally determined and, therefore, in a sense "social" (see Clancy, 1971, for a discussion of this point).

Importantly, our current work (Clancy, 1971; Phillips and Clancy, 1971b) in which we examine the joint effects of *both* social approval and trait desirability represents a departure from that of other students of "social desirability" who tend to examine one or the other. Edwards' (1957) "social desirability" score, as an example, is an attempt to measure a personality characteristic, whereas his earlier (1953) work is more like Dohrenwend's (1966) paper and our own in so far as it deals with the desirability of traits.

(4) Our "interpretation of the yeasaying-naysaying response set data is clouded by the questionable response set index." We disagree with Harvey's bold assertion that our "naysaying index is . . . simply another index of mental health." The fact is that several different indicators of yeasaying-naysaying have been related to scores on the Langner inventory (Clancy, 1971; Dohrenwend, 1966; Phillips and Clancy, 1970). True, one might argue that *all* indicators of this response bias are related to mental health scores and that both represent an underlying dimension of mental illness. But this seems a little far-fetched. While scores on the Langner inventory are inversely related to socioeconomic status, neither Phillips and Clancy (1970) nor Clancy (1971) found any evidence of a relationship between different indicators of acquiescence and socioeconomic status.

Unfortunately, the space allocated to us for our response to Harvey's comments does not allow us room to attend to many of his other remarks. Nor is there space here to mention some of our own doubts regarding our previous discussion of systematic bias (p. 510), where we failed to consider some difficult issues surrounding questions about the validity of measures as contrasted with the validity of relationships. However, we have attempted to attend to this in a subsequent paper (Phillips and Clancy, 1971b). Finally, we wish to note that Harvey's comments have helped to highlight the disagreement between us as to whether it is at all possible to locate the biases in survey research. Whereas one of us (Clancy) is relatively optimistic on this score, the other

(Phillips) questions the possibility of achieving such research purity. But we are in agreement as to the truth of Hauser's (1969:127) assertion that "It is at least a moot question as to whether, up to this point in the history of the use of survey results, more misinformation than information has been gathered on many subjects." Thus, we welcome Harvey's concern with problems of validity and bias in survey studies, and the opportunity to respond to him.

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#### COMMENT ON "THE CAUSES OF RACIAL DISTURBANCES: A COMPARISON OF ALTERNATIVE EXPLANATIONS"

Spilerman emphasizes the independent causal effects of black population size and percent of city population black in the occurrence of disorders. He observes that, in addition to the greater frequency of potential "precipitating incidents" and the greater sense of security that large numbers bring, a large black population is causative of disorders because all blacks are equally frustrated with white society. Consequently, the more blacks there are, the more likely is disorder (this idea is a modification of Tomlinson's (1968) evaluation of the mass media activation of black grievances).

But Spilerman does not have empirical justification for rejecting the "expectational factors" (both absolutely and relatively better life conditions for blacks in disorder-prone cities) because their relationship to disorders disappears when size of black population is controlled. First of all, support for the relevance of the expectational factors exists in the zero-order correlation (Black populations in disorder-prone cities tend to have higher incomes, occupational status, and educational level than the black populations of other cities) and so they cannot simply be ruled out as causal factor in specific situations. Secondly, the effect of black population size might be indirect in that motivation for disorders may actually be due to rising expectations caused by superior life conditions for blacks, caused in turn over time by their greater power in the community because of their large numbers. If such is the case, the amount of variation remaining in the expectational factors after black population size is controlled might be too small to have any differential impact in cities with the same size black population. Thirdly, Spilerman does not differentiate type of disorder for us, and so we

cannot be sure which of the hypothesized independent variables are related to collective disorders and which are associated with more organized forms of disorder such as planned ambushes of police.

Spilerman's rejection of the importance of social disorganization factors does not seem warranted either, since his measures of social disorganization are relatively indirect and since Warren (1969) found patterns of social organization to play an important role in differentiating riot, counter-riot, and noninvolved neighborhoods in the 1967 Detroit Riot. Another analysis of variables differentiating rioters from nonrioters *within* the black population of Detroit tends to cast doubt on Spilerman's conclusions. Geschwender and Singer (1970) found that riot participants were inferior to nonparticipants with regard to occupation, income, time employed, and education and, thus, represented the more deprived elements of the black population. On the basis of this study, it would seem that even black population size and expectational factors taken together could not satisfactorily explain the occurrence of disorders.

And finally, despite a strong zero-order correlation between population per councilman and disorders, Spilerman rejects the possibility of causal significance of unresponsive political structures for riots because the relationship does not seem to exist when black population size is controlled. Again, this conclusion is questionable since it rules out the specific situation and since we do not know what level of variation remains in the hypothesized independent variable after controlling for black population size.

Spilerman's study suffers in that the author has made conclusions which are far too general and absolute to be justified by his methods. Part of the problem, I believe, stems from the way in which correlational data were used to arrive at a causative explanation for disorders which resulted in a narrow single factor explanation for a social phenomenon which can only be fully explained through a combination of demographic, social psychological, and social structural variables. Spilerman has probably identified the most important factor in the development of disorders, but an adequate explanation must focus on a constellation of factors and yield a model for the occurrence of disorders including all the relevant aspects of the social matrix of which riots are a product.

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## REPLY TO DEFONZO

First, some factual matters need to be cleared up. In his initial sentence DeFronzo contends that I emphasize the independent causal effects of black population size and *percent black in the city*. This is incorrect. The burden of my argument is entirely on the black population size variable. In fact, I specifically *deemphasize* the role of percent non-white in the discussion (Spilerman, 1970:644).

DeFronzo (1971:515) also states, "Spilerman does not differentiate type of disorder." However, I indicate rather precisely, and at some length, that the analysis was restricted to instances of Negro aggression which were "spontaneous" in origin (pp. 630-631, also footnote 10). Thus, DeFronzo's claim that "we cannot be sure which of the hypothesized independent variables are related to collective disorders and which are associated with the more organized forms of disorder such as planned ambushes of police" is incorrect. The reader is clearly informed that the latter kind of incident has been excluded from the study in order to reduce heterogeneity in the type of disturbance.

The more substantial points in DeFronzo's comment concern my justification for rejecting the expectational and social disorganization explanations. His argument regarding the expectational variables rests upon two points. DeFronzo believes that the statistically significant zero-order correlations between these variables and the measure of disorder-proneness provides evidence for an expectational thesis; also that multicollinearity between Negro population size and the indicators in the expectational clusters may substantially reduce the amount of variation remaining in these factors once black population size is controlled.

With respect to the first point, the existence of a significant zero-order correlation simply is insufficient to establish a causal relationship. The most accessible discussion of this matter

is in Blalock (1960:337-339). It should also be noted that the absence of a zero-order correlation is insufficient to exclude the possibility of direct causality (Li, 1955:162). One constructs a theory by *assuming* a particular ordering of variables, and then estimates the parameters to see if they are consistent with the model. The major assumption which I made was that, owing to the type of racial disturbance which we experienced in the 1960's, the more Negroes present in a city the greater its expected number of disorders. Conceptually, this is a trivial point and does not prove the irrelevancy of other community characteristics to disorder-proneness, but it does indicate that this factor must be controlled before the contribution from the community structure variables can be assessed.

This matter can also be stated in a more technical way. The correlation results which I present in Table 5 derive, of course, from a regression model. In order to obtain unbiased estimates of the b-coefficients for variables in the expectational clusters *it is required that Negro population size be included in the regression equations*. DeFronzo does not dispute the causal importance of Negro population size (see his concluding sentence), nor that this variable is correlated with the expectational indicators—indeed, this is his point of concern. Yet, this situation is precisely where deletion of Negro population size would result in a specification error. As Blalock (1964:48) writes, "we must assume . . . that any variable that is a major cause of the dependent variable Y and at the same time is correlated with any of the X<sub>i</sub> has been explicitly brought into the causal model" (see Draper and Smith, 1966:31-84 for a more technical discussion of this matter). Once Negro population size is entered, almost all of the b-coefficients for other community characteristics are statistically not different from zero. Indeed, these coefficients have the same significance pattern as reported for the partial correlations in Column (2) of Table 5.

With respect to the second point. If Negro population size were highly correlated with the other explanatory variables, little of their respective contributions to the explained variance could be separated by statistical methods. In this situation I still would be willing to argue, on conceptual grounds, that the variation in the dependent variable which can be explained by either should be assigned to Negro population size (see pp. 643-645 for my argument). Nevertheless, the statistical situation is far from hopeless. The zero-order correlations between Negro population size and the indicators of absolute and relative deprivation (which were



used to assess the merits of an expectational thesis) are less than .25 for eight of the nine variables in these clusters. The exception is percent Negro ( $r=.71$ ); however, although this variable was included with the other deprivation indicators, it does not relate to an expectational argument (see footnote 32 for clarification). Consequently, it is not the case that multicollinearity was high so that by controlling on Negro population size one has reduced the variation in the expectational indicators by a substantial extent.

The above discussion also constitutes my reply to DeFronzo's comment regarding the population-per-councilman variable. Turning to his comments on the social disorganization thesis, I would suggest that he is guilty of committing an ecological fallacy. My analysis uses city as the unit of observation; the problem I address is to explain why racial disturbances were more frequent events in some communities than in others. The fact that Gerschwender and Singer (1970) report that riot participants in Detroit "were inferior to non-participants with regard to occupation, income, time employed" has no necessary bearing on why a riot should have occurred in Detroit rather than elsewhere. In passing, I would point out that other analyses of riot participants have produced contrary findings. Thus, the Kerner Commission (National Advisory Commission, 1968:75) reported that their survey shows no correlation between riot participation and income level or whether one is unemployed, and a *positive* correlation between riot participation and educational attainment.

Twice in his comment DeFronzo contends that "this conclusion is questionable since it rules out the specific situation." Frankly, I do not fully comprehend the meaning of this rule. If he is arguing that there are idiosyncratic causes of a disturbance, then we are in agreement. But it is precisely these particularistic factors which I wish to deemphasize. My concern is with systematic causes, whether certain structural arrangements in a community characteristically make for disorder-proneness. After all, what can one learn from particularistic findings?

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#### COMMENT ON "THE ASSIGNMENT OF NUMBERS TO RANK ORDER CATEGORIES"

Labovitz (1970) has argued for ascribing the metric (interval-ratio) property to ordinal data. I am not convinced of the asserted near-general validity of his ascriptive procedure, nor do I find his caveats to have much operational value. The evidence he presents takes the form of Pearson coefficients between the NORC occupational prestige ranks of 36 occupations, an assigned linear rank order variable with values 1 to 36 except for ties, and a set of 18 "monotonic random generated scoring systems." Details of the method for generating these last systems (metric scales) are not given. The potential range of each metric scale was 1 to 10,000; the NORC ranks ranged from 7 to 97. Support for Labovitz's conclusions is that, of the 190 possible coefficients, all are above 0.90, and 157 are above 0.96. I contend that the only coefficients that directly bear on the question of ordinal to metric validity are those between each of the 18 metric scales and, in turn, the two ordinal scales. These coefficients are given in Table 3 of the paper at the intersections of rows (1) and (2) with columns (3) to (20).

Now these coefficients can be viewed as two sets of data resulting from sampling the constrained random process which is Labovitz's methodology. These data and any statistic obtained from them are at the meta-analytic level\*; i.e., we are testing a methodology of rank conversions and not the relationship of sociological or phenomenological variables. An appropriate meta-statistic is again Pearson's  $r$  measuring the degree of association between the

\*It is important to distinguish between the stratified analysis used herein and "multiple-level analysis involving data at several levels of aggregation" (Coleman, 1969). The distinction is qualitative. The latter is correctly placed in the methods of sociology. Here we are concerned with the verity of methodological propositions in the Gödelian context discussed by Ferdinand (1969).

two sets of derived Pearson correlation coefficients corresponding to the two ordinal scales. The value of the meta-statistic is less than 0.05. A scatter diagram further shows that the relationship is weak for simple curved regression lines. Since the ordinal property—i.e., scale values are ranks—of the two scales implies that association must be strong for valid metric conversions, we must also conclude that the paper's methodology does not produce the asserted findings.

Another type of argument may be made. Suppose each ordinal scale is taken to be a metric image of the other. Then by Labovitz's criterion—high (0.98) correlation—the ordinal scales are better than most of the elaborately calculated "scoring systems."

What produced the high original correlations? Simply that the monotonicity constraint, the large range differences (more than two orders of magnitude), and the large number of occupational items in the original data set all combine to force a free random number generation process to be a quasilinear transformation process. An illustration is in order. Consider just three ranks (1, 2, 3) and allow just two-digit random monotonic selection of an image scale in the range 1 to 10. This was done. The candidate metric scale is 7.4, 9.7, 9.9. Pearson's  $r$  is 0.90. What Labovitz demonstrated in a particular case is the power of monotonicity in correlation measurement. The significance is numerical, not methodological.

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#### COMMENT ON THE PEARSON R IN RANDOM NUMBER AND PRECISE FUNCTIONAL SCALE TRANSFORMATIONS

In "The Assignment of Numbers to Rank

Order Categories" (*ASR*, June 1970:515-524), Labovitz has shown that high values of the Pearson  $r$  are obtained when an interval scale is monotonically matched against random numbers between 1 and 10,000. To assume that high correlations obtained in this manner justify the claim that ordinal scales can be treated as if they conform to interval scales can be grossly misleading. The Pearson  $r$  is a statistic which can be quasi-invariant to incorrect nonlinear functional transformations and to the  $N$  in such transformations. Labovitz's generation of random numbers between 1 and 10,000 which are monotonically ranked leads to curves where the average departure from some crude linear approximation is small. Under these conditions the insensitivity of the Pearson  $r$  to gross functional errors is not obvious. The Pearson  $r$  also yields high values when *precise* but *incorrect* nonlinear functional scale transformations such as  $Y=X^2$ ,  $Y=X^3$ ,  $Y=X^4$  are made. Since the Pearson  $r$  is the correlation statistic most frequently used in sociology, we have taken some pains to illustrate *quantitatively* the insensitivity of this statistic both to precise nonlinear scale transformations and to the number of items in such scale transformations. Values of  $r$  and  $dr/dN$  were calculated as a function of  $N$  for all integers from 1 to 100 for a number of nonlinear functions ( $Y=X^2$ ,  $Y=X^3$ ,  $Y=X^4$ ,  $Y=e^x$ ). In these examples the interval between successive items is constant for one scale ( $\Delta X=1$ ) and progressively increases in all of the matched scales. In all cases  $X$ , and therefore  $N$ , varies from 1 to 100. The range of  $Y$  varies in the four examples from  $\Delta Y=10,000$  to  $\Delta Y=10^{10}$ . Sample results are shown in Table 1.

Even in the case of  $Y=X^4$ , where the interval scale from 1 to 100 is matched against a monotonic scale from 1 to 100,000,000, the correlation is high for all values of  $N$  from 2 to 100, in spite of the fact that successive intervals in the  $Y$  scale increase systematically from  $\Delta Y=15$  to  $\Delta Y \sim 4,000,000$ . Only in the case of a linear scale matched against an exponential scale does  $r$  vary markedly with  $N$ . However, in this case the range of  $Y$  becomes ludicrously large before  $r$  becomes small.

It is of some interest that the rate of change of the Pearson  $r$  with  $N$  is a very rapidly decreasing function which is quite similar for all three power functions once  $N$  exceeds the value of 5. (The major differences in the rates of change of  $r$  with  $N$  between the three power functions occur for very low values of  $N$ .) Typical common values for  $Y=X^2$ ,  $Y=X^3$ , and  $Y=X^4$  are

TABLE 1. Variation of the Pearson  $r$  with  $N$  for Successive Integers from 1 to 100 For  $Y=X^2$ ,  $\bar{Y}=X^2$ ,  $Y=X^4$ , and  $Y=e^X$ .

$N(\text{or } X)$	$Y(=X^2)$	$r(x)(x^2)$	$Y(=X^2)$	$r(x)(x^2)$	$Y(=X^4)$	$r(x)(x^4)$	$Y(=e^X)$	$r(x)(e^X)$
2	4	1.00000	8	1.00000	16	1.00000	7.389	1.00000
5	25	.98110	125	.94312	625	.90341	148.4	.88628
10	100	.97456	1,000	.92839	10,000	.88168	22,030	.71687
15	225	.97241	3,375	.92408	50,620	.87573	$3.27 \times 10^3$	.60992
20	400	.97135	8,000	.92205	160,000	.87302	$4.85 \times 10^3$	.53870
25	625	.97072	15,625	.92088	390,600	.87148	$7.20 \times 10^{30}$	.48742
30	900	.97030	27,000	.92011	810,000	.87049	$1.07 \times 10^{38}$	.44834
35	1,225	.97000	42,875	.91957	1,501,000	.86981	$1.59 \times 10^{45}$	.39193
45	2,025	.96961	91,125	.91887	4,101,000	.86891	$3.49 \times 10^{19}$	.37067
50	2,500	.96947	125,000	.91862	6,250,000	.86861	$5.19 \times 10^{21}$	.35252
55	3,025	.96936	166,375	.91843	9,151,000	.86836	$7.70 \times 10^{23}$	.33679
60	3,600	.96926	216,000	.91825	12,960,000	.86816	$1.14 \times 10^{26}$	.32299
65	4,225	.96918	274,625	.91812	17,850,000	.86799	$1.70 \times 10^{28}$	.31076
70	4,900	.96912	343,000	.91801	24,010,000	.86784	$2.5 \times 10^{30}$	.29982
75	5,625	.96906	421,875	.91791	31,640,000	.86772	$3.7 \times 10^{32}$	.28996
80	6,400	.96901	512,000	.91782	40,960,000	.86761	$5.5 \times 10^{34}$	.28101
85	7,225	.96896	614,125	.91774	52,200,000	.86751	$8.2 \times 10^{36}$	.27284
90	8,100	.96892	729,000	.91767	65,610,000	.86742	$1.2 \times 10^{39}$	.26534
95	9,025	.96889	857,375	.91761	81,450,000	.86735	$1.8 \times 10^{41}$	.25843
100	10,000	.96885	1,000,000	.91755	100,000,000	.86728	$2.7 \times 10^{43}$	.25203

$N$	$(-dr/dN)$
5	$10^{-3}$
15	$10^{-3}$
35	$10^{-4}$
100	$10^{-5}$

From these values one can estimate that an  $N$  of about 10,000 would still yield a correlation coefficient of about 0.9 for  $Y=X^2$ . (Since the Pearson  $r$  is invariant with respect to interchange of  $Y$  and  $X$ , all the results given above also hold for  $Y=X^2$ ,  $Y=X^4$ ,  $Y=X^2$  and  $Y=\ln X$  respectively.)

Although the correlations calculated in Table 1 are for successive integers from 1 to 100, random choices of numbers yield essentially the same values of  $r$  vs  $N$ . In Table 2 the column  $X$  lists 20 random numbers from 1 to 100 that are matched to their squares. The Pearson  $r$  for this example is 0.969, a value which differs by only about 0.2% from the value of  $r$  for the integers from 1 to 20.

Since the Pearson  $r$  gives high correlations between linear scales and incorrect nonlinear functions and since the value of  $r$  is insensitive

Table 2. Correlation of  $X$  versus  $X^2$  for 20 Numbers from 1 to 100 ( $r=0.969$ ).

$Y=(X^2)$	$X$	$Y=(X^2)$	$X$
9	3	2809	53
49	7	3481	59
144	12	3844	62
256	16	4356	66
529	23	5625	75
784	28	6241	79
1089	33	7056	84
1444	38	7225	85
1681	41	8464	92
2209	47	9604	98

to  $N$  over large ranges, Labovitz's use of this statistic is a poor justification for the general claim that ordinal scales can be treated as if they conform to interval scales.

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#### A NOTE ON TREATING ORDINAL DATA AS INTERVAL DATA

Since social scientists are often forced to treat ordinal scales as interval scales, the problem of estimating the error involved in this procedure is relevant to much of the applied research in sociology today. Labovitz (*ASR*, June, 1970:515-524) demonstrated, by use of a Monte Carlo simulation, that "reasonable" changes in scale do not affect the sample correlation greatly. However, as pointed out by Mayer (*ASR*, October, 1970:916-917) it is not clear whether his results can be interpreted as sufficient evidence for treating ordinal data as interval data. The purpose of this note is to show that in fact the effects of treating ordinal data as interval can be disastrous, even in terms of the correlation coefficient.

Suppose a sociologist wants to study the relationship between two variables  $Y_1$  and  $Y_2$ , and that he is willing to assume that  $Y_1$  is an interval level variable which has a normal distribution with mean 0 and variance  $a^2$ . In addition suppose  $Y_2$  is an ordinal variable but that the sociologist is willing to assume that some monotone function of  $Y_2$  has a normal distribution (i.e.,  $Y_2$  is a monotone distortion of a normal variable). Suppose that  $\log Y_2$

is the true (unmeasured) interval level, normal random variable with mean 0 and variance  $a^2$ . Thus  $Y_1$  and  $\log Y_2$  are bivariate normal random variables with some unknown correlation  $p$ .

The sociologist wants to proceed by correlating  $Y_1$  and  $Y_2$ , since he is unaware that  $\log Y_2$  is an interval variable. Note that this problem is most reasonable, since examples can be given in which it is not clear whether a variable  $Y$  or its logarithm has the relevant metric (e.g., in dealing with Gross National Products or Likert type attitude scales). The purpose of this note is to show that by correlating  $Y_1$  and  $Y_2$ , the sociologist may seriously underestimate the relationship between the two variables. Note that we are dealing with population correlations and no sample error has been introduced.

In order to compute the correlation between  $Y_1$  and  $Y_2$ , we use the following transformations:

$$\begin{aligned} Z_1 &= Y_1 - p \log Y_2 & Y_1 &= Z_1 + p Z_2 \\ Z_2 &= \log Y_2 & Y_2 &= e^{Z_2} \end{aligned}$$

It can be shown that  $Z_1$  and  $Z_2$  are independent, normal random variables with means 0 and variances  $a^2(1-p^2)$  and  $a^2$  respectively. In order to compute  $p(Y_1, Y_2)$ , the correlation between  $Y_1$  and  $Y_2$ , we note

$$\begin{aligned} p(Y_1, Y_2) &= \frac{E(Y_1 Y_2) - E(Y_1)E(Y_2)}{\sigma(Y_1)\sigma(Y_2)} \\ &= \frac{E[(Z_1 + pZ_2)e^{Z_2}] - E(Z_1 + pZ_2)E(e^{Z_2})}{\sigma(Z_1 + pZ_2)\sigma(e^{Z_2})}, \end{aligned} \quad (1)$$

where  $\sigma(\cdot)$  denotes the standard deviation of the variable.

Since  $Z_1$  and  $Z_2$  both have mean 0,  $E(Z_1 + pZ_2) = 0$  and (1) reduces to

$$\frac{E(Z_1 e^{Z_2}) + pE(Z_2 e^{Z_2})}{\sigma(Z_1 + pZ_2)\sigma(e^{Z_2})}. \quad (2)$$

Since  $Z_1$  and  $Z_2$  are independent,  $E(Z_1 e^{Z_2}) = E(Z_1)E(e^{Z_2}) = 0$  and (2) becomes

$$\frac{pE(Z_2 e^{Z_2})}{\sigma(Z_1 + pZ_2)\sigma(e^{Z_2})}. \quad (3)$$

It may be seen that  $\sigma(Z_1 + pZ_2) = [a^2(1-p^2) + p^2 a^2]^{1/2} = a$ ,

and thus the problem is to compute  $E(Z_2 e^{Z_2})$  and  $\sigma(e^{Z_2})$ . From moment generating function theory it is known that  $E(e^{tZ_2}) = e^{1/2 t^2 a^2}$  (5) and by simple differentiating this gives

$$E(Z_2 e^{Z_2}) = a^2 e^{1/2 a^2}.$$

Since  $\sigma^2(e^{Z_2}) = E(e^{2Z_2}) - E(e^{Z_2})^2$  we obtain

from (5) that

$$\sigma(e^{Z_2}) = (e^{2a^2} - e^{a^2})^{1/2}. \quad (7)$$

Using (4), (6) and (7),

$$p(Y_1, Y_2) = \frac{ape^{1/2 a^2}}{(e^{2a^2} - e^{a^2})^{1/2}} = ap(e^{a^2} - 1)^{-1/2} \quad (8)$$

Thus the correlation between  $Y_1$  and the ordinal variable  $Y_2$  is given in (8). The amount of distortion is a function of the variance  $a^2$  and is tabled in Table 1.

Table 1. Distortion in Correlation,  $p(Y_1, Y_2)$ , As Function of  $a^2$ .

p	$a^2$				
	1	2	5	10	20
.10	.08	.06	.02	.00	.00
.20	.15	.11	.04	.00	.00
.40	.31	.22	.07	.01	.00
.60	.46	.34	.11	.01	.00
.80	.61	.45	.14	.02	.00
.90	.69	.50	.36	.02	.00
1.00	.76	.56	.18	.02	.00

Observe that if  $p=1$  and  $a^2=2$ , then the variable  $Y_1$  explains 100% of the variance in the interval level variable ( $\log Y_2$ ) associated with  $Y_2$ , but that by using  $Y_2$  instead of  $\log Y_2$ , it explains only 31% of the variance in  $Y_1$ . The reported correlation would be .56 while the true correlation is 1.00.

Although it might be argued that the exponential transformation is an extreme transformation, if considered as a member of the class of all monotone transformations, the exponential transformation has many convenient properties (continuous, differentiable, etc.). If the sociologist ignores the fact that his data are ordinal, he may be using a much more severe monotone transformation of an interval scale.

We suggest that the class of rank order statistics (e.g., rank order correlations) be used on ordinal data (rank order correlations would be undisturbed by the exponential distortion used above), and that social statisticians concentrate more effort on providing rank order statistics which will enable the sociologist to perform all types of analyses on ordinal data without making any interval assumptions.

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### IN DEFENSE OF ASSIGNING NUMBERS TO RANKS

Because of recent criticism of my article (Labovitz, 1970), further defense is necessary for assigning numbers to ranks. Basically, my defense revolves around the notions that assigning numbers is an aid to data analysis (by permitting the use of sensitive, versatile, and interpretable statistics), and the procedure is not risky if care is taken to avoid extreme exponential distributions. One of the critiques (Mayer, 1970) is partially supportive of my thesis, while the other three (Vargo, 1971; Schweitzer, 1971; and Mayer, 1971) are in opposition.

Mayer's major points in his first (1970) critique are (1) the treatment of ordinal data as interval is not proven for all statistics, and (2) besides the nature of the scale (ordinal or interval), most inference statistics require a specific distribution. As stated in my article, I too would like further evidence, although the use of correlation coefficients and standardized beta weights were supported by the test results. In a previous paper (Labovitz, 1967), support was given for use of the point-biserial correlation and Student's *t*. A careful reading of Mayer's first commentary suggests that he is skeptical (apparently) only on assigning numbers to ranks for inference statistics. Two strategies for handling inference statistics are (1) the use of robust statistics, so that violating certain assumptions has a small effect on interpretation, and (2) assigning numbers to meet the distribution assumptions.

Mayer's second critique focuses on an "extreme" case, where it may not be advisable to treat ordinal data as interval. By essentially dichotomizing a scale, he finds low correlations. The results of this type of extreme case using an exponential transformation was pointed out in my article. Mayer could have reduced the "error" by applying the equal interval scoring system; admittedly, however, a dichotomous scale will yield the largest error rate, because the amount of error is inversely related to the number of ranks. If a dichotomy is expected, it is advisable to be quite cautious in treating the data as interval; and the interval transformation should not be performed without a sound rationale about the nature of the dichotomous values.

Schweitzer also demonstrates some error in using an exponential example. Again essential dichotomization results, which reduces variance and attenuates *r*. Schweitzer also argues that the Pearsonian *r* is inappropriate to test the claim that ordinal variables can be treated as

interval, because it is insensitive both to non-linear scale transformations and to the number of ranks of a variable. It is precisely these characteristics, however, that warrants its use for the problem, and permits accurate interpretations of *r* for transformed scales. Rather than blaming *r* for being insensitive, it should be praised for being "robust." Robust statistics like analysis of variance or Student's *t* have been proven to be virtually "insensitive" to the violation of certain assumptions.

Vargo's critique of my article is more extensive than Mayer's and Schweitzer's, and is characterized by some serious errors and misinterpretations. He is not convinced of my "asserted near-general validity" of assigning numbers to ordinal scales and treating them as if they are interval. He fails to note that in the article I recommend tests on other statistics, and that I do caution the reader about overgeneralizing the conclusions from my results. His three basic arguments are (1) failure to stay at the meta-analytic level, (2) the supposed metric equivalence of ordinal scales, and (3) demonstrating numerical, rather than methodological significance.

Before discussing these three arguments, it is necessary to challenge a statement in Vargo's first paragraph, where he implies that there are two ordinal scales—scoring systems (1) and (2). He maintains that only the correlations between these two systems and the 18 metric scales are relevant to the problem of converting ordinal scales to interval scales. Actually, I use only one ordinal ranking scale, which is labeled scoring system (2) in Table 3 (the NORC scale). The other scoring system he labels "ordinal" is actually an assigned intervally based linear system (system 1). These misperceptions form part of two of Vargo's criticisms of the thesis.

Vargo's emphasis on considering the correlations between the "two" ordinal systems and the 18 metric systems as the only relevant correlations misses one of the major points in the article. Assuming that we do not know the "true" scoring system, only very small errors accrue when assigning almost any scoring system that is consistent with the monotonic order. Consequently, even if we assign the "wrong" scoring system we can still treat the variable as if it is interval, and be confident that the resulting errors are small.

Vargo's first major criticism is that my analysis is at the meta-analytic level, which, in this instance, pertains to the testing of "a methodology of rank conversions and not the relationship of sociological or phenomenologi-

cal variables." Contrary to his assertion, both "levels" were analyzed in the paper, first, by considering the intercorrelations among the scoring systems (apparently the meta-analytic level) and second, by applying the results to the substantive problem of predicting suicide partially in terms of interval values of occupational prestige. With regard to the meta-analytic level, Vargo maintains that I should have computed an overall correlation based on the correlations of the "two" ordinal scales and the 18 randomly generated scoring systems. The use of this "meta-statistic correlation" indicates a serious misunderstanding of the problem, which was to demonstrate that an assigned scoring system, consistent with the monotonic order, results in small error when used in substantive problems. That is, the value of the correlation coefficient is close to the "true" value and, therefore, can be interpreted accurately.

The problem was not whether the linear scoring system and the NORC scoring system correlates exactly with the 18 randomly generated scoring systems. The important point is that they all correlate quite highly with the randomly generated scoring systems *and* with each other. The extremely high and nearly identical intercorrelations almost necessarily results in a very low overall correlation at the meta-analytic level (less than .05 according to Vargo). It is *not* important to our problem to see if system (2) correlates .96 with system (9), while system (3) correlates .98 or .94. The minute "error" strongly suggests that we can interchangeably use system (2), system (3), and system (9) in our analysis.

Vargo's second argument on the supposed metric image of the two ordinal scales is invalid, because there is only one such scale, as stated previously. First, the very idea of assuming that the "two" ordinal scales may be metric images of one another misses the major points that we simply do not know the correct measurement. Second, he asserts under the metric image thesis that the ordinal scales are better than most of the elaborately calculated scoring systems. Finally, his contention that the .98 correlation between the "two" ordinal scales is higher than the correlation for most of the other scoring systems is simply not true. Actually 77 of the correlations are higher than .98; 57 are slightly lower; and 53 are equal to this value.

Vargo's final criticism focuses on three factors that presumably produced the high correlations in Table 3: (1) the constraint of mono-

tonicity, (2) the large range differences in scoring systems, and (3) the large number of occupational items.

The constraint of monotonicity, the first factor, happens to be one of the major results of the tests. Because monotonicity is so important, it is legitimate to assign numbers to ordinal rankings. I partially agree with the second factor on large range differences. High intercorrelations, however, also are obtained with small range differences, which is illustrated by comparing the linearly assigned scoring system (1) and the NORC scoring system (2). The range differences here are not high, but the correlation is .98. Finally, I agree with his third factor (as stated in my article), that is, the larger the number of ranks the higher the intercorrelations. Correlations of very large magnitudes, however, are obtained with substantially fewer categories. In a previous test (Labovitz, 1967), only four categories were used, and very high correlations were obtained.

In conclusion, the advantages of using interval transformations far outweigh the slight chance of falling upon an extreme exponential ( $e^x$ ), an highly unlikely example. Although a virtual dichotomy and a linear variable will not be closely related, we are generally beyond such minimal distinctions. If a procedure is helpful nearly all the time, it should not be dropped because an unlikely example is presented.

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## REVIEW ESSAYS

### **POLITICS AND SOCIAL STRUCTURE, by Talcott Parsons. New York: The Free Press, 1969. 557 pp. \$13.50.**

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It's amazing how far a good analogy can carry us. Perhaps we forget this because we are so wary of being misled by bad analogies that the method of argument has become discredited. Moreover, what we are dealt are frequently not analogies at all but metaphors—provocative images that serve to highlight some particular feature to which we wish to call attention. Taking an analogy seriously is a different matter. If we are indeed correct in identifying an isomorphism between one system and another, then we should be able to use our greater understanding of one to spot analogous parts and processes in the other.

It is no criticism, then, to say that Parsons' contribution to political sociology rests largely on the extended use of a very rich analogy between the economy and the polity. Parsons' use of this analogy should not be confused with its more metaphoric use in, for example, exchange theory. The latter merely recasts certain social relationships in a language that highlights an instrumental element similar to relationships in the marketplace. It then enriches the metaphor not with concepts drawn from economic theory, but with such social-psychological paraphernalia as norms. Distributive justice, for example, has no counter-part in economics unless one pokes around in the ashes of the long abandoned concept of "just price."

An example from Parsons' use of the economic analogy should illustrate the difference. First of all, his general paradigm tells him that if he can abstract a subsystem—the economy—that is focused on the functional problem of adaptation, then there ought to be an analogous system for each of the other three problems. The polity is his answer for the subsystem that mobilizes resources to attain collective goals.

Now it so happens that he has already spent a good deal of energy analysing the economic system (see especially *Economy and Society*, with Neil J. Smelser) and, for that matter, economists have done a bit of analysing themselves. It follows that if the analogy is a good

one, it should be possible to draw on well-developed bodies of theory in economics to understand the operation of the polity. Take monetary theory, for example. The introduction of a generalized, circulating medium of exchange is one element that makes a modern economic system radically different from a simple barter system. Furthermore, modern money is not a precious metal with value of its own—it has value only in exchange and not in use. It has a special and complicated relationship to precious metals, but it is not the same as this commodity base; it is a symbolic medium.

Parsons takes this analogy and applies it to the polity. If there is a generalized, symbolic, circulating medium in the economy, then shouldn't there be one in the polity as well? What one looks for is a generalized capacity for mobilizing resources in the pursuit of collective goals. This is certainly one traditional element in the manifold writing on political power, so it is not surprising that Parsons appropriates the word "power" to describe this analogue of money in the polity.

In an unusual display of contentiousness, Parsons defends his definition against the charge that it is merely a matter of personal taste. It is not arbitrary, he argues, because it derives from a coherent and continuing analysis of the social system as a whole. But then neither are the alternative uses of the term any more arbitrary; they are chosen for their usefulness in dealing with given phenomena. What is at stake here is not a contention about labeling but an issue about which phenomena need some accounting. Clearly, there are important aspects of the power theme that Parsons does not attempt to deal with; he is free to appropriate the term for the particular aspect that concerns him only if he is sensitive to the fact that other important "power" phenomena are ignored in the process.

Be that as it may, his analogy enables him to handle certain issues very well and produces a series of striking insights. One of these has to do with the relationship between power, as he defines it, and force. Force plays a role vis-à-vis power similar to the role of silver or gold vis-à-vis intrinsically "valueless" paper

money. As long as there is confidence in what the money symbolizes, there is no need to back it up with anything of real value in use. But the confidence rests, in part, on the belief that it could be backed up if necessary. Similarly, power is not force, but rests on the confidence that obligations could be made binding, *if necessary*, by using force. This clearly implies that a political system which must resort to force is not demonstrating its power but its lack of it.

Parsons also makes extremely effective use of the analogy between credit and its role in the expansion of money in the economic system, and similar processes in the polity. It enables him to argue that generalized political support functions in an identical manner in allowing the expansion of power available to a system. Thus, a polity enjoying the widespread confidence of its citizens can undertake commitments well beyond its capacity to enforce them, thereby increasing its effectiveness by transcending the limits of its power base. Of course, it needs to have a healthy reserve behind its commitments if that confidence is to remain realistic.

An unfortunate omission from this collection of Parsons' essays on political sociology is his "Some Reflections on the Place of Force in Social Process." The omission is mitigated by the fact that this article appears in both a previous collection of his own essays, *Sociological Theory and Modern Society* and in the volume edited by Harry Eckstein, *Internal War*. It is omitted, apparently, because it represents more of an application of the basic argument than a development and elaboration of it. However, it is as good an illustration as anything that Parsons has written of both the value and limitations of his contribution to

political sociology. The relationship of power and force is shown with subtlety and brilliance, as is the social-psychological foundation of his political sociology, grounded as it is in generalized attitudes and expectations about the political system and its managers.

In the end, however, Parsons' analogy is curiously one-way. The analysis of the economic system is used to yield insights about the polity, but there is nothing of the reverse. Political scientists and sociologists have often tried to deal with how subgroups in a society attempt to mobilize resources and direct them at changing some aspect of the social organization. A particularly interesting problem is that of turning an incoherent aggregate of dissatisfactions into a coherent political force—what one might call the creation of power by challenging groups. Surely this is a problem worthy of the attention of a political sociologist and, I would argue, one of comparable importance to those at which Parsons directs his analysis. If one really takes the analogy between the polity and the economy seriously, why not look systematically for the counterpart of social movements and collective behavior in the economy?

Like any paradigm, Parsons' has its limits. It produces a rather sterile politics—at times, quite apolitical. In his sometimes brilliant essay, "Full Citizenship for the Negro American?" (reprinted here from *Daedalus*), one is only obliquely aware that civil rights and black power movements have been sweeping the nation in the last two decades. They appear, in his vision, more or less as epiphenomena. But perhaps this is ungracious. His political sociology is incomplete, but there can be little doubt that he has enlarged our vision of political phenomena.

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**SOCIOLOGICAL WORK: METHOD AND SUBSTANCE**, by Howard S. Becker. Chicago, Ill.: Aldine Publishing Co., 1970. 358 pp. \$11.75.

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Fame can, and usually does, have strange effects on sociologists: it leads either to a relaxation of personal and professional drives, in which case the sociological enterprise is done with noticeable sophistication; or it can heighten the thirst for accolades and acknowledgments to such an impassioned degree that

the tasks and even the tools of sociology become a burden to further effort, in which case sociology is subverted in the name of higher realms, or abandoned altogether. These papers, conveying as they do two decades of "work," reveal a man who is increasingly at ease with himself and his materials. Even the choice of title indicates the sense of proportion and balance that has characterized Becker's efforts of recent years.



The papers from the earlier years, particularly those done with James Carper in the mid-1950's, in their very self-consciousness of being novel, are also the least interesting. The three papers co-authored on the elements and development of identification with an occupation (Chapters 12, 13, and 14) summarize problems of adjustment of engineers and physiologists and philosophers so briefly and ineptly that substantive issues in career choice can simply not be explained. Instead, the materials are summarized by noting "that conflict does not necessarily occur in assuming an occupational identity; when conflict does occur, it centers around disparities between parental and occupational expectations" (p. 211). Unfortunately, such an abstract explanation does not readily come to terms with specific problems of adjustments to the materials that await the would-be engineers, physiologists or philosophers—or even the question of why parents react differently to particular occupational choices. The problem is not conventional occupational expectations, but hard-headed commercial expectations, a choice more likely cherished by parents than by students. Hence, what may present itself as a family dispute is more nearly differential response to the ideal of economic security. Discounting economic factors in social decisions permeates these earlier papers—a weakness that is less noticeable as Becker moves along to research in deviance, where economic considerations loom so large.

I mention problems in his early papers not to prove my "objectivity" by being hypercritical. There is no need to do so. Becker is a close personal friend, and he would remain so, even if he were not among those very few sociologists of this century who are leaving an indelible stamp on the discipline as a whole. What is intriguing is to chart the path from talent to brilliance—from sociology as a form of interpretation to sociology as a style of engagement. By 1965, Becker is working on non-college youth and specifically on conflicts that are not so much a function of familial or professional tensions as they are a reflection of social class and organization. "Perhaps because he is interested in the subject matter and not in more abstract goals, the trade-school student may have a problem not characteristically faced by college students. He may have to be concerned because he is interested mainly in the utility of the knowledge he is getting, the maximum amount of knowledge out of his teacher. When we consider that trade schools are profit-making organizations, it seems a possibility that teachers tend to string out the presentation of substantive material so as to spread it over a longer period of time and get

more tuition" (p. 241). As early as 1952, in discussing teacher-pupil relations, Becker appreciated the fact that all institutions have established assumptions about the character of society and the persons with whom they deal. By 1965, he managed to get at these assumptions and make them available to that public interested in problems of education. Indeed, one might read the volume as a treatise in the sociology of education, particularly if one includes learning as the essential core of education.

The first section of the book, "Problems of Sociological Method," presents the most satisfying writing in the volume. Becker is one of the foremost ethnographers in the profession. Therefore, to have these papers, some of them prepared specifically for this volume, and others that have appeared in relatively nonaccessible places, is a distinct advantage to those who wish to follow his career line. At the least, sociologists will proceed with caution when it comes to describing survey research and questionnaire data as "hard," in contrast to field study and life history techniques as "soft." Becker is much more apt to turn it around. For essentially he is saying that the type of analysis is not uniquely determined by level of precision. Beyond that, there are few forms of analysis available to the sociologist that are harder or tougher to refute than field work gleaned in interaction between investigators and investigated.

The phrase "sociological method" may be a rubric that covers more than it can possibly sustain. Chapters 3, 4, and 5, on field work evidence, the life history, and the scientific mosaic and social observation, could be classified just as easily as theory and perhaps as part of the sociology of knowledge. Papers 6, 7, and 8, on the nature of a profession, on problems in the publication of field studies, and the famous "Whose Side Are We On?," all have at least as much to do with ethics as with methods; in fact, perhaps a good deal more so. Even the first two papers, entitled "On Methodology" and "Problems of Inference and Proof in Participant Observation," contribute to fundamental sociological theory; particularly in their examination of how frameworks are chosen, what kind of hidden assumptions sociologists make in their work, the process of developing hypotheses, the credibility of informants, and the construction of social systems. Nonetheless, Becker's strategic decision in calling these papers methodological should be understood in a strategic context. For if one man's ethic is another man's nonsense, the same is not true when it comes to method. Perhaps it should be. Nonetheless, since we have made a fetish of methodology, Becker is quite

proper in saying that his own approach should be considered on methodological grounds rather than on ethical grounds—in which case, the word method simply comes to stand for what is objectively true.

All the noisy assaults upon "Whose Side Are We On?" have reflected a seeming incapacity to recognize that behind the assertion of the need for partisanship is not a reductionistic or tendentious desire for the advancement of a political ideology, but rather an attempt to get beyond partisanship. The aim is to re-establish an equilibrium in the evaluation of those who are ruled and those who do the ruling. The identification with the poor and with those labeled as deviant or marginals has as its purpose an attempt to redress a one-hundred-year imbalance in the history of western sociology, which assumes a one-to-one relationship between those who have authority and those who have truth—or at least those who have respectability with those who need problems solved. Beyond that, this major statement offers a theoretical approach concerning "the hierarchy of credibility," which in effect signifies that sociologists should not make the assumption that the "men at the top know best." On an operational level, Becker points out how social deviance is often more punished than political marginality, because there are simply no mechanisms of legitimation for deviants as there are for the third, fourth, and fifth political party advocates; or even devotees of political violence. That means, too, that there are no juridical safeguards for such people. In this sense, Becker's work is not only eminently political, but frankly humanitarian. That one could seriously read this paper as an argument for everything from welfarism to relativism is a tribute to a much lower order of partisanship than Becker is willing to entertain.

This book is also an important theoretical contribution to sociology in its distinction between *sociological* work and *social* work. Becker provides a feeling as well as a framework for starting with social facts that have to be understood before being corrected. His volitional model carries with it the further assumption that oftentimes the problem is precisely the fanatic dedication to removing a social problem, instead of a calm acceptance of the fact that there are pluralistic life styles that do not require removal or resolution. While the emphasis on this theme is more fully developed in Becker's *Outsiders*, it also appears quite noticeably in the third part of the volume, entitled "The Processes of Personal Change." A characteristic example is in Chapter 19 on personal change in adult life,

where he points out that: "One way of looking at the process of becoming an adult is to view it as a process of gradually acquiring, through the operation of all these mechanisms, a variety of commitments which constrain one to follow a consistent pattern of behavior in many areas of life. . . . The process of commitment accounts for the well-known fact that juvenile delinquents seldom become adult criminals, but rather turn into respectable, conventional, law-abiding lower-class citizens. It may be that the erratic behavior of the juvenile delinquent is erratic precisely because the boy has not yet taken any actions which commit him more or less permanently to a given line of endeavor" (p. 235). In other words, perhaps we should put less emphasis on removing juvenile delinquents from society and more emphasis on speeding the process of growing up. This kind of sociological imagination infuses the entire third section of the volume, and is the core of Becker's scientific posture.

An unusual aspect of this collection is that sociologism of the type exhibited by Becker, far from leading to impersonalism or dehumanization of people as "objects," leads to opposite consequences. There is an almost inordinate emphasis on personal choice and individual change. The concept of socialization is subtly transformed into a modest statement of career lines and patterned transactions which, in fact, somehow mediate the claims of institutions as structures on one side, and careers of individuals on the other (pp. 245-259). Then there is the concept of commitment, which figures largely in Becker's work. It is not that all commitments are made consciously or deliberately; nonetheless, he does speak of commitments in terms of "valuables" with which "bets" can be made in the world he lives in. Just how volitional and deterministic elements intersect, remains largely unexamined (pp. 271-272). Likewise, in discussing personal changes in adult life we are informed of commitments as "side bets." Social structure, while creating the conditions for change and stability in social life, strangely seem to be more permissive in the hands of Becker than for functionalists (pp. 286-287). It would seem that in "adult socialization" there is greater emphasis on self-actualization than any inhibitors to change. The theory of social and symbolic interaction in Becker has an interesting dimension that, while present in the world of his Chicago mentors, was not quite as developed; namely, the role of adult socialization as stretching the capacities of men to become something else. It is a most effective assault on the limits of psychoanalytic theory and its emphasis on child socialization. "Growing up" is something that goes on all the time.

That is why we need a sociology to explain psychoanalytic theory.

For someone who has for so long been identified with the Chicago school, and in general the use of sociologism no less than sociology, this heavy emphasis on personality and human choice will be seen as refreshing. Nonetheless, a certain problem arises that is typical of Chicago-school types, old and new, from Park through Becker. That is the exaggerated tendency to see volition and individual behavior as identical, and in turn to minimize or slight the role of ideology, organization, and determinist elements in behavior. In Becker's world, people are always making "side bets," decisions and choices as to presentation and behavior.

One cannot help but wonder whether such free choice is first of all structured in the situation, or simply in the eye of the actor. Further, the amount of mechanical activity and behavioral response to organizational pressures and institutional constraints is either left out, or reinterpreted in terms more amenable to personality, i.e., familial problems, etc. In short, the philosophical bias of the volume in favor of voluntarism may create a more "wide open" and hence more exotic (albeit "deviant") universe, than the one people live in every day.

*Sociological Work* is a rich resource. It requires no defense and warrants no assaults—at least not from me. It needs only to be read and understood.

### **NEW JOURNAL OF BOOK REVIEWS**

Beginning in 1972, the American Sociological Association will publish a new journal of book reviews, titled *Contemporary Sociology: A Journal of Reviews*. The *American Sociological Review* will then cease publishing reviews.

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**720 Main Building**  
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## BOOK REVIEWS

*Embattled Reason: Essays on Social Knowledge*, by REINHARD BENDIX. New York: Oxford University Press, 1970. 395 pp. \$9.75.

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Compare the trust in social knowledge and the distrust of human nature, look particularly at what reductionist perspectives have done to rational man, consider how knowledge may be safeguarded against ideology, eschew integrationist and evolutionary theories, and distinguish one's own position from other writers, especially Weber, Durkheim, Tocqueville, Marx, Freud, Bacon, Edmund and Kenneth Burke, Cassirer, Kuhn, Kerr, Daniel Lerner, Mannheim, Merton and Parsons—and one may then approach a conception of what Bendix attempts in this volume. Written over a twenty-year period, the first three chapters are grouped under "Conditions of Knowledge," the next three under "Theoretical Perspectives," and the final six under "Studies of Modernization." Images of man and society, issues of ideology and objectivity, and problems of generalization in comparative studies of cultures, social structures, and political communities are recurrent themes, diversely emphasized. Their discussion is imbued with special attention to meanings, nuances of description, and the historical and moral impact of competing claims.

Bendix's skill in presenting familiar and not-so-familiar generalizations in succinct prose adds clarity and quiet persuasion to his writing. Thus, Weber "does not view scientific neutrality as a by-product of the historical process as does Marx, of circumstance and personal quality as does Tocqueville, or of the effort to establish sociology after the model of the social sciences as does Durkheim. For Weber objectivity results from the deliberate efforts of the scholar." In a chapter on managerial ideologies Bendix writes: "But it is clear that differences between cultures and the particularity of historical events permit systematic comparisons and contrasts only in so far as the assumption is correct that frictions between managers and workers in large scale economic enterprises are universal."

To appreciate these and similar generalizations, they should be seen against the background of the author's resistance to received conclusions, in a day when sociology is sometimes given to claiming more than it deserves. To illustrate,

after briefly discussing three perspectives, society as an object of government, politics and government as a product of society, and society and government as partly interdependent and partly autonomous spheres of social life, Bendix appends the following footnote: "'Society as an object of state-craft' may be considered a Machiavellian approach, but this perspective is also characteristic of the social-welfare state which is not 'Machiavellian' in the conventional meaning of that term. Government considered as a 'product of society' might be called the sociological perspective, but this is also characteristic of Marxism which should not be identified with sociology, and then there are sociologists like Max Weber and Robert MacIver who do not adhere to this view. The theory of a partial dualism between society and government is a characteristic feature of European liberalism, but to call it the 'liberal orientation' carries overtones of a specific political theory which need not be associated with this approach. In view of such difficulties I have decided to avoid convenient labels and repeat the three phrases mentioned in the text."

One of the most intriguing features of the volume is the alertness of the author to the paradoxes, dilemmas, and uncertainties endemic in social science. The flavor and authenticity of this emphasis cannot be grasped without attention to text and context; nevertheless, a few passages may provide illumination. The prefatory remark that "collectively we seem to believe in science but not in reason, and we advance social knowledge while despairing of human nature" is reiterated with stylistic and substantive variations throughout the book. For example, sociology and psychoanalysis "cannot remain free of contradiction, if steps forward in the analysis of man are steps backward from the belief in knowledge 'for the benefit and use of life' . . . So far, few men have grappled with the corollary that the advancement of knowledge may one day destroy man's 'technical mastery of life'. . . . For scholarship the task ahead will be to moderate the unconditional quest for knowledge by an intensive concern for priorities, but without impairing the quest itself. Its tasks will also be to develop an ethics of conduct which can relate the scientist's indispensability and the powers created by knowledge to the weakness of the scientist as a man."

The necessary paradoxical metaphysics un-

derlying these and similar passages is revealed in the following: "Multiple, but finite, interpretability lies at the core of an empirical world possessed of continuous gradations and hence lacking natural benchmarks or distinctions. In such a world the scholar is obliged to imprint distinctions of his own devising which have nothing to commend them but a scholarly productivity for which we possess only proximate criteria. . . . The scholar who would comprehend that world must impose his abstractions, well knowing that they are indispensable and arbitrary."

Whenever a reviewer has little cause to challenge the organization and aims of a book, reference to scholarly ideals preferentially expressed is conventional. Speaking ideally, then, preference might have been given to a more systematic, less discursive treatment of the concept(s) of reason. Bendix uses reason to refer "to our estimate of the role of knowledge in human affairs," to different intellectual constructions which arise from the basic assumptions of social inquiry. He discusses belief in reason as an attribute of the quest for knowledge, the Marxist view that reason is historical, and concludes that reason is the result of pragmatic approximations, favorable or unfavorable, to the historical conditions of knowledge. He touches only informally and secondarily on deductive and inductive processes of reasoning, and makes no attempt to evaluate current designs of explanation and rhetorical argument. A critical examination of activist styles of "inference," choice and affirmation, might have, conceivably, enhanced our awareness of the respects in which reason is, indeed, embattled. However, it may be said in Bendix's defense that he ably exemplifies the rational man in search of knowledge, and demonstration of this may be more telling than analytic accounts of its principal forms. Still, such accounts might have enabled the author to sharpen his definition of ideology which suffers, like other definitions of this doughty word, from insalubrious confusion with the presuppositions of science.

Bendix believes that social scientists would be well-advised to distinguish between propositions about objects (object-language) and propositions about statements (metalanguage), since their logical attributes are different. "The content of a proposition in sociology is logically distinct from the conditions under which that proposition is asserted, and also from the criteria by which its validity is ascertained." Although Bendix recognizes that "a scholarly community is engaged in continual debate concerning these criteria" and shows concern for the conditions

and consequences of knowledge, I have the impression that he assumes sociological "objects" can be identified apart from perspectival interpretations of their conditions, consequences and validating criteria. This is a moot question since so much depends upon the manner in which metatheoretical, theoretical, and empirical criteria are distinguished. Bendix could have followed his pragmatic inclinations and drawn somewhat different conclusions. He might have said, for instance, that sociological objects are identified through their conditional and consequential relations and that the interpretation of these relations (transactions) are validated by our purposes (individual or collective). Thus, under condition S, "object" X may be treated as A for one purpose, as not-B for another purpose, and as C for a third purpose. In these terms scholars might agree that Weber, Marx, or Freud are each valid for their respective proponents. A universal theory of human action would then appear when comparative studies produced transactional laws so context-free, culturally independent, and pan-purposive that all humans would accept their validity.

The above are, however, heuristic comments and pertain less to the cogency of *Embattled Reason* than to its pedagogical and policy implications. The writing is, in its entirety, informed and judicious. The extensive notes compensate, in part, for the absence of an index.

*The Iron Cage: An Historical Interpretation of Max Weber*, by ARTHUR MITZMAN. New York: Alfred A. Knopf, 1970. 328 pp. \$7.95.

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*The Iron Cage* is a psychoanalytic interpretation of Max Weber's life and work. The title of the book refers to Weber's statement in *The Protestant Ethic* that modern man's life is determined by the iron cage of institutionalized asceticism. Mitzman accepts this proposition and analyzes Weber's conscious and unconscious reaction to the encagement, tracing his quest for liberation.

The book is divided into two main sections, the first dealing with Weber's early familial and personal associations, which are used to describe the conflict between his mother's religious standards of conscience and his father's ethic of happiness. According to Mitzman, the story of Weber's life and work is the story of his response to this tense situation in the Weberian household. As a young man Weber accepted his mother's values, and Mitzman judges that his

compulsion to work was an indication of this acceptance. Since Mr. Weber, Sr., was insensitive to Mrs. Weber's human condition and treated her in an authoritarian manner, Max, Jr., became antagonistic toward him; and Mitzman interprets Weber's attitude toward the Junkers' policies on the East Elbian land issue as symbolic of his antagonism to his typically Prussian father. Prior to his marriage, Weber's psychological stress continued because he was trapped in a dependency relationship with both parents—dependent on his mother for love and on his father for financial support. However, his marriage and financial independence only temporarily alleviated his stress. For a few years after his marriage he became bound up with guilt over his father's death, it having occurred shortly after he had ordered his father out of his house. Apparently the nervous collapse which followed was a catharsis as well as a precondition for his introspective analysis of asceticism as a social force.

In the second part of the book Mitzman briefly states the content of Weber's work on religious beliefs, sketches other scholars' influence on Weber's intellectual development, and interprets the last years of Weber's life as his progressive liberation from ascetic rationality. He does a penetrating analysis of Weber's theories of asceticism and mysticism, which, in his opinion, developed out of his ambivalence about the contrasting values of his parents. From this perspective, Weber's criticism of bureaucratic rationality is evaluated as symbolizing his rejection of his father as an authority figure. As a solution to oppressive bureaucratic rationality, Weber's choice of charisma, with its affinity to mysticism, signified his rejection of asceticism and his acceptance of the utility of other modes of being—the mystical, the aesthetic, and the erotic. Thus, during the last years of his life, Weber's judgment that the erotic and mystical were worthy of intellectual consideration was the culmination of his liberation from his own psychological prison built on the foundation of institutionalized asceticism.

*The Iron Cage* is a contribution to our knowledge of Max Weber and the history of his ideas. Mitzman has combined a chronological interpretation of Weber's personal life and scholarly activity with a report of Weber's attitudes about German politics. The task of integrating these three dimensions into a chronological unity is a difficult one, and portions of the book are, therefore, fragmented.

In the first section of the book Mitzman meticulously cites reports of events in personal correspondence by and about Weber. Many of

these events are quite ordinary; indeed, they seem to describe the condition of every man. I think Mitzman should have taken on the burden of the novelist and made the reader care about the person to whom they happened. He has not done this; and the reader loses interest, appreciating the first section of the book only after he has read the second.

Since Mitzman has selected the portions of Weber's writings relevant to his psychoanalytic frame of reference, *The Iron Cage* is not a comprehensive summary of Weber's ideas. Unlike many historians, who do not fully explain how or why they reach their conclusions, Mitzman has reported the events and presuppositions on which his are based. Scholars may disagree with his interpretations; they cannot ignore them.

*Politics and Society*, by ROBERT M. MACIVER.

Edited by DAVID SPITZ. New York: Atherton Press, 1969. 571 pp. \$11.50.

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"MacIver's eminent achievements, in both method and vision," Spitz writes, "stamp him as the most distinguished of social and political theorists" (p. xx). Certainly the breadth of the essays in this volume, chosen from those produced throughout MacIver's roughly sixty years of prolific and productive writing, demonstrates his tremendous versatility and welcomed literate, almost jargon-free, social science scholarship. Although all but seven of these "otherwise fugitive essays" were previously published, they are not minor essays. The topics range from political philosophy and ethics to normative sociology, and even critiques of serious contemporary cultural attempts in literature, music, and philosophy.

The book is divided into six parts, plus the first complete and extensive bibliography of MacIver's work. Part I consists of six essays on the organization of social science scholarship and its epistemology. Part II has seven essays on political philosophy, theory, and ethics, including one on his fundamental theme, "Unity and Difference: The Ordering of a Multigroup Society." Part III includes eleven essays on the key issues relating state to society—power, authority, order, liberty, pressure groups, and, especially, the differences between totalitarian and democratic governments in using these social forces. Part IV consists of five essays on the changing roles of governments in society with regard to labor, economic

activity, and social welfare. Part V has five essays on problems in international order and disorder. The six concluding essays of Part VI treat contemporary issues of education, poverty, civil rights, achievement-ascription and self-determination, the contemporary cultural "unbalance of our times," and the principles of abstract thinking, "contemplation," in dealing with these problems.

MacIver's work is a mixture of the analytic and the normative (cf. also p. xiii of the editor's Introduction). MacIver states at one point, "All education indoctrinates. I can conceive no education that does not" (p. 490). This includes his own writing. He is not content simply to comment on society or on others' conceptions of it. He is a forthright and intelligent apologist for democracy (cf. especially Ch. 19, p. 263, *passim*). He wants us to take it and him seriously. He writes in the essay form, a form intentionally created to convince people of the validity of arguments.

The reference point of his thought is the pervasive and almost mystical concept of social unity, or social bond, through a people's culture, that is, the "mutual tolerance and trust" people have for each other (p. 508). The problem with which he deals is that of the social unity of multigroup society, the problem of unity in the face of diversity. He recognizes two primary types of social unity: the one requires *conformity* of citizens *vis-à-vis* their individual and social goals in a society; while the other permits, indeed encourages, *diversity* (cf. Ch. 23). The first is best exemplified by absolutist totalitarian regimes, which of course he abhors; and the other by pluralist democratic orders, which he endorses. A major problem is that he finds excellent models for the former in contemporary history, especially Nazi Germany. But he points to no viable concrete models for the latter. They remain largely idealistic and normative.

These themes appear repeatedly as he interprets the rise and fall of nations, contemporary social changes, international or domestic conflicts, and even individuality. He is a man with the important vision of the free development of *e pluribus unum*, and he brings to bear powerful analytic skills to convince others of the viability of his vision and theory. He is a man who encourages us to use this vision in our social science (cf. especially pp. 418 ff.). In fact, many of his analyses are remarkably insightful, and with some changes in style, would be congruent with modern path analytic techniques (cf. especially pp. 322 ff. and 385 ff.).

Despite such virtues, there is also something sad about his work. Here is a man who has written beautifully and insightfully for over

half a century, a man who has been appreciatively and gratefully received by students, colleagues, and assorted official bodies. But here is also a man who recognizes the same problems near the end of his career as near the beginning and who, really, presents much the same type of response to them. His work moves from the more philosophical to the more practical over the years but has remarkable continuity. His work is extended and embellished; but the basic concepts, problems, and responses to the problems remain quite similar.

My observation is, that in his overwhelming concern to explicate his model properly, MacIver makes the relatively simplistic assumption that understanding an ideal is tantamount to its realization. Thus, his two major responses for dealing with problems are "a more socially relevant educational system" and "involvement and participation by men of good will in politics" (cf. especially pp. 180 ff., 195 ff., Ch. 28, 519 f.). Both are necessary, but neither is sufficient for the problems' magnitudes. The leaders of our great institutions, for example, certainly conceive of themselves as men of good will. But data show they have been increasingly ineffective in handling the problems of freedom and social unity. And it may be that a population's social consciousness precedes rather than follows its educational consciousness.

A second observation on his work, at least as presented in this volume, is that MacIver shows little awareness of certain processes which have been detrimentally affecting such social unity. He rarely mentions the effects of mass society, of inequalities, of bureaucracies, of high geographic mobility, of the fantastic power of relatively few super-national corporations, of remote governments and mass media. And he mentions not at all techniques for keeping a system responsive and pluralistic in the face of these conditions.

Instead, with only a few exceptions (cf. especially, p. 418) he characteristically reverts to the facile and normative "should," rather than producing directions for the kinds of detailed technical and propositional analyses and strategies necessary to raise the level of social unity with diversity in our communities and societies in the face of such adverse trends. He recognizes that the social unity of pluralism is in danger. But by omissions of important trends and by oversimplified responses to them, he may be seriously misjudging both the extent of the danger as well as its sources.

Of course, for a resolution, both the technical analyses and the vision are necessary. Indeed, it would be good if they could be combined in

dozens of such energetic and skilled men. What MacIver does do is to sensitize us to some complexities and likely pressure points in our vision. At a time when most of us seem to have little vision at all to guide our research and teaching, this is enough for one man. If more of us embodied his vision and productivity in our new and more powerful analytic tools, perhaps the more effective links among theory, method, and practice which he wants could be discovered.

*The Balance of Power in Society and Other Essays*, by FRANK TANNENBAUM. New York: The Macmillan Co., 1969. 368 pp. \$9.95.

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The eighteen essays in this book concern many relevant topics: international relations, trade unions, education, race relations, crime and the balance of power in society. Tannenbaum, a distinguished professor of Latin American history at Columbia University from 1936 to 1961, is now Director of University Seminars there. The essays, nearly all previously published, were written with policy concerns over a span of forty-five years. On these grounds, we might expect an illuminating and exciting book.

However, the essays often seem dated and conventional, and there are bothersome inconsistencies among them. The moral intentions and concerns evoke a sympathetic response, but too often the interpretations seem simplistically optimistic. These disappointments are partly a consequence of publishing a collection of essays written over many years, but partly the disappointments reflect changes in the world and our intellectual orientation to it. It will be worthwhile to examine some of these matters, even in a brief review.

First, it is necessary to specify the disappointments. Tannenbaum, in the title essay, emphasizes the importance of power in society. This discussion should have particular pertinence now, and in many ways it does. He says some wise things, for example, about the impossibility of society having a "sense of direction." Still, originally published in 1946, the essay seems alien from the recent American turmoil. The same is true even of the fine 1960 essay on the diversity of power and decentralization of government as necessary for political stability. The essays imply that the multiplicity of forces do tend to balance each other, at least in the

U.S.; the faith in pluralistic opposition reminds one of James Madison. The tone is certainly more optimistic than is that of current observers of power and conflict.

The inconsistencies among the essays are also significant. For example, given Tannenbaum's emphasis upon the balance of power within societies, his attack in other essays upon writers emphasizing the balance of power in international relations is surprising. His reconciliation of the inconstancy lies in the image of a possible federal world with each state equal in dignity (p. 96). But the denial of differences in power in such a conception seems to me to belie the significance of what he argues about power balances within societies. Another inconsistency may be seen in his attack upon economic determinism in discussing slavery (1946) and his argument that revolutions do not occur in industrialized societies (1931).

Another kind of failing is what seems to be a naive optimism. For example, in an essay revised for publication in this book Tannenbaum discusses the growth of international business corporations as an extranational base for a new political order displacing the international state system. Transnational developments certainly must underlie the emergence of a world society beyond the nation-state system. Yet, to discuss the international corporation as such a vehicle and to ignore the concerns of underdeveloped countries about "neo-colonialism" while commenting that China and Russia must come to participate in such new ventures if they are to "become industrialized in the sense the United States is," (p. 64), makes the analysis less credible than it would otherwise be.

What are the sources of these failings by a person of such stature? First, essays do not favor specification and elaboration; and the extended treatment which these topics receive in his books may clear up some matters. But the essays also betray the consequences of letting hopes affect analysis and of letting commitment to the dominant American ethos channel interpretations. If shared by the reader, this may not be noted. When the tragedies as well as the glories of America are experienced and subjected to intense criticism, it is difficult to share them unreflectively.

There are some sensible and significant ideas in these essays—for example, in his observation that cooperation depends upon equality (p.109). The issues discussed certainly deserve attention. Reading the essays may also instructively remind us that current radical ideas may appear as simplistic clichés 20 years from now.



*The Sociology of Marx*, by HENRI LEFEBVRE.  
Translated by NORBERT GUTERMAN. New  
York: Pantheon Books, 1968. 214 pp. \$5.95.  
Vintage Books, 1969. Paperbound. \$1.65.

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This little book requires two readings—one to get the text, writing style, and notes into some kind of comprehensible form, and another to excavate the substantive contributions of the volume. As regard the first, the challenge awaiting the reader is prodigious; regarding the second, the reader is left with the impression that Professor Lefebvre is often pregnant but not delivered.

The writing style or the editing or the translation of this work (it is impossible to identify which) ranges from being remarkably sloppy in some places to simply aggravating in others. On the aggravating side are such sentences as "Contrarily to what Hegel thought, . . ." (p. 5); "Political economy must be superseded, is capable of surmounting itself" (p. 15); "What does the revolution consist in? In this, that first to Hegel and then to Marx, the object of investigation and knowledge is time" (*sic.*, p. 27). In addition, not only are the footnotes at the end, but they are not divided by chapter (they simply run consecutively). Finally, it is disconcerting to discover Chapter 3 ending with ". . ." (p. 88) and to read some 11 times in the first 28 pages (I stopped counting after that) that Marx was expanding and/or reacting to the thought of Hegel. On the sloppy side are such items as footnote 2 on p. 201 which reads: "*Contribution to the Critique of Hegel's Philosophy of Right* (1843/44), opening lines" and the fact that the work does not contain a bibliography which would direct the reader to those "opening lines." Also, on p. 39 of the text: "Recognition of the subjectivity of other human beings does become a human—that is, a social—fact until the point is reached where. . ." Obviously, the "does" should be "does *not*," which of course completely alters the meaning of the sentence. In addition, the reader will find on the following pages *direct quotes*, the references for which are not noted anywhere in the text: pp. 57, 65, 68, 104, 135, 154–155, 172–173, 177, 174, 180, 182–183. Finally, although most readers would probably label as non-scholarly Lefebvre's statement concerning "higgledy-piggledy" tendencies in the contemporary Marxist movement (p. 35), this reviewer welcomed it as a refreshing interruption. Inasmuch as Lefebvre is identified as "the eminent French sociology professor and

researcher. . . who since 1960 has been director of the *Centre National de la Recherche Scientifique*, and holder of other professorial chairs" (back cover of the Vintage edition), I am inclined to indict both the translator and editors of this volume for an abominable job.

Chapter 1, "Marxian Thought and Sociology", tells us that the author will "attempt to re-construct Marx's original thought" (p. 3) and explore the idea that "Marx is not a sociologist, but there is a sociology in Marx" (p. 22). The chapter is confusing. In discussing the comment that Marx is often identified as an economist, the author, in the space of 4 pages notes: "This interpretation overlooks the subtitle of *Capital*, which was a 'Critique of Political Economy' (p. 14); then '*Capital* . . . is not a treatise on political economy' (p. 16); and finally 'The work contains some political economy' (p. 17). Marx's reaction to philosophy, religion, Hegel, and the state is briefly summarized in the chapter, although no earth shattering revelations are presented.

The real contribution of the book is found in Chapter 2, "The Marxian Concept of Praxis" although it is cloaked in the ubiquitous confusion. The concept of *praxis* in Marx's thought is an element that has been relatively neglected by Marxist scholars, and Lefebvre fills that void to a certain extent. His discussion is confusing in that one is never quite sure what he is calling praxis. He notes, for example, that praxis is "society's actual doing and making" (p. 31); is "defined by being opposed to philosophy" (p. 33); is "acts, courses of action, interaction" (p. 34); is "the practical-sensuous" (p. 38); and is "reality" (pp. 47, 71, and 119). One could develop the argument, I think, that the Marxian concept of praxis has theoretical affinity to Weber's concept of social action, but the relationship remains only implicit in Lefebvre's discussion. The author also makes a distinction (p. 44), based upon the work of Marx, between what he calls *poiesis* (activities concerned with physical nature) and *praxis* (activities concerned with human beings), which at first appears useful theoretically. The distinction, however, collapses as the author later notes: "Study of praxis (including the aspect of it we have named *poiesis*) . . ." (p. 56, emphasis mine). The real "meat" of this chapter (and of the book) lies in Lefebvre's discussion of three levels of praxis—the repetitive, mimetic, and innovating (p. 52). As types of social action these levels offer viable tools to get at the sociology in Marx; but Lefebvre remains, on this score, only pregnant.

Chapter 3, "Ideology and the Sociology of Knowledge," offers some interesting contrasts

between Marx's concept of ideology and Durkheim's "collective representations," as well as commenting upon the sociological features of ideology. Chapter 4, "Sociology and Social Classes," makes the case that sociologists should study revolutionary praxis—with which I readily agree. Chapter 5, "Political Sociology: Theory of the State," is a good summary of Hegel's view of the state and Marx's reaction to that view. There is also an informative (though brief) discussion of Marx's reaction to Lasalle's view of state socialism in which Lasalle sees the state as the manager of economic life. The chapter also contains (pp. 139–160) a good discussion of Marx's analysis of bureaucracy with commentary on how it anticipated the work of Weber. There is a final section on conclusions which ends prematurely by making a distinction between growth and development.

This is a frustrating book. The attempt at merging theory and action (through praxis) in the work of Marx needs to be done in sociological theory. Lefebvre, in my view, remains undelivered on this score. The book may be useful for graduate students studying Marx; if nothing else, it may provoke them. Undergraduates, I am afraid, will pull out their hair if exposed to the book. I would not enthusiastically recommend it to students or colleagues until it is cleaned up a bit.

*The Political Sciences: General Principles of Selection in Social Science and History*, by  
HUGH STRETTON. New York: Basic Books,  
1969. 453 pp. \$10.00.

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It is difficult in any science to formulate problems with sufficient precision so that decisions can be reached regarding the value of the conclusions. This book discusses successfully some of the major problems for the social sciences in this regard, under the headings of Truth (Part 1), Use (Part 2), and Truth in Use (Part 3). Stretton makes it quite clear that a central issue in the use of theories consists of satisfying the definition of truth inherent in the theoretical perspective pursued, recognizing that each theory can have a variety of usages that are frequently not compatible with one another. In addition, he makes it clear throughout the book that as a measure of truth each author of a theory ultimately must address himself to the relationship between that theory and social reality. The necessary isomorphism between science and the actual world rests, then, not only on the assumptions

*per se*, but also on the reasons why, and the way in which, assumptions are selected as major components of theories. In addition, assumptions render service in helping to explain the results of scientific inquiries as truth in use.

Underlying this complex theme is a high concern with the relevance of the social sciences for the everyday world. His ideas on relevance lead the author to conclude after elaborate examinations that social science inquiries are usually not successful because they have selected irrelevant areas of investigation determined by false selections of theories. This contention is supported by repeatedly maintaining that the social sciences by and large have not added valuable insights to improve social life over the last 30 to 40 years (the time of their most intensive and most supported efforts). Sociology and its offshoots come under particularly heavy fire because their explanations and discoveries often reduce to theoretical redundancy and are also not cast in a general theoretical framework having true causal laws. In this and other ways the author suggests his own supererogatory philosophy of social science, in which a distinction is possible using the relationship of inquiry with theory to decide which social sciences are "helping" mankind and which are not.

It might be true, as the author suggests, that the gap between social life and the social sciences is on the increase, and that it impairs the efficacy of the social sciences to help solve major problems of social life. This larger issue is not pursued in much detail, though it underlies the best discussions. Rather, the author concentrates most of his efforts on problems arising from the selective use of theoretical assumptions, hidden or manifest, as explanations in the social sciences. However, the categorical denial of all value to nine-tenths of social science efforts leads to an emphasis on ideology termed political science. The reasons for this orientation are most clearly pointed out in a statement on objectivity in the next to last chapter, which—except for the first sentence—is quite innocuous: "Science must always select; but selectors (*assumptions*) should be adjusted to purposes, rather than becoming restrictive theories of knowledge. It is often important to go about with all your senses sensitive, none atrophied, and no impressions banned. It is equally important to photograph, measure and count whatever your more poetic faculties might misreport. Many tasks in social science require the intricate relation of both types of observation" (p. 407). It would be gratuitous to assume that the author is unaware of the large number of objections that can be raised on different grounds against the first sentence.

He only introduces in a more sophisticated form a chicken and egg problem of ancient vintage: what was first, science or mankind?

Many of the problems raised are thought to have been overcome in the apparently purposeful and selective assumptions of "elementary economics." The isomorphism between science and reality therein expressed is the only one that finds favor, because it "prospered as it forsook generality, deduction from simple psychological axioms, symmetrically unified theory, and the search for sufficient explanation, for more practical attention to the facts of life and more specialization of diverse models and theories to the diverse particulars of relations in life" (p. 341). Two sentences later we are admonished that "People's dealings with money, and with the goods and services to which they are willing to ascribe money values, have high average reliability in advanced societies. . . . Much of economics can be value free in the sense that 'given goals' and comparatively simple instructions from comparatively single-minded clients supply all the values it needs" (p. 342).

Clearly this book is an attempt to resurrect the orientation of political economy. The author thinks in terms of adjustments made in economics in response to changes that occur in a money-economy. It is undeniable that definite economic relationships can be established under economic definitions. But imposing such redundant methods and their underlying appraisal of reality on the other social sciences only leads to the same sterility that characterized British political economy of 100 years ago. In contrast, social scientists today tend to think in terms of alternative explanations rather than responding exclusively to changes in social life in terms of one basic theory or one belief. As a result, they have been more aware than Stretton seems to be of the diversity, ambiguity, and contrariness of social relations—a sensitivity which furthermore seems to be absent from the author's main contentions as soon as he looks over his own hedge. As a result, the book, although enjoyable for its sometimes witty but often extrinsic elaborations, fails in its objective: a revolution in the methods of social science and the selection of its purposes.

*L. J. Henderson on the Social System: Selected Writings*, edited by BERNARD BARBER. Chicago, Ill.: The University of Chicago Press, 1970. 261 pp. \$11.50.

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Lawrence Joseph Henderson (1878–1942), a prominent biochemist who taught in both the

natural and social sciences at Harvard College and Harvard Medical School, was an important influence upon many leading figures in contemporary sociology, including his students Talcott Parsons, George C. Homans, William F. Whyte, W. L. Warner, and Robert Merton, and his colleagues Elton Mayo, Crane Brinton, and T. N. Whitehead. While Henderson was a precursor of the structural-functionalist tradition in American sociology, he is also representative of a generation of part-time sociologists who promoted the growing science.

Barber has provided an introductory essay as well as useful abstracts of each of the nine original writings in this collection. With the exception of the first of these essays, which was Henderson's only attempt at a systematic sociology, all of the others are presented in chronological order. While the editor might have provided more information about the sociological climate in which Henderson worked, both the publisher and editor have performed a valuable service in bringing L. J. Henderson's work into print.

The editor discusses five major themes in Henderson's writings. In the area of "philosophy of science and methodology," Henderson stressed the probabilism inherent in all science. Science is viewed as a process which results from "close observation and intuitive familiarity" with one's subject. Utility is the ultimate criterion for scientific concepts and theories. For Henderson, the study of the "trivial and vulgar," what some now call the sociology of the everyday world, was an essential part of the sociological perspective.

A second major theme is Henderson's "critique of positivism and excessive rationalism." Following the work of Pareto, Henderson rejected the psychologized view of man and argued for a sociological approach to the study of values and emotions in human relationships. A third and perhaps most important theme is Henderson's use of "the concept system and social system." While Henderson saw great utility in the idea of systems, he also warned his students "that convenience often imposes a certain measure of oversimplification" (p. 88). His own writings are replete with analogies from biological systems, yet he argued "do not reason from this or any other analogy" (p. 113).

A fourth and related theme is Henderson's instructions on "the usefulness of Pareto's work." He felt that Pareto was one of the few men in history to "write what men do and not what men ought to do" (p. 189). Finally, Henderson must be credited as a founder of the field of "medical sociology."

What strikes this reader is not simply the style of the man's work, but the contemporary

relevance of his ideas. Those who would dismiss this volume as simply an historical document from an "ancient" phase of American sociology would do so in error. L. J. Henderson's work reminds us of both the infancy and the maturity of our discipline.

*Methodology: Foundations of Inference and Research in the Behavioral Sciences*, by ADRIAAN D. DE GROOT. The Hague, The Netherlands: Mouton and Co., 1969. 400 pp. 42 Dutch Guilders.

BENJAMIN G. DENNIS  
*University of Michigan, Flint College*

It is a rare pleasure to read a book in which there is a clear and self-conscious effort to simplify complex problems of research methodology, a book aimed at reducing the jargon of conceptual terminology and efficiently developing a systematic theoretical statement. De Groot has accomplished these tasks and has rendered a significant service, especially to beginning students interested in methodology and to others interested in the interdisciplinary approach. It was natural for him as a European to have relied heavily on literature from European scholars rather than from Americans. In a similar vein, as a psychologist he drew more on the literature of psychology and psychiatry than of sociology, anthropology, political science, and history. However, the book represents a generally useful recipe of research methodology.

The book has all of the basic ingredients necessary for texts on methodology. However, the book tends to be redundant and tasks the imagination of those sophisticated in methodology. The author is aware of this when he writes that "the reader who wishes to adhere to a stricter canon of research behavior can add his own specific rules to those he finds in the present volume" (p. vi).

The author's predilection for applied problems of research, on the grounds that such problems have some major advantages, clearly influences his choice of, and reliance upon, research examples using non-parametric statistical tests. The congruency of non-parametric statistical tests with the tenets of applied research is self-evident, since non-parametric tests make no specific assumptions regarding the distribution of the variable within the population. The major advantages asserted for the "applied" emphasis are that the researcher cannot hide easily in the technicalities and jargon that often permeate and besiege pure research and that the cost of methodological refinement must be weighed openly against the social importance of the

expected outcome—which is bound to foster methodological soul-searching and self-criticism (p. vi). While these assertions are cogent, they are an invitation to a partisan and simplistic approach to the problems of empirical research. If one accepts unequivocally De Groot's position, one must also accept the transformation of empirical researchers into social workers. This means that the standards of empirical research must be geared to the thought patterns of non-researchers who judge its importance and acceptability in regard to what it does for them. Applied problems, however subtle, are defined in terms of ameliorating social problems rather than cultivating the mode of understanding of such problems—the hallmark of true scientific research.

Notwithstanding, De Groot succeeds in pulling together and organizing the significant elements of empirical research into some sort of unified whole and in introducing students to the more important steps in social research. He ingeniously begins the exposition by the introduction of a subtle concept ("empirical cycle in science," p. 1). The acquisition of this cyclical experience in empirical science makes possible the awareness of the world or at least the sector of the world in which the observer is interested; such awareness gives rise to the selection of problems for investigation. From this, De Groot proceeds systematically to the cords of scientific research, the designing of theories and hypotheses. His elaboration of these two basic concepts with their ramifications constitutes the major portion of the text—which is as it should be. His ability to introduce, manipulate, and integrate various concepts which most books of methodology take for granted and his insistence upon operationally defining each concept is a tribute to a high quality of professional competence.

*Issues in Participant Observation: A Text and Reader*, edited by GEORGE J. MCCALL and J. L. SIMMONS. Reading, Mass.: Addison-Wesley Publishing Co., 1969. 359 pp. Paperbound. \$5.95.

KICHIRO IWAMOTO  
*University of Santa Clara*

McCall and Simmons state in their Preface that "of all the research methods now employed in various disciplines, direct field work . . . is the least systematized and codified. . . ." The authors' goal, then, is to bring "together in one volume the full range of problems and issues [in field work] . . . presently available in [the] literature." The editors have attempted to combine the best of two worlds by designing their

book as a text and a reader: "the present volume while not quite a textbook is rather more than a book of readings or an analytic review of the methodological issues." Upon examination of their volume this becomes quite evident, since less than 40 of the 340 pages are the authors' own comments. It would be unjust, though, to review this work only in terms of quantity of pages contributed by the editors. In fact, McCall and Simmons have performed admirably in terms of their stated goals, and have assembled a well-organized set of readings that are thoughtful and well-annotated.

Their personal contributions perform a variety of functions. The chapter introductions, subsummaries, and conclusions at their best contribute to the integration of the various articles within the theme of each chapter. The introductory comments usually provide sufficient information to allow a reader with little knowledge of field work techniques to gain from a chapter's selections. The comments also contribute to the integration of the chapters by referring to ideas from preceding chapters. Another type of comment by the editors cites and briefly summarizes references which are not included in the readings, but which are relevant to the issue or ideas in the preceding article or section. There is also a fairly extensive bibliography at the end of the book.

Three different styles of type were employed in the printing of the book. One style signifies a reprinted article, another style identifies quotations, and a third style is used with McCall and Simmons' comments. Unfortunately the styles of type are not distinct enough, and at times one has difficulty recognizing where a selection ends and where the editors' comments begin.

The editors selected a representative collection of articles dealing with both the analytical implications of participant observation, such as generation of hypothesis, validity, and reliability, as well as the practical problems of proposing time schedules, paperwork demands, and rules of thumb for data collection, thus introducing the reader to the various issues involved with this approach and guiding him step by step through the various stages of this approach. He is given a picture of the development of participant observation techniques by the inclusion of some historical issues (e.g., analytic induction debate *à la* Znaniecki, Robinson, Tumin). The final three selections present Becker and Geer's comparisons of participant observation and interviewing, Trow's comments criticizing their over-statement of the applicability and combinations of this technique, and then Becker and Geer's rejoinder. McCall and Simmons' inclusion of these selections demonstrates ade-

quately the problems of the inefficient terminology of our discipline. The concept of participant observation has been used to refer to both a single research technique and also a style or strategy of research combining several techniques such as "direct observation, informant interviewing, document analysis, respondent interviewing and participation with self-analysis." At the conclusion, the editors clearly state their preference for the latter interpretation and construct a fairly strong supportive argument through their selection and organization of articles.

Though this volume presents no new insights in the area of field research, the editors do bring together many significant articles on this subject, making the book beneficial to graduate students, practicing sociologists, and advanced undergraduate students.

*Marginal Natives: Anthropologists at Work*, edited by MORRIS FREILICH. New York: Harper and Row, 1970. 624 pp. \$8.95.

EDWARD M. DOLAN  
DePauw University

Aram Yengoyan, one of the contributors to this book, writes that "the ethnographer who feels . . . he must be accepted and 'liked' by all members of the group he is working with is fairly naive. . . . It is practically impossible to immerse yourself fully in another culture." Such reasoning leads to the conclusion that anthropologists in the field can never be more than "marginal natives"—hence Freilich's title.

*Marginal Natives* includes ten "what-happens-in-the-field" essays, anecdotal and virtually theory-free, written by contemporary American ethnographers, followed by a lengthy concluding chapter by Freilich in which he seeks to establish operational and theoretical bases for the field worker.

Each of Freilich's contributors—Nancie Solien González, John Gulick, John Honigsmann, Robert Maxwell, Pertti Peltto, Melvin Perlman, William Schwab, Norman Whitten, and Aram Yengoyan—was presented with an outline suggesting that the essays be developed in three parts: (1) discussion of an earlier (usually the first) field project the writer undertook, (2) a later project, and (3) comparisons between the two experiences. There are no attempts at formal "field reports." Rather, each tells how he went about developing and carrying out his project, how he conducted himself in an alien culture, and what happened in the field.

As Freilich puts it, contributors to this book entered a public confessional and by so doing

help to disperse the myths of life-in-the-field. The many problems faced in the field and dissatisfactions with attempted solutions to those problems, plus lack of knowledge of the similar plight of other field workers, often lead beginning anthropologists to unduly harsh evaluations of their work.

Unlike most other social scientists, the anthropological researcher is often a "loner," cut off from friends and family, without the comfort of familiar surroundings, and oftentimes forced into contact with not always pleasant natives. Under such circumstances psychological and physical well-being become problems. Honigmann's first seven weeks in the Canadian north were described as "dreadful"; Schwab, in Nigeria, found that he and his wife were under constant scrutiny; Whitten, in Ecuador, and Maxwell, in Samoa, suffered ill health; and Yengoyan, among the Mandaya of the Philippines, ran into a segment of the population openly contemptuous of him.

The book is aimed at the advanced graduate student about to embark on his first field study and plagued with doubt as to his ability to handle the seemingly innumerable problems of preparation for, and conduct in, the field. To that student it may come as a pleasant surprise to learn that proven field workers are assailed by the same doubts and, at times, seem unable to make the right decisions. The student will learn, too, that opportunism (or better, perhaps, flexibility) plays a part in the choice of a field project: Honigmann, blocked from an intended research area by impassable roads, turned to a neighboring community; Maxwell, free to go wherever his passport would permit, spun a globe and picked an exotic and exciting-sounding place. Only too often availability of funds determines where and, to some degree, how one will work. Aside from training and intelligence, adaptability is one common quality possessed by experienced field workers.

Discussion of pragmatic problems covers such disparate topics as choice of a graduate school, development of a field project, preparing to leave the country, and combatting rumors that one is a spy. Theoretical details, revolving around human behavior and theories of methodology, borrow heavily from sociology and psychology. Freilich concludes with a strong plea for scientism in anthropology, and suggests that statistical methods must be used. The charts and mathematical-like equations sprinkled through the final chapter may contribute to scientific respectability; certainly they are necessary to follow the Freilich discussion; but they do make it difficult for the reader to maintain continuity of thought while flipping back and forth between text and chart.

This book should find a place in graduate field work courses. Moreover, the discussions of life in the field are often entertaining reading.

*A Way of Seeing*, by MARGARET MEAD and RHODA METRAUX. New York: The McCall Publishing Co., 1970. 335 pp. \$7.95.

ALICE B. KEHOE  
Marquette University

Eight years of columns written for the magazine *Redbook* have been collected by Mead and her long-time friend Rhoda Metraux into a set of short essays commenting upon, according to the book's table of contents, "Perspectives on the World," "The Public Good," "The Private Good," "Dialogues" (discussions between the authors and women invited by *Redbook* to air the topics "peace" and "marriage"), "Categories of Belief," and as an epilogue, "The Measure of Freedom: New Guinea Revisited."

Obviously not designed for a professional audience, *A Way of Seeing* is a record, as Mead remarks in her foreword, of "personal responses to events at different moments in time." Thus she considers student riots, the generation gap, and couples who flee the anonymity of the city. Mead claims, however, that her responses are more than merely personal: "It is the anthropological viewpoint that gives focus and continuity to these many different essays on our contemporary life." This anthropologist, for one, cannot accept that statement.

In this book Mead calls for two kinds of legal marriage, one intended for cohabitation without the production of children, the other intended for parenthood—more difficult to enter into and to dissolve. Anthropologists certainly know of many societies that recognize two forms of marriage, but the two forms differentiate the status of wives into matron and concubine or the status of children into those for whom full economic reparation has been made so that they belong wholly to their father's clan, and those for whose mother bride-price is still owing so that the mother's clan retains some jurisdiction over them. A legalization of cohabitation with no expectation of offspring is, to my knowledge, a unique invention by Mead and quite counter to the anthropological generalization that marriage is basically a means of ensuring the physical support and proper socialization of children.

Mead urges the universal adoption of the language of a small literate nation as a second language for all peoples to facilitate the evolution of a true world community. Her argument, following an excellent short exposition of the

principle of redundancy in language, is that artificial languages lack redundancy and therefore lack richness, while the major spoken languages now threaten to exterminate the linguistic heritages of the politically unimportant peoples. (Emphasizing the desirability of all peoples learning a second language would presumably protect the minor ones against linguistic imperialism). Anthropological experience would suggest that the strong but subtle link between language and cognition renders the learning of a language without a true immersion in its associated culture a superficial and transient exercise. Teaching Albanian or Andamanese to every child in the world would simply fill the world with people who briefly memorized and then forgot Albanian or Andamanese, as the United States is filled with people who took French in high school but are wholly unilingual. If Mead were thinking this problem out as an anthropologist, she might suggest Liverpoolian English as a universal language because the popularity of the Beatles gives that dialect an emotional effect and an experiential background promising true fluency among millions of youths.

Mead believes households should contain more than one woman, partly to accustom children to various personalities and sources of care, partly to facilitate the careers of those mothers who wish them. Again, anthropologists may be familiar with multi-woman households; but such polygynous or extended households contain only many women doing the same tasks. It is here that the true source of Mead's way of seeing becomes apparent. Mead's own mother was a sociologist, and both as a child and as a mother herself she has lived in the household type she recommends. An anthropologist notices that Mead, like nearly all her fellow humans, holds values reflecting the cultural pattern in which she matured.

The sum effect of Mead's collected essays is Margaret Mead, vigorous, intelligent, sincerely desirous of bettering the world. This Mead is an anthropologist by profession, but when she writes for *Redbook* she writes not as an anthropologist but as a voice of American liberal humanism.

*The Machiavellians: A Social Psychological Study of Moral Character and Organizational Milieu*, by STANLEY S. GUTERMAN. Lincoln, Neb.: University of Nebraska Press, 1970. 178 pp. \$9.50.

GARY L. ALBRECHT  
Emory University

There has been quite a lag between the development of the Machiavellian scales and pub-

lished research based on this personality variable. To date, Guterman provides the only detailed empirical report on Machiavellianism and its correlates in book form, although Richard Christie and Florence Geis' *Studies in Machiavellianism* is now in press.

The Machiavellian manipulator seems unconcerned with morality in the traditional sense, is basically cool in interpersonal relationships, seems to be more concerned with means rather than ends, and is most rational in dealing with others. A series of statements that were judged to be characteristic of this ideal type were selected from *The Prince*, *The Discourses*, and other sources. These items evolved into three usable scales: the Mach IV scale, the Mach V scale, and the Kiddie Mach scale. Mach IV is the original scale with counterbalanced items. A forced choice version of this scale, Mach V, was developed to offset the observed negative correlations between Mach IV scores and social desirability scores. The Kiddie Mach scale was constructed to use with children and low-education adults.

Since the Machiavellian scales were produced on the basis of some hunches and insights into behavior patterns, the scale did not have a strong theoretical foundation. The major thrust of Guterman's book is "to explore the sources of the Machiavellian out" (p. xvii) and to examine the relationships between the Machiavellian personality and social structure. Thus, the book makes a much-needed theoretical and empirical contribution to the study of the nature and correlates of Machiavellianism.

Guterman is extremely interested in the relationship between Machiavellian personalities and morality (norm violations), and how the Machiavellian reacts to social control mechanisms. He focuses upon three sets of independent variables to explain the Machiavellian personality, starting with Freud's theory of the superego which is used to derive a set of hypotheses relating the development of Machiavellianism to family milieu during childhood and adolescence. He then employs Cooley's theory of the looking-glass self to derive hypotheses relating "ethicality" (p. 40) to sympathy and the need for social approval. Next, he examines the moral outlook of the individual in relation to the solidarity of the group or the society within the context of Tönnies' theory of *Gemeinschaft* and *Gesellschaft*.

Despite the usual pejorative connotations that surround the word "Machiavellian," none of the initial research by Christie and others supports the contention that high Machs are more hostile, vicious, or vindictive than low Machs. Instead they seem to have a "cool detachment" towards people, issues, and situation. Guterman

finds that the perceived Machiavellianism of the parents, perceived rapport with the parents, perceived strictness of the parents, and perceived punitiveness of the parents all relate to Machiavellianism. He also demonstrates that sympathy and the need for social approval correlate with Machiavellianism. In addition, the relationships are positive between Machiavellianism and friendship ties. Finally he discovers that Machiavellianism correlates with the urbanism of residential locality during adolescence but not during the present stage of adulthood. Guterman concludes from these data that "most likely Machiavellianism is a relatively stable characteristic molded by influences during childhood and adolescence and usually not subject to change after a person enters adulthood" (p. 79).

Part II of the book attends to the relation between personality and social structure. Low feelings of solidarity on the part of employees are associated with a high level of Machiavellianism, a discrepancy between the management and staff in Mach scores, and heterogeneity of Mach scores among the employees. Furthermore, the personality characteristics of the management affect the solidarity feelings of the staff.

This book makes some important contributions towards an understanding of the theoretical bases of the Machiavellian personality and towards further delineating the interrelationship of personality and social structure. The data show in the study of conscience that the parental variables "do not operate in the same manner in all social environments" (p. 80). Further, there is a statistical interaction between parental behavior and social environment that must be taken into account by any theory of conscience.

Despite these conclusions, the book has some apparent weaknesses and leaves many questions unanswered. The author states that the contribution of the book "has not been to suggest a synthesis" (p. 80); indeed, the author is correct in his assessment. The book is not well integrated. There is no convincing argument proposed for the selection of his particular sets of independent variables. Further, why stress the theories of Freud, Cooley, and Tönnies? Guterman had difficulty in operationalizing each one of those theorists, which made his results and conclusions less convincing. The sample itself also provided problems. He sampled the management and staff of the Hotel Corporation of America and Treadway Inns. After stating that he does not have a probability sample and that statistical tests of significance are, strictly speaking, not appropriate, he

then proceeds to use them. In addition, his presentation of the data is not clear. He uses a contingency table analysis in which he frequently presents a separate chi square for each row and column of the table. He also discusses correlations between variables without giving a measure of association until he has presented a large amount of data. This analytical approach and presentation is rather cumbersome and at times confusing to the reader. Anyone else doing research in this area would also be well advised to pay strict attention to the indices and operational measures employed by Guterman. Do they really measure what they purport to measure? Might some of the results be different if the measures were changed? For instance, in his treatment of childhood and adolescent experiences he relies totally upon the perception and recall of the subjects.

Guterman has made a major contribution to the literature on Machiavellianism by presenting a body of data which test hypotheses derived from some theories of conscience. However, all of the evidence is not in. This is an important and very suggestive initial step, but more careful research must be done. How is the Machiavellian socialized? Does his socialization continue in the adult years? What are the interrelationships between the Machiavellian and various social structures? What are the roles and occupations where the Machiavellian is most successful? How is the development of conscience related to Machiavellianism? In what ways does the Machiavellian respond to agents and mechanisms of social control? Is Machiavellianism related to personality variables other than the need for social approval? As Guterman suggests, are deviants and non-deviants different in regard to Machiavellianism? And, how is the development of Machiavellianism related to an individual's opportunity structure? Guterman's book is more provocative than conclusive.

*Euratlantica: Changing Perspectives of the European Elites*, by DANIEL LERNER and MORTEN GORDEN. Cambridge, Mass.: The MIT Press, 1969. 447 pp. \$12.50.

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Lerner and Gorden have assigned themselves a very ambitious task: to document empirically the adaptation of British, German, and French elites to the "loss of European hegemony over the rest of the world" following World War II. Their book is divided into four parts: contexts,



issues, transformations, and perspectives. Essentially, "context" involves the loss of world hegemony, this process comprising several stages (anxiety, acceleration, acceptance, and ambivalence). The first part of the book is analytical and contains a number of important observations, e.g., "that part of the American Model which has been most fully absorbed and adapted is its allegedly technological component. Not so the psychocultural and sociopolitical components . . ." (p. 28). The reader expects, of course, that the isolation of these stages will serve the analysis and that they will be demonstrated empirically within the text. After all, the authors set out to "write contemporary history in a new way"; and this attempt ought to involve the usage of the empirical techniques of the social sciences. However, these stages are never discussed again, a surprising omission since the authors interviewed a panel (some respondents being interviewed several times between 1955 and 1965).

One of the issues facing European leaders is protection from outside aggression. The authors claim that the respondents "share a common view of international tension," a claim weakly substantiated by the data since the French panel always differs from the German and British respondents. Clearly demonstrated, however, is the fact that the same respondents no longer want the NATO posture to be one of deterrence and, while committed to NATO, they are not willing to share the cost of running that organization.

Another issue involves prosperity. We are told that, from the beginning, economic welfare was seen by European elites as depending upon integration. Outside of economic welfare, however, European leaders showed strong diverging views concerning the purpose of the Common Market. It is implied, although never clearly said, that European leaders want to integrate, providing each country gets something out of it. Integration, it seems, is taking place largely on the basis of nationalist considerations.

Part C explains why the panels "have been able to converge so rapidly toward a consensual set of attitudes on their key issues." These attitudes are limited to two broad issues: the perception of defense as depending upon common action, and of economic prosperity as depending upon European integration. What the authors do not seem to realize is that the consensus they demonstrate does not involve specific issues. When the panel is asked to give its views on concrete problems—as for example the goals of the Common Market—the alleged consensus disappears, showing that a consensus can always

be found providing the order of abstraction be sufficiently large. One also wonders why, if the consensus was so overwhelming, Great Britain is still facing very stiff negotiations in Brussels after de Gaulle's departure?

If the main conclusion is doubtful, so is the methodology. We are not told how elites were defined and the sample drawn. Further, a proportion of the sample had to be abandoned due to cost considerations. Who were these people? The authors relied on interviews, but the potential richness of interviews is never utilized. The questions asked, in fact, may not enable an adequate testing of the hypotheses, yet the lengthy interviews could provide some useful answers. The simplicity of the methods leads to simplistic answers. For example, transnational solutions may not imply a change of ideology, as the authors claim, but rather a change of strategy (again, interviews could provide the answer). Ironically, this change of strategy is substantiated by a number of statements the authors make.

This simplification of issues is more clearly visible in the claim that pragmatism has replaced the old ideology (Class and Nation). But when have leaders not been pragmatic? Several times the authors create a mythical past which help them in demonstrating their case. They should instead have limited their comments to changes within the panel. On the issue of pragmatism, for example, one wishes that Chamberlain and Daladier had been less pragmatic or that the Vichy Government had been less aware of the realities of German occupation and cooperated less enthusiastically. On the issue of the now prevailing pragmatism, Lerner and Gordon should remember that the decay of European aristocracies (especially in France) is a 19th century phenomenon, and that senior civil servants have been, for the most part, meritocrats for a long time (university education in Britain and Germany, *grandes écoles* in France). It is therefore not at all convincing that a consensus has developed. As usual, leaders are engaging in pragmatism and are realizing that economic welfare depends upon some form of integration.

The theoretical framework implicit in this work is that events lead to attitudes and attitudes to behavior. That is very plausible, but the attitudes should be more closely analyzed. Further, one would like to know what kind of behavior is involved. In a short appendix one is told that elite attitudes predict governmental policy in the majority of cases. This is empirically debatable, especially in light of the very slow progress made by European integration.

In addition to demonstrating that elites can be studied empirically over time, Lerner and Gorden have isolated a very important problem and even outlined a possible way of tackling it. Yet, it would seem that greater care in the sampling would have answered one important question: do elites adapt or are they replaced? Utilizing the richness of interviews and, possibly, a greater awareness of the sociological problem they were treating would have yielded some useful information. It is not at all sure that the elites of the three countries studied in fact adapted to a common problem in the same manner. The problem these elites were facing may have been defined much in the same way (and, on this point, Lerner and Gorden's case is strong); but the adaptation may differ. One can only hope that the errors of pioneers will be avoided by those who continue this important line of research. Such followers should, in any case, read carefully the author's chapter on "feel of the field" in order to learn how data for this type of research must be gathered.

*The Age of Aquarius: Technology and the Cultural Revolution*, by WILLIAM BRADEN. Chicago, Ill.: Quadrangle Books, 1970. 306 pp. \$7.95.

CLAUDE E. STIPE  
Marquette University

The author, a reporter for the Chicago *Sun-Times*, derived the title of this book from the astrological determination that we have reached the end of the "bitter and divisive Piscean Epoch" and have entered the "harmonious Aquarian Epoch." His discussion of the cultural revolution in American society places in opposition those oriented toward science and those oriented toward the humanities. The former are seen as contributing to the "technetronic society" in which men are indentured to their needs or supposed needs which require them to maintain a certain income. The latter are more concerned with the present suffering of man and the possibility of a society in which man is liberated from those repressing needs. According to Braden, his purpose is to demonstrate that the

on ecology, environmental control, and the often mystical worship of nature; in the reassertion of ethnicity; in the now-defunct Death of God theology as well as the newer attraction referred to as the Theology of Hope (pp. 7-8).

He associates technology with a patriarchal emphasis and humanism with matriarchy. Since many adolescents today are essentially raised by their mothers, who are often in humanistic pursuits, "the female eros may be responsible in part for an anti-rational reaction to technology and the formal logic of science" (p. 173).

After considering the effect of these opposing stances on a wide range of developments in American society, Braden concludes that he would not care to be governed by self-appointed leaders, whether of the Black Panthers or the New Hope theologians. He sees a dictatorship imposed by revolutionary elitists as no less repugnant than a dictatorship by technetronic elitists (p. 214).

The protest of the Cultural Revolutionaries is accepted as relevant because they have challenged our present concepts of technological growth, but "the militants have failed to challenge any of our fundamental assumptions about technology. They would offer us at best a change of managers" (p. 291). He concludes that the problem is not so much capitalism *per se* as it is unbalanced development, and that what society needs is a "humanized technology" rather than a choice between either technology or humanism (p. 292).

Braden has obviously read widely, taken copious notes, and attempted to organize the material in a meaningful way. Over 160 people representing almost every academic discipline, as well as novelists and poets are alluded to, 23 of whom were interviewed personally. However, it is extremely frustrating to find that there is no bibliography and that less than a dozen citations include both the source and date. Even direct quotations may be identified only by the name of the author. Many statements are not supported by any form of reference; for example, he states that there is "evidence that the radical-producing family is typically matriarchal in character, centered around a nurturant mother figure" (p. 9), but gives no indication of the sources for that evidence. It is often difficult to distinguish the author's views from those whose material he is using, especially since he often presents two opposing views with no attempt at evaluation. In a random sample of 29 pages, 40% of the text consisted of direct quotations, and a large proportion of the remainder was paraphrasing. The problem is compounded by the inconsis-

Two Cultures conflict can be seen as a basic factor in the Black Power movement; in the protest of the New Left; in the supposed confusion of sexual roles; in the development of an LSD culture; in the so-called Leap to the East by many drugstore disciples of Hinduism and Zen Buddhism; in the current faddish enthusiasm for astrology, witchcraft and sensitivity training; in anti-scientism; in the newly emerging emphasis

tency in handling quotations: some very short ones are italicized, whereas much longer ones are included in the body of the text.

One often wonders whether an author with Braden's background understands the material he uses, and there are indications that he failed a number of times. A few examples suffice. He states that Freud defined ego as a part of the id (p. 2), rather than realizing that Freud saw the id and ego as separate systems of the personality. He claims that Whorf "has shown that man's perception of the world is programmed by his language, and that men from different cultures live in different sensory worlds" (p. 35). It is true that Whorf presented this as a theory, but linguists generally do not agree that he demonstrated its validity. Commenting on one of the families in Oscar Lewis' *Five Families*, he says that "in effect they really have no culture at all" (p. 226), which is a meaningless statement since no one can live without culture, and it also contradicts Braden's earlier definition of culture. One of Fletcher's examples in *Situation Ethics* was grossly misrepresented (pp. 284-285), but whether this was the fault of the author or of a secondary source he used is not clear.

In spite of its faults, this book is an interesting attempt to explain the basis of the cultural revolution in American society.

*Changing Characteristics of the Negro Population: A 1960 Census Monograph*, by DANIEL O. PRICE. Washington, D.C.: U.S. Department of Commerce, Bureau of the Census, 1969. 259 pp. \$2.75.

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This is the fourth in a series of census monographs designed to "provide interpretation of census and related statistics that would illuminate current problem areas." Price undertakes the specific tasks of delineating major trends in the internal migration of the Negro, assessing shifts in employment and economic status relative to whites, and determining the nature of changes which have occurred in their educational attainment, marital status, and family composition. To this end he employs a cohort approach in addition to the more standard cross-sectional analysis of his data.

Cohort analysis enables Price to show the cumulative net effect of migration through time on initial cohorts of 1000 persons 0 to 4 years of age by color and sex for both rural and urban sectors of the major regions of the

United States. Successive cohorts of Negroes are characterized by an "increasing rate of out-migration from the rural areas of the Secessionist South," and an increasing tendency to bypass the urban areas of the South in favor of direct moves to northern urban centers.

The major part of this work is devoted to comparative analyses of changes in occupational distributions of white and non-white populations. Percentage distributions are based on total populations in each age-sex-color group, rather than the total experienced labor force, in order "to see the consequences of some of the movement of individuals in and out of the labor force with changing age." Notwithstanding the importance of these consequences, perhaps it would have added to the clarity of the analysis had this topic been treated separately within the monograph. If the primary focus was intended to be on changes in occupational characteristics of non-whites relative to whites actually in the labor force, extraneous variations induced by differential movement into and out of the labor force would make it difficult for anyone but the more experienced to assess correctly the significance of changes in the relative size of specific occupational groups through time.

Utilizing both cohort analysis and an index of occupational change applied to cross-sectional data, Price shows that a significant improvement in the occupational position of Negroes did occur during the 1940-50 decade. More important is his finding that a continuing improvement into the 1950-60 decade occurred only for females. Progress for Negro males was relatively insignificant, and in some areas the status associated with their employment actually declined. The more favorable situation for Negro females is attributed to their generally higher educational achievement, *vis-à-vis* the Negro male; and the fact that females are continuing to increase their educational advantage does not provide any basis for an optimistic prognosis for the economic future of the Negro male. Analysis of marital status and family composition shows the effects of the more disadvantaged position of the male, with indications that the proportions divorced, or married with spouse absent, will continue to increase.

As is often the case for research based on census data, it is extremely difficult to obtain the additional detailed cross-tabulations necessary for testing alternative explanations of observed relationships. In this instance, Price is unable to tell whether the similarity between white and Negro females, in contrast to the situation for males, is the consequence of a common discrimination shared by all females

or whether the Negro female simply has to work longer hours to achieve income parity with her white counterpart. Neither can the analysis tell us whether or not the apparent deterioration of the Negro male's economic position is due to a real widening of the gap in pay for the same work or to increasing differences in the distribution of white and non-whites within major occupational groups. It is worth noting that these and other questions could be answered on the basis of data which have already been collected, provided an efficient and inexpensive data retrieval system could be devised.

The analyses lack clarity and precision in those areas where it was not possible directly to control for age and sex differentials, and where changes or problems in enumeration procedures have affected the quality of census data. However, these problems are shared by all users of census data and are obviously as irritating to the author as to the reader. On the other hand, considering the importance of age-sex differentials for comparative studies of this kind, it is disappointing not to find a chapter dealing with the age-sex structure of white and non-white populations. Also disappointing is the absence of a chapter on fertility in a monograph concerned with changing characteristics of the Negro population. These omissions do not make this monograph any less important for the student of population or anyone interested in the future of the American Negro. Hopefully, the 1970 Census program will provide the opportunity to expand this important research in both its scope and depth.

*General Theory of Population*, by ALFRED SAUVY. Translated by CHRISTOPHE CAMPOS. New York: Basic Books, 1969. 551 pp. \$12.50.

T. R. BALAKRISHNAN  
The University of Western Ontario, Canada

Sauvy was the first director of the *Institut National d'Etudes Demographiques*, long a leading center for demographic research. However, while benefiting those demographers who know French, much of INED's outstanding work is lost on others due to lack of translations. It is fortunate that Sauvy's book, one of the finest in demography, is now available to English readers.

Demographers have often been accused of an overconcern with mundane statistical analysis

to the detriment of theoretical development. To some extent this feeling is shared by those within the profession itself. Sauvy's book, the product of a lifetime's research in demography, refutes this criticism and demonstrates abundantly the type of theory that already exists if one knows how to look intelligently behind data and the many possible lines of theoretical development.

Part One of the book ("The Economics of Growth") deals with population from the economic angle. Elegant models often used in economics are applied to study the interrelationships of economic variables to demographic variables. For example, the implications of technological progress, external trade, and social legislations for demographic trends are examined in detail. There is also an excellent section on cost of producing men and their value to society from the economic point of view. Part Two ("The Life of Populations") is more concerned with sociological issues and, due to measurement problems, is less amenable to application of models. Social factors of mortality, fertility, migration, and overpopulation in developed and underdeveloped countries are considered. The discussion of Malthus versus Marx is very instructive, and Sauvy shows that their ideas are still very much alive in the population philosophies of many nations.

One of the hallmarks of the book is the way in which Sauvy uses elegant, simple models to analyze the interrelationships between economic, social, and biological variables. The rather overworked concepts of the past which have fallen into disuse recently—such as maximum population, minimum population, and optimum population—become powerful tools in Sauvy's hands as heuristic devices in theory building. The reference to Malthusianism justifies a special mention. Nowhere has this reviewer seen such a succinct explication of Malthusianism and its implications. Malthusianism is a "state of mind, appearing in many circumstances, characterized by the fear of excess—faced with two quantities that need adjusting, it tends to lower the highest instead of boosting the lowest" (p. 391).

If you put on gloves that are too small, you will find that your fingers cannot reach the end of them. A malthusian attitude will whisper to you that they are too large and you may order an even smaller pair. But if you are a non-malthusian you will order a larger pair and may then get your fingers into the ends. In a period of crisis a Malthusian worries over the unemployed sectors, and one can hardly blame him; but he believes that they are the key sectors and in this he is quite wrong. The non-malthusian tries to stimulate the sectors already working at full-time in order to ease the unemployed sectors or bottle-

necks, by providing them with consumers (p. 395).

It is obvious that Sauvy is convinced of the many dangers of the Malthusian attitude for a nation's economy. The many protective barriers a Malthusian raises so that he does not have to divide the existing wealth with too many souls is in the long run harmful to national welfare. Some of Sauvey's ideas on Malthusianism are, of course, open to dispute; but many of them are indeed difficult to refute.

Sauvy sees capitalist systems paralyzed by fear of unemployment and overproduction derived from the Malthusian notion that better productivity decreases the need for manpower. For proponents of zero population growth in industrial countries, Sauvy's comments on the interrelationships between growth of population and economic progress should give food for thought. "A stationary or very slowly moving population does not benefit enough from the advantages of growth. There is no historical example of a stationary population having achieved economic progress. Theoretically it is not impossible, but in practice, in our period especially, it does not happen" (p. 184). The essential point is that there is what is known as an optimum rhythm of growth. On the relation between social class and population growth, Sauvy observes that, both in feudal and communist systems population growth is favored or is not an issue, while societies with large middle classes, such as capitalist societies, are essentially Malthusian—which he explains in terms of the power struggle that develops between social classes in different political systems. Regarding international migrations, highly restrictive immigration policies are seen by Sauvy as arising from a myopic lack of understanding of the effects of emigration and immigration on a nation's occupational and age structure. Sauvy's controversial ideas will definitely stimulate much thought on the social and demographic implications of different population policies.

A possible criticism of Sauvy's book is that the models are too simple, that examining the interrelationships of two or three variables at a time is unrealistic because demographic phenomena are the combined result of changes in many variables. However, this is one of the strengths of the book. What Sauvy has done is to attempt to construct middle-range theories rather than global theories such as the "theory of demographic transition"; and he has done a remarkable job. A broader synthesis of many of his theoretical generalizations is a task for

the future, and it will be a difficult one as it has in the other social sciences.

It is inevitable that such a collection of essays shows some lack of continuity. While using a wealth of empirical information to prove his theoretical generalizations, Sauvy keeps statistics to a minimum and relegates all mathematical derivations to footnotes. The result is an extremely readable book written in a style that is incisive, fresh, and never drab—unusual for a book in demography. The book is not a text, but will be valuable additional reading for advanced undergraduates and graduates. For professional demographers it is a "must."

*Society and Economic Growth: A Behavioral Perspective of Social Change*, by JOHN H. KUNKEL. New York: Oxford University Press, 1970. 368 pp. \$6.50.

KAROL KRÓTKI  
*University of Alberta, Canada*

The subtitle of this book is important because the book is not about economic growth. It is about the sticks and carrots, largely non-economic, with which the donkey of society can be manipulated. Moving in a new and important direction, Kunkel is clear about his purpose "to integrate men and behavior in the analysis of economic growth on the basis of recent work in sociology, psychology, and anthropology." Integration with economic literature is misleadingly suggested only on the publisher's blurb. The author makes no use of the hard-nosed economic works and limits himself to parts of the softer economics of Sir Arthur Lewis, Albert Hirschman, and Gunnar Myrdal. He even manages to discuss savings without bringing in the rate of interest (a feat indeed!). He ignores the importance of discounting in spite of the high rates of interest in underdeveloped countries; yet planning horizons of individuals and of national officials are short in most circumstances. The important contribution of the book is to suggest convincing modes, within a comprehensive theoretical framework, of manipulating behavior in the context of developmental needs. Purely economic models having been failures only too often, the book attempts to satisfy a long-felt non-economic need in an economic field.

Early in the book the author comes out firmly in favor of a behavioral model of man. Men can learn, and learn quickly. It is not necessary to wait for generations to change

the inner state of man, as the psychodynamic models would require. From the level of the individual, the reader is taken slowly and carefully through increasing levels of generalization. Case studies are offered in support of the hypothesis that a careful alteration of the components of social structure can and will initiate, shape, or extinguish behavior patterns relevant to economic development. Agents of change, once created, produce further change or countervailing forces, depending on whether the society is morphogenic or morphostatic. There are no further problems in a morphogenic society; suggestions are offered for morphostatic societies. In his least innovative treatment, Kunkel argues that the static world view can be changed by education, while social systems can be changed by innovation and deviation. Finally, the central thesis that "behaviour is replicated when contingencies remain the same, and is changed when contingencies are altered," is discussed in its implications for large-scale (national) and long-term planning and action.

There are severe lacunae, so severe that in fairness to the author they might be reduced to the reviewer's complaint that the author did not write another book. There is a discussion of the population problem, but it is peculiar in the sense that neither family planning nor birth control found their way into the index. The great controversy of the century started by Kingsley Davis in *Science* in 1967 has no place in this book. Simon Kuznets is in the references but not in the text. Richard Easterlin and his school of socio-economic and demographic integrators are not there at all, and literature of the political left is ignored (Baran and Frank), though compulsory development is specifically excluded from discussion. The complete disassociation from theories of individual development (though correct in the opinion of this reviewer), such as the Dabrowski concept of positive disintegration, may have given a bias to the argument. There is no discussion of race differences except for one oblique reference to genetic and physiological factors (p. 36), and no admission that there might be some. But then the author is only sharing in the 20th century's hang-up and participating in the conspiracy of silence comparable to the Victorian hang-up over sex. There is no word on pollution, and the opportunity has not been taken to point out to zero-population-growth advocates that economic growth contributes four times more to pollution than population growth. ZPG, presumably, remains in the pockets of big business in the same manner as defenders of free traffic in firearms must be paid by crime syndicates.

There is a strange whiff of 19th century *laissez-faire* in the author's suggestion that a successful development plan should fade out of existence. Abstracting from all these problems made for an easier book to write and to read, but the task of integration of theories still lies ahead.

The reader will, of course, go carefully and slowly through the Preface, the Table of Contents, and the Introduction. He will then do well to read pages 283-285 for a summary of the main argument, which will help in early understanding of this otherwise well-constructed book. Kunkel certainly follows the principles of telling the reader what he is going to tell him, particularly when he moves from one stage of the argument to the next. The text makes for interesting and lively reading. The often and, of necessity, abstract discussion is frequently broken by descents to the real world of living men. This reviewer will certainly recommend the book to his graduate students; but undergraduates will be able to benefit from it only if they had more than introductory sociology and at least some social psychology.

*Basic Data for Cities, Countries, and Regions*, by KINGSLEY DAVIS. Volume I of *World Urbanization, 1950-1970*. Population Monographs Series, No. 4. Berkeley, Calif.: University of California Institute of International Studies, 1969. 318 pp. Paperbound. \$3.00.

ROBERT C. ATCHLEY

*Scripps Foundation for Research in Population Problems, Miami University*

Davis has compiled an impressive array of data on world urbanization. For each country there are data on total population; total rural, urban, city, and town populations; growth rates; land area, population density, and degree of first-city primacy; and total population and growth rates for individual cities of 100,000 or more. All data are given for three points in time: 1950, 1960, and 1970. This brief summary does not accurately reflect the full diversity of the data in the book, but it gives a rough idea of its scope. The chief virtues of the book are that it covers all countries of the world and that the data are consistent in terms of definitions and points in time used.

In order to make the data as comparable as possible, it was necessary for Davis to do a considerable amount of estimating; as a result the text offers abundant illustrations of how population data can be estimated from

sometimes skimpy data. At the same time, however, the limitations of such estimates are also made clear.

About one-third of the book is text devoted entirely to documenting definitions, sources, and methods. Analysis of the data is reserved for volume two. Davis has done a very thorough-going job. One may not like the way a particular problem was dealt with, but at least it was recognized. Particularly useful, for example, are the codes which are included to give the reader an indication of the quality of each data element.

In the section on definitions, Davis has made a useful contribution with regard to the minimum size used to classify places as "urban." Rather than impose an arbitrary minimum city-size on all countries of the world, Davis argues for using the minimum employed within the country itself, on the grounds that what is urban in one culture may not be urban in another and that the local government is in a better position to know what is urban in its culture than is Davis. In this case, apparently the demographer's zeal for statistical symmetry was tempered by the sociologist's desire to avoid doing violence to cultural data. Demography could do with a great deal more effort toward creating this kind of perspective.

The most doubtful aspect of the book is the 1970 projections, particularly for the cities of around 100,000 population. In large cities, periodic fluctuations in growth are seldom big enough to distort the overall population change rate very much. In the smaller cities, however, events such as plant closings or land development can create a situation in which the simple extrapolation of growth rates can produce sizeable errors. For example, a comparison of Davis' 1970 projection for Hamilton, Ohio, against the preliminary census count for 1970 showed that he overestimated Hamilton's urbanized area population by around 40%. As a result of this type of problem, Davis' data for small cities taken individually would appear much less reliable than that for larger cities and nations as a whole, a fact that Davis himself mentions. Taken in perspective, however, this problem applies only to a very few of the many uses to which these data might be put.

It is obvious that this book will be an important reference source for those interested in world urbanization. Less obvious perhaps, is the fact that this book is a must for those considering cross-national research, in order to take urbanization into account in judging the adequacy of cross-national samples. For both purposes researchers will find this book invaluable.

*Build a Mill, Build a City, Build a School: Industrialization, Urbanization, and Education in Ciudad Guayana, Venezuela*, by NOEL F. MCGINN and RUSSELL G. DAVIS. Cambridge, Mass.: The MIT Press, 1969. 334 pp. \$12.50.

WESLEY W. CRAIG, JR.  
*Brigham Young University*

The Joint Center for Urban Studies of M.I.T. and Harvard provides another challenge for developers in urban planning in a continuing series of publications on a newly industrializing area in a relatively isolated region of Venezuela. This time the challenge is given to planners in education. How to convert a typically backward Latin American rural educational system into an efficient organization capable of providing trained technicians for a burgeoning new industrial complex—this question pinpoints the task for both educators and sociologists.

The book can be divided into three uneven sections of varying worth. The first is the expert sociologist in action, bringing to the scene a kit of methodological tools adequate to the description and analysis of students and their families. This is a sound methodological study and is badly needed in educational research in Latin America. Lacking, however, is an equally penetrating study of other actors in the educational system such as teachers, and their relationships to the community, the administration, other teachers, and even to students. No mention is made of teachers' unions or other pressure organizations. Nevertheless, of special worth is a succinct introductory overview of rural education in Latin America and a chapter on the family and school efficiency in which correlations are established with regard to the independent variable of school drop-outs.

In the second section the sociologist drops out of the picture and the professional business-administrator takes over. He gives his elaborate and detailed projections of what inputs are needed to meet the industrial sector's growing manpower needs. This section fails to incorporate any serious consideration of the cultural pitfalls inherent in the transference of technology (educational) from one type of cultural system to another.

The third section confirms this last-mentioned deficiency. It comprises a poor, defensive review of program implementation, in which the educational administrator seems to be writing and the sociologist has either gone on to other research activities or only pops into the office long enough to give an occasional comment. The result is a disappointing treatment of an

extremely significant problem—how to change social systems in different cultural settings. Not even the availability of significant funds for the job of educational development (which makes this case quite atypical for most Latin American countries) seems to alter the failure to confront these more basic issues.

The book's most singular fault is the apparent failure of the authors to recognize that their development of a new educational system was inextricably connected with the existent national system. Little comes through of the inter-relationships of the traditional system (represented by the National Ministry of Education) and the would-be innovators (represented by the C.V.G., an autonomous regional development organization, with money) and the foreign consultants with their foreign (to the national system) inputs. How these three entities did or did not get along (mostly the latter) is a crucial factor. Here is where the sociologist of organizations should have come to the fore, both in research and program implementation. Either his voice was muted or the appropriate skills were not available.

To a larger extent the limitations of the book reflect the traditional limitations in studies of educational systems by the U.S. sociologist. Questions of power relationships, cultural transference, and sectoral interests have received too little attention in the past. It is too bad that the tradition continues in this study.

*Studies of Latin American Societies*, by T. LYNN SMITH. Garden City, N. Y.: Doubleday and Co. (Anchor Books), 1970. 412 pp. Paperbound. \$1.95.

J. OSCAR ALERS  
Boston College

In a revealing study by Brown and Gil-martin, evidence is presented that between 1940 and 1965 American sociology became increasingly provincial, concentrating more narrowly on studies of the United States (*The American Sociologist*, Vol. 4, 1969, pp. 283-291). Fortunately, there are a number of distinguished American sociologists whose work has constituted a notable exception to this trend. Among these is T. Lynn Smith, who has devoted the better part of 40 years to the study of Latin America. That some of the best examples of his prolific output have been brought together in this inexpensive volume is

a boon to all students of comparative sociology.

The studies comprising this book are derived from Smith's numerous articles and books, as well as from previously unpublished conference lectures and testimony before Congress. They are organized into four sections. Part I, "The Development of the Sociological Study of Latin American Societies," addresses itself briefly to the origin and development of Latin American studies carried out by sociologists from the United States and Latin America. Part II, "The Population of Latin America," analyzes population growth, racial composition, fertility, and rural-urban migration. Part III, "Social Structure and Social Institutions," focuses on the rural community, class structure, and the family. Part IV, "Social Problems, Values, Change, and Development," encompassing about half the book, considers a variety of themes centering generally on agrarian reform and urbanization. Each of these four major sections is preceded by a brief introductory note. Substantively, the studies tend to concentrate on Smith's abiding interests in demography and rural sociology; geographically, they give special emphasis to Brazil and Colombia.

Within the confines of this brief review, two points may be singled out for specific comment. First, the value of comparative studies is illustrated by Smith's observation that such data as are available on racial differentials in fertility suggest, if anything, that Negroes may have a *lower* birth rate than whites (and other racial groups) in Latin America. This is quite in contrast to what would have been expected on the basis of studies confined to the United States. Second, noting the rise of a middle class in Latin America, Smith suggests that its members are recruited not so much by upward mobility from the lower classes as by skidding from the traditional elite. He then goes on to say that the pessimism to be encountered in Latin America is due to the downward mobility experienced over the generations by the former members of the elite. In fact, however, Smith provides no systematic evidence by means of which either the origins of the middle class or class differentials in pessimism may be assessed, and for the present his claims can therefore only be regarded as an interesting piece of speculation.

In these studies Smith actually devotes minimum attention either to issues of theory, beyond conceptualization and classification, or to questions of methodology, apart from noting the poor (but improving) quality of the demographic data. Also, despite his focus on rural life, little space is given to peasant movements; and regardless of his stress on population, vir-



tually nothing is said about birth control. The studies do exhibit an impressive amount of first-rate scholarship dedicated to the solution of some of the serious practical problems faced by Latin Americans, the most rapidly increasing major segment of humanity.

*Masses in Latin America*, edited by IRVING LOUIS HOROWITZ. New York: Oxford University Press, 1970. 608 pp. Clothbound, \$13.50. Paperbound, \$3.95.

WILLIAM L. FLINN  
*University of Wisconsin*

*Masses in Latin America* is a collection of articles from the series, "Studies of Comparative International Development." The 17 contributing authors include the "Who's Who" of Latin America social scientists: Barraclough, Stavenhagen, Frank, Germani, Flores, Furtado, and Camilo Torres. Horowitz's own introductory essay presents the thesis for the volume: "to understand the processes that go under the rubric of social development it is necessary to study masses as well as elites, nameless peasants and urban dwellers no less than military manipulator and political celebrities." Thus he stresses the need for the social scientist to study and understand Latin America from the standpoint of interaction among masses and elites. The central issue for development is seen as the conversion of a disorganized mass to organized class participants.

Following this introductory chapter, Horowitz subsumes the articles under three major sections: "Masses and Mobilization," "Masses and Urbanization," and "Masses and Politicization." Some issues treated include the relationship between masses and class consciousness, land reform, urban poverty, politicization of urban squatters, political violence, and revolutions of the left and right. Despite the well-written introduction, skillful editing, and an excellent index, the book suffers from many of the maladies of a collection of readings on a specific topic—namely, lack of continuity and the uneven quality of the contributions. One may wonder why more articles were not chosen or solicited from outside the series.

For the reader who is not familiar with the literature on Latin American masses, Horowitz's book provides a good view of the theory and research. For the informed student, however, the book has limited utility because much of the material is available elsewhere.

*Barrios in Arms: Revolution in Santo Domingo*, by JOSÉ A. MORENO. Pittsburgh, Pa.: University of Pittsburgh Press, 1970. 226 pp. \$8.95.

PAUL E. ZOPF, JR.  
*Guilford College*

This is an account of the events in the Dominican Republic between April 24, 1965, when a group of young army officers staged a revolt against the generals who had deposed President Juan Bosch seventeen months earlier, and September 3, when the rebels surrendered. It clarifies the sequence, nature, and significance of those events and places the participants in perspective. The book also is a concise analysis of several of the social components of revolution, including antecedent conditions, characteristics of rebels, functions and dysfunctions of conflict, degree of operability of existing groups and institutions, emergent social forms, leadership dynamics, and the role of ideology.

The author's methods were those of a sympathetic participant-observer. A native of Cuba, he lived and worked with the Dominican rebels during their five-month effort to reinstate Bosch and the 1963 Constitution. Moreno's use of participant observation to answer the question, "are the so-called rebels any different from the rest of their fellow citizens?" (p. 85), was adequate to the task although he tends to deprecate the technique. This has led him to apologize for the absence of a sophisticated statistical analysis and to rely upon "quasi-statistics," not all of which convey clear meaning (e.g., Table 1, p. 92). Nevertheless, the method was well used and its limits known and generally respected, except that Moreno may have depended too heavily upon long-term recall (p. 6) and placed too much confidence in the willingness of his subjects to be candid (p. 10). His judgment of the place of his study in sociology is realistic when he suggests: "If other such case studies are undertaken on other revolutions, it may be possible in the near future to produce comparative research on such a complex and important topic as the restructuring of a society undergoing a revolutionary process" (p. 13).

Beyond Chapter 1 on methods, the book is divided logically although not formally into two sections. The first (Chapters 2-5) describes the background of the revolution and the sociopolitical system constructed by the Trujillistas, the political organizations that existed just prior to the crisis and separately among rebels and loyalists during it, the paramilitary

organization of the rebels, and the variety of old and new groups that operated during the rebellion. This section makes the smaller contribution to the literature, partly because it is descriptive rather than analytical and partly because of its prolific detail and personal anecdotes (e.g., pp. 48-61 and 66-70). These chapters do convey the flavor of life during the revolution but might have been abridged.

In the second segment (Chapters 6-12), Moreno makes his basic contributions, also summarized in Chapter 13. Especially lucid is his differentiation of rebels and loyalists on the basis of the particular ideology and relatively high degree of alienation of the former (pp. 86-97 and Chapter 9), despite the fact that "the rebel group was, roughly speaking, a cross section of Dominican society" (p. 86). Moreno concludes that the rebels were not extreme leftists by any standard and holds: "The revolution 'was about' the Constitution" (p. 106), which, as perceived by the rebels and much of the population, guaranteed "a job, a minimal level of welfare, and an education" (p. 107), along with other "human rights." On these issues the official Communist Party and the U.S. Embassy appear to Moreno as equidistant left and right of center, respectively (pp. 130 and 131), and the U.S. involvement as especially myopic (pp. 125, 126-127). This positioning forces a classification of the rebels as "moderates," an unexpected but accurate designation in that they were attempting to generate a return to constitutionality and commitment to planned social change and were defined as "rebels" essentially because of the intransigence of the loyalists.

The author's typology of rebels and his appraisal of the relative importance of charisma and institutionalized authority in the development of leadership are closely tied to a discussion of the emergence of social organization designed to precipitate and cope with the crisis situation. His treatment of the growth of informal organizations in the commandos (groups of rebels) and their relationships with the larger society provides insights into types and sources of social solidarity, the nature of stratification, forces that establish and propagate behavioral norms, and some of the linkages between primary and other groups. Finally, Chapter 12, "Conflict and Conflict Resolution," illustrates well several of the relations between internal and external conflict, especially latent functions of conflict, conceptualized earlier by Lewis Coser.

The book has much utility for those interested in the characteristics of revolution and revolutionaries in general. It is indispensable

for those seeking an ordering and evaluation of the events of the Dominican rebellion. To take maximum advantage of the ordering the reader is well advised to begin with an examination of Appendix 1, "Chronology of Events."

*A New England Town: The First Hundred Years*, by KENNETH A. LOCKRIDGE. New York: W. W. Norton and Co., 1970. 208 pp. \$6.95.

WILLIAM A. SWAIN  
*Macalester College*

Lockridge has provided a record and interpretation of more than passing interest to sociologists, particularly those whose concern is with the mechanisms of community change. He seems to have been most rigorous in his search of the records, and "many techniques of social-science analysis—demography, mobility analyses, statistical breakdowns of the distribution of wealth—will inform [this] narrative." Several maps help clarify the dynamics of growth and change.

Lockridge's aim is to determine what it was like in "the world we have lost." He shows the formal and informal arrangements for town and church administration, the limited recognition of status-differences, the tenuous concern with the parent Massachusetts Bay Colony and its administration. Then, phase by phase, he discusses the changes in the community-style which occurred during the first century of the town's history (to approximately 1736). Probable parallelisms of structure and of change both with other New England towns and with towns in old England are noted, using both historians' town studies and anthropological concepts, particularly Eric Wolf's conception of closed corporate peasant communities.

Since the statistical materials are introduced only in the narrative, without systematic tabular format, and since Lockridge's concern is with the shifts and modifications of structural forms, there is little here for those who would wish to pursue mathematically-oriented reinterpretations of his data and/or comparisons with other statistical data; there is a bit more for those who would illustrate a structure-functional analysis.

The main sociological value I find in this account lies in its careful examination of possible factors accounting for the changing social arrangements of the town. While gently debunking the concept of the uniqueness of the American frontier community experience, Lockridge provides evidence for what F. S. Chapin chose

to emphasize as "the unplanned-for consequences of purposive actions." Lockridge is not content to see consequences as accidents of history, but seeks to account for them by close examination of the patterns of social action both within and without the town. Sociologists of community and of social change will find *A New England Town* a useful addition to their resource libraries.

*Town, Country and People*, edited by G. V. OSIPOV. London: Tavistock Publications (Distributed in the U.S.A. by Barnes and Noble, New York), 1969. 260 pp. \$8.75.

H. KENT GEIGER  
University of Wisconsin

As Director of the Concrete Sociological Research Section of the Institute of Philosophy, Soviet Academy of Sciences, and President of the Soviet Sociological Association, Osipov originally selected and edited (with the assistance of a small editorial board) the papers contained in this volume. Translations into English were prepared in Moscow by the Novosti Press Agency but checked against the original Russian versions and "considerably amended" by Maurice Hookham of the University of Leicester, who also has supplied a 26-page introduction. This is the second volume in a series appearing under the general title of *Studies in Soviet Society*; the first was *Industry and Labour in the U.S.S.R.*

Topics discussed in the 17 papers include trends in Soviet demographic and other vital statistics, the family and marriage, research methods—including problems of constructing models, social stratification, religion, social change in local communities, and rural sociology. There is a ten-page bibliography on rural sociology, urban sociological problems, and demography and social hygiene, but no index.

The Introduction by Hookham is a review of Soviet work on these topics and a reporting of symposia held in 1966 and 1967 in Leningrad and Sukhumi. It is uncritical, poorly organized, and inadequate for readers not familiar with Soviet concepts, presuppositions, and modes of discourse. It is on a par with the copy editing of the book, which suffers from cryptic passages, incomplete bibliographical referencing, spelling mistakes, and other signs of lack of care.

In spite of lackadaisical editing there are some things to learn both from the Introduction and the contributed articles. It is made quite clear, for example, that many Soviet sociol-

ogists have adopted a paradigm for the generation of research problems which involves the contrast of "natural" or "spontaneous" processes and structures with ideal—"optimal" or "rational"—ones. Rates of growth of different-sized towns and cities are compared with a town of "optimum size," "unorganized" patterns of migration are analyzed in relation to a "rational" distribution of the population over the various surplus labor and labor-shortage areas of the USSR, and spontaneous income and wage differentiation is contrasted with "a scientific system of distribution of income." Several of the articles discuss the problems of specifying the characteristics of such normative patterns.

The most interesting article was "The Development of Collectivist Attitudes Among Agricultural Workers" by O.I. Zotova and V.V. Novikov. Using responses given in questionnaires and interviews, the researchers found that industrial workers are more collectivist than are collective farmers. They show a more positive attitude to the achievements of others, are more ready to help others on their own initiative, more apt to demand an equal participation in work, and more willing to recognize personal responsibility for the activities of others. These authors also tell of a quasi-experimental procedure followed on several collective farms which was designed to raise the level of commitment to collectivism. When the usual highly specialized work teams (cattle-rearing, field cultivation, tractor operation, etc.) were replaced by "multi-purpose" work teams operating on a profit-and-loss basis, both morale and team productivity rose substantially.

This type of research finding, suggesting that the organization of daily work activities rather than the form of property ownership produces "real collectivists," conveys a sense of both the immense practical significance and the associated ideological vulnerability of Soviet concrete sociological research.

*Social Networks in Urban Situations: Analyses of Personal Relationships in Central African Towns*, edited by J. CLYDE MITCHELL. Manchester, England: Manchester University Press, (Distributed in U.S.A. by Humanities Press, New York), 1969. 378 pp. \$8.25.

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This volume concerns the use of social networks as an analytical technique in urban situations. It is a collection of seven papers by

different authors, three of which have previously been published, from a seminar for fieldworkers held in 1964-5 at the University College of Rhodesia and Nyasaland. Four of the papers were written during fieldwork and so are not necessarily the final analysis of the topics concerned. However, as Mitchell claims, these papers "show a considerable advance . . . on empirical data on social networks at present available, and for this reason alone, irrespective of the intrinsic interest of the analyses themselves . . . are worth publishing" (p. vi).

The book is part of the growing, but still relatively small, literature on social network *analysis* (which I abbreviate to s.n.a.) as a technique rather than as a metaphorical concept. It will interest those involved with, or wishing to acquire some knowledge of, s.n.a.—including Africanists, micro-sociologists, and social anthropologists. One paper by Kapferer will interest industrial sociologists. Readers acquainted with sociometry, small group studies, studies of communication, or, to a lesser extent, graph theory, and who have not previously delved into this type of analysis will not find themselves on altogether unfamiliar ground.

The papers vary in lucidity and in the detail and depth of analysis. As the editor admits, some discrepancies and differences of opinion between the papers are apparent. It is evident that there are problems of definition and conceptualisation to be sorted out, difficulties of data-gathering when dealing with larger-scale social worlds to be overcome, and analytical techniques to be refined. S.n.a. is still in the development stage, and the individual investigator must make up his own mind where he stands in regard to the choice of terminology and definition and which aspects of the analysis are likely to be most fruitful for future use.

The first two papers are methodological. Clyde Mitchell writes on the concept and use of social networks, while J. A. Barnes (who in 1954 introduced into British studies the use of s.n.a.) gives an exposition on the "principal formal properties of networks of social relations." These papers are a must for anyone seriously interested in the use of s.n.a. The remaining five papers use s.n.a. as an analytical technique applied to differing field studies. The application varies in detail and emphasis. Different papers will interest different readers. Two papers by A. L. Epstein cover social network and urban social organisation, and gossip-norms and social networks. P. D. Wheeldon studies the operation of voluntary associations among Coloureds in relation to personal networks in the political processes. B. Kapferer presents a detailed micro-analysis of the norms

and manipulation of relationships in a cell room on a Copperbelt mine. D. M. Boswell investigates the crisis of funeral and other arrangements following death, in relation to the mobilisation of the social network, while P. Harries-Jones is concerned with homeboy ties in political organisation in a Copperbelt township. He also includes a brief appendix discussing certain of A. C. Mayer's conceptions. Not only do these five papers reflect the individual concepts of s.n.a. held by the investigators, but they offer insights into the different aspects of social life investigated. I was particularly interested in the insights gained by Kapferer in his study of the internal system of the cell room (which links back via Homans to Elton Mayo and his associates), and in Wheeldon's study of an aspect of a Coloured community in Zambia (which suggests rather different interpretations of certain phenomena from those given by Dickie-Clark in his study of marginality among Coloureds in Durban).

At this stage in the development of s.n.a. one cannot expect a consistent, polished whole. The differences between authors, both in regard to application of the technique and to some extent in conceptualisation, will serve a useful purpose in sparking off argument and further work.

*Tradition and Transition in East Africa: Studies of the Tribal Element in the Modern Era*, edited by P. H. GULLIVER. Berkeley, Calif.: University of California Press, 1969. 378 pp. \$7.00.

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This book, a product of a seminar at the School of African and Oriental Studies, University of London, explores in thirteen papers the interrelationship of tribalism and modernization in the multi-tribal new nations of Kenya, Uganda, and Tanzania. A final paper on Somalia is included, presumably to illuminate their situation by comparison with a neighboring unit-tribal nation. In each case, however, tribalism has been both a unifying and particularizing factor.

The book is presented in two parts. Part One is a series of general studies in which tribalism is first likened to European nationalism (W. J. Argyle), and then discussed in relation to politics (George Bennett), social and economic institutions (Tom Mboya), national language (W. H. Whitely), law (Eugene Cotran), and education (J. W. Tylor). Part Two consists of regional and case studies of tribal-

ism: the Gisu (J. S. La Fontaine); Eastern Uganda (Michael Twaddle); the Chagga (Kathleen M. Stahl); the Arusha and Masai (P. H. Gulliver); in various special situations such as a tribal revolt (Kirsten Alnaes); in Kampala (D. J. Parkin); within a railroad workers' union (R. D. Grillo); and in a traditional kingdom (H. F. Morris). Alnaes' study of the Konzo revolt is methodologically interesting since she traces its history from a sequential analysis of the songs it inspired.

Much consideration is given by several of the authors to the meaning of "tribe." In his Introduction, Gulliver reviews their conclusions as well as those of other scholars to produce a working definition of "tribe" as a group of people who share both a territory and a distinctive culture. This group is not fixed; it can be reduced to constituent groups, themselves defined sometimes as "tribes" and sometimes as "sub-tribes," or expanded to larger groups, likewise dually defined to serve the occasion as "tribes" or "super-tribes."

The mutability of the tribal unit is a recurrent theme in many of the essays. While tribes were in existence in East Africa when the European (first the missionary, then the administrator) arrived, they were relatively unimportant, people generally considering themselves members of a community and of kin groups, rather than of a tribe. The outsider, partly due to a misreading of the situation and partly for administrative convenience—and also, according to Argyle, because of the "personal attachment of some European missionaries, administrators and anthropologists to 'their' tribes" (p. 51)—delineated (sometimes creating) and strengthened various tribes.

The influence of the outsider, not only on tribalism but on many aspects of these emerging nations, is the most fascinating message of the book. Space restrictions dictate limiting examples to the fate of Swahili in the different countries. In Tanganyika the Germans had made use of Swahili agents as local administrators throughout the territory. These agents dispensed their language along with their authority; its use was widespread when the new nation declared Swahili to be its *lingua franca* (Whitely, p. 110). Swahili is the indigenous language of coastal Kenyans, as it is of those of Tanzania. But the version of Swahili which spread inland was not that of the tribal speakers (Ki-Swahili), but rather that spoken by European settlers (Ki-Settla) (p. 113). This version was too limited for adequate communication and too symbolic of foreign domination to be acceptable to other tribes. Since publication of this volume, however, the Kenya government,

on May 9, 1970, decreed that Swahili is to become the national language, effective Jan. 1, 1972. In Uganda, Christian missionaries, established early in the colonial period, deliberately discouraged—through their near monopoly of education—the spread of Swahili as the language of the rival Islamic faith.

Other differences to be found among the three nations are due primarily to differences in their tribal make-up. Again, a single example, the relation of tribes to nationalism must suffice. In both Kenya and Uganda the national capital is located in the area of a dominant tribe. In Kenya this facilitated nationalism since the local Kikuyu were eager to further a nation in which they hoped to be the leading tribe. In Uganda this obstructed national unity as the local Ganda pressured for the independence of their separate state. In Tanzania, where the largest tribe, the Sukuma, is located remote from the capital, no one tribe exerted special influence. In fact, Bennett finds that, as a result of two concurrent politico-economic forces, "tribalism became subsumed in nationalism" (p. 81). The first of these forces, agricultural producers' cooperative societies, tended increasingly to cut across tribal boundaries; the second, a national political party, spread pan-tribally among people united in their opposition to oppressive government agricultural and resettlement schemes.

This Tanzanian experience testifies to the importance of agriculture and its modernization in the history of emerging nationalism in East Africa, and in the lives of the East African people, roughly 90% of whom still earn their living by plant or animal husbandry. That this predominance of agriculture is only meagerly reflected in these essays is, in my judgment, the major fault of this book. President Nyerere of Tanzania evidently found similar fault with his country's educational system as inherited from the British. In his "Statement of Education for Self Reliance" of 1967, he called for emphasis on agricultural education, both to aid economic development and to increase the common interests and national identity of the educated and the peasantry (Tyler, p. 173).

*Social Change in a Hostile Environment: The Crusaders' Kingdom of Jerusalem*, by AHARON BEN-AMI. Princeton, N. J.: Princeton University Press, 1969. 193 pp. \$7.50.

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This book is a lucid and fairly effective plea for a closer rapprochement between sociology

and history. Or at least it is a convincing proof that sociologists can render a bit more intelligible the incoherent diversity often inherent in historical events and processes. Ben-Ami treats the confrontation of the Crusaders' Kingdom of Jerusalem with Islamic societies of the 12th century as an instance of a transplanted society in a hostile environment. He handles the case study so that it almost takes the form of a controlled experiment: a causal connection is established between a changing independent variable (inter-societal system) and a dependent variable (social structure) while other factors are held constant.

The book is conveniently organized: to separate fact from analysis, historical chapters are always followed by analytical and theoretical elaborations. For the uninitiated reader in crusade history (e.g., this reviewer), the historical chapters are slightly tedious. The analytical chapters, however, have much to offer, particularly to students of social change and political modernization. The author is aware of the theoretical limitations of a case study and cautions the reader not to draw broad generalizations regarding general theories of social change. As he says, "A case study cannot establish or disprove a theory. It can only contribute empirical generalizations suggesting some theoretical import" (p. 178). In fact, three theoretical questions are considered by the author (p. 16): What kind of change occurs when a social system is transplanted into a new political environment? What happens to traditional institutions under such conditions when adaptational tendencies are incongruent with the new social order? How does a given type of international system act and react to a newly introduced factor which disrupts its balance?

Ben-Ami begins with a brief survey of some of the leading theorists of social change, from Ibn Khaldun to Parsons via the classic tradition. In Chapter 3, some of the problems associated with transplantation and interlinkage are analyzed in terms of "institutional lags" and "innovative functions." The lags result from conservative application of past culture to a new international setting, and innovative functions represent adaptational efforts by a social movement to overcome resistance to institutional change. Three such innovative tendencies, all marked by sharp departures from the existing social order, are discussed: (1) efforts to attract Christian Syrians of all sects to settle in the Kingdom; (2) commercial and maritime development; and (3) the development of frontier communities of religio-military orders. In Chapter 5, analysis shifts to a consideration of the

three basic induced functions of the international system: (1) the mutual reinforcement of a spirit of holy wars, crusades, and *jihads*; (2) a growing tendency to monarchical centralization; and (3) the appearance of charismatic movements.

On the whole, Ben-Ami does a creditable job. His conceptual scheme, though not entirely novel, is helpful, but its application is occasionally marred by a few gaps. For example, he speaks of a "dialectical connection between international relations and stratification" (p. 180), but does not convincingly demonstrate how this connection manifests itself or what form it takes. He also fails to answer the more relevant question regarding the conditions under which the fusion of roles (in this instance, the knight and monk roles in the feudal Kingdom of Jerusalem) became necessary and functional. We are, for example, repeatedly told that when a knight is converted from a "vassal" to a "brother" the basis of action shifts from "self-seeking cooperation to self-sacrificing effort"; but we are not told under what conditions this fusion of religio-military orders takes place. This has important implications for the conceptual relevance of role fusion as a form of social change, and certainly more than just a passing hint would have been welcome. Since the religio-military orders such as the Templars and Hospitallers were the most far-reaching instruments of social change in the Kingdom of Jerusalem, the analysis would certainly have benefited from a more elaborate discussion of the social organization, internal structure, recruitment, and socialization processes of the orders. Finally, the author's inference that the Sufi orders and the religio-military orders of Templars and Hospitallers share a lot in common (p. 185) is questionable. A recent study (S. M. Stern, "The Constitution of the Islamic City" in A. H. Hourani and S. M. Stern, eds., *The Islamic City*) provides historical evidence which suggests that neither in form nor in function could the Sufi orders have resembled the orders created by the Crusaders.

The reader cannot but be impressed by the striking parallels between the Crusaders' Kingdom of Jerusalem and the present state of Israel. In many instances the same perspective—of institutional lags and induced functions, the diffusion of cultural elements, and the internally functional but externally dysfunctional institutions—may be appropriately employed to assess the viability of a "transplant" in a hostile environment. Just as then, the "ill-advised attack on Damascus only encouraged more Islamic jihads, which further aided Nur-ad-Din in the unification of Syria" (p. 131); the contemporary

analogies, I am afraid, are too apparent to be overlooked.

*Rural Sociology in India*, by A. R. DESAI.  
Fourth Edition. Bombay, India: Popular Prakashan, 1969. 986 pp. Rs. 60.

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This volume is a commentary on contemporary India, its historic development, and its problems for future change. Desai, author and editor, has compiled information from sources as diverse as the *Journal of American Folklore* and the *Fourth International No. 12*, while making use of articles drawn from a wide range of social science studies and government documents. He states simply that the design of social order in changing India "requires to be comprehended" (page vi).

Part I, "Introduction to Rural Sociology in India," provides an orientation to India in terms largely of the concepts and categories used in American rural sociology. Part II, "Readings in Rural Sociology in India," elaborates the analysis and commentary on the past, the present, and the possible future of rural India. Running through Part II are a fundamental theme and a cogent question. The theme is the transition from noncommercial agrarian regional societies of villages united under feudal governments, to a commercial nation which is partially industrialized and urbanized. The question is whether it is possible—and, if possible, necessary or desirable—for an Asiatic nation to modernize with the same developments in social structure which have characterized the modernization of European and American nations. Will, then, ultimate developments in India be toward a predominance of private capitalism or of state capitalism (Capitalism or Socialism)?

Desai's focus is on the relationship of village community structures to the national economy and government, and their adjustment to programs of modernization which are national in scope. A pervading impression is conveyed that modernization programs have failed, at least in part, since they have not put benefits, including economic and political power, in the hands of persons of low status in village communities. Land reform, cooperatives, administration, revival of a version of the traditional village *panchayat*, community development programs, and social movements seem to have had little success.

Current status and power structures, however,

are regarded as reflecting the inroads of British colonialism on the ancient non-commercial Indian villages with communal ownership of land, with strong family ties, with status and caste systems built to a great extent on occupational differences, and with the overseeing feudal governments. Into this setting the British introduced concepts of individual property, money, money-lending, absentee landlordism and rental systems which provided revenue for the British, credit and commercialism, wage payments for artisan's skill, and laws and a judiciary which supplanted those of the villages. On the basis of new criteria of status, a class system began developing in a manner which sometimes coincided with caste hierarchy and sometimes did not; in addition, villages were deprived of their traditional isolation and were brought into contact with other villages and the outside world to an extent previously unknown. It is in the context of this contemporary village community that attempts are being made to effect modernization programs. The above comments about village communities are not made to minimize the significance of tribal people, who, outside the institutional structures of villages and cities, also have not benefitted from programs of modernization.

Even though impressed by Desai's breadth of perspective and concern, I am intrigued by the implications of his statement that the design of the changing social order "requires to be comprehended," the repeated implication and statement that nationwide programs for modernization have failed, and the concern expressed for theories of agrarian development. The combination suggests to me an innuendo to the effect that the programs of the Congress Party have produced no favorable results according to the criteria used here, so that perhaps something different should be tried. If my interpretation is correct, the perspective can be understood on the basis of the nation's long experience in a colonial system, its geographic location and position among nations which make having a "mixed economy" economically advantageous and politically judicious, strong feelings of nationalism among leaders, and intensity of motivation to modernize, at least up to a point. Under these circumstances one can but wonder about the extent to which this perspective on the future of rural India is influenced by fact and fantasy about an ancient village community, mingled with the realities of investment and accumulation of capital, dreams of equal opportunity and the elimination of exploitation, and the anticipation of reaching a "take-off" period of self-sustaining economic growth.

*Land and Social Change in East Nepal: A Study of Hindu-Tribal Relations*, by LIONEL CAPLAN. Berkeley, Calif. University of California Press, 1970. 224 pp. \$6.00.

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The research on which this book is based was conducted in 1964-65 on the theme of Hindu dominance and alienation of the native population of Limbu tribesmen in East Nepal, a Himalayan kingdom. Caplan has not chosen a fresh ground, for the theme has been considered by many earlier students of tribal problems, e.g., S. C. Roy, Verrier Elwin, Bose, Furer-Haimendorf, and Ghurye. The author suggests that mutual transmission of cultural elements between the native and the alien groups does not tell the whole story of the interactions between the two groups. He calls to the reader's attention the dimension of change in the ownership of land occupied by the "so called" alien groups, which has threatened tribal economy and led to cultural annihilation.

To support his arguments the author traces the history of the last two centuries and endeavors to demonstrate that the history of east Nepal is shadowed with a perpetual conflict between the Hindus, particularly Brahmans, and the Limbu tribesmen. In seven chapters Caplan provides a brief history of the region and discusses the distribution of land and population, government land policy and its impact on the Limbus, the ability of Brahmans to manipulate resources, the role of cleavages, factions and *panchayat*, and the feeble attempts and future of the Limbu political movement.

The particular value of this book lies in the penetrating and highly sophisticated analysis of counterpoise between different segments of east Nepalese society. Though Caplan suggests that there exists an ongoing confrontation and "alienation of the natives," there are several instances which support the theory that there has been mutual exchange in economic and political spheres in spite of cultural distinctions among the groups. It is interesting to note that, while the rest of the groups, excluding Limbus, are influenced by *sanscritization*, Caplan thinks that the Limbus have resisted emulative tendencies only for one reason: vested economic interest in their traditional *kipat* land. The Limbus are a majority tribal group in a region infused with a great degree of identity and social solidarity, while other tribes are not, and perhaps they are aware that Brahminical values are opposed to their own. Consciousness of this vulnerability has again reinforced their own values.

The author does not go into the issue of the opposing contentions or whether the tribals should be saved from the outside or alien cultures or should be completely assimilated, but he clearly demonstrates how economic exploitation of a native people by an alien group leads to the creation of an unbridgeable gap between impecunious clients and powerful impostors.

What is valuable about the book is not so much the author's main thesis and the material he has gathered to substantiate it, but his analysis of the problem, and particularly his incisive historical approach to the problem. The situation he describes is not uncommon to the rest of South Asia, and it is in this sense that the study contributes to a framework that can be tested in other areas.

The book is highly readable, and offers much information with economy and insight. However, professionals interested in Nepalese society are better referred to recent works by professors Furer-Haimendorf and Hitchcock.

*Dimensions of Urban Social Structure: The Social Areas of Melbourne, Australia*, by F. LANCASTER JONES. Buffalo, N. Y.: University of Toronto Press, 1969. 149 pp. \$8.00.

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In this statistical study of Melbourne, F. Lancaster Jones, a sociologist at the National University, uses census tract data to map the social structure of the city in 1961, building on the work of Shevsky and his associates. A major aim of the study was to test the usefulness of socio-economic status, household composition, and ethnicity in analysing factors affecting the social differentiation and stratification of the city and the "processes by which socially differentiated population groups come to be residentially segregated." Jones also hoped to provide a basis for urban planning and to identify determinants of social problems, political behavior, and group membership. He bases his use of ecological data for the analysis of the structure and process of social interaction on the work of Lazarsfeld, Kendall, and Menzel on the relation between individual and collective properties. He sees residential areas as "salient environments within which many important relationships occur," which can then be studied with individual data.

The main findings are these: (1) There is no relation between the socio-economic status of an area and its degree of familism. Familism—"relatively many private houses, young children, high fertility and dependency ratios, low work



force participation, few old people and unmarried adults"—resembles one of the stages of family life in Rossi's *Why Families Move*, cited by Jones. This finding might have been altered if length of residence in the census area could have been used to clarify socio-economic mobility in these areas. (2) The areas vary considerably in ethnic and religious characteristics (which Jones combines), but exhibit three distinct distributions—Southern European and recent immigrant, Northwestern European, and large proportion Jewish. (3) Ethnicity and familism are related inversely. Immigrants settle disproportionately in transitional zones with few children. (Does he mean that there are many immigrant children who get averaged out with the few non-immigrant children in an area analysis?) (4) The negative relationship between ethnicity and socio-economic status is stronger than the others.

The study is a valuable effort to exploit census information in a new area and link it theoretically to major work in population research. The nature of the data imposes severe restrictions on the analysis, as the author recognizes. The census units are not social entities—they have no names, no functional identity. Are they, in fact, "salient environments"? Lack of systematic data for another time-period limits process analysis and adds to the static, taxonomic quality of the book. It calls to mind de Jouvenel's "tactile agnosia" where one can have all the information on, say, a pencil, and not recognize it. Melbourne, where this reviewer spent twenty years, would have remained *terra incognita* but for the use of proper names. History from other sources is used to illustrate but not to analyse. For example, confinement of Jewish refugees from Europe to their residential areas after curfew during World War II affected their settlement patterns, a fact Jones ignores.

Now that this useful research is reported, the book remains to be written by someone who can say it with figures. Finding the essential ones now is more tedious a task than a field study of ethnicity in Arlington National Cemetery.

*Principles of Inductive Rural Sociology*, by T. LYNN SMITH and PAUL E. ZOPF, JR. Philadelphia, Pa.: F. A. Davis Co., 1970. 558 pp. \$9.95.

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Because this book is written for college students and persons planning agricultural policies

throughout the world, the authors' general objective "has been to give due consideration to all scientific approaches that bear upon the field and to assemble from the most reliable sources pertinent facts, significant tested hypotheses, and inductively derived theoretical generalizations relative to the social relations among those who make up rural populations" (Preface). A massive amount of information is brought together in a logical and coherent form; and, while hypotheses and generalizations are elaborated, it is difficult to tell whether the generalizations govern the selection of the data reported or are derivations from the many empirical studies available. Nevertheless, it is a significant compilation of material in rural sociology and has useful applications as well in the general field of sociology.

The book begins with a superficial introduction to the scientific study of rural society, but moves to a concise history of rural sociology and a treatment of new research interests developed since World War II, e.g., health and medical services, diffusion of agricultural practices, aging and retirement, suburbanization, and rural society in other lands. Various characteristics of rural populations are presented which show the continued numerical dominance of rural people in spite of the attention accorded urbanization and industrialization in sociological literature. Though the referencing is generally adequate, there are some gaps. For example, in discussing the effect of rhythmic rural work cycles, such statements as "such a distribution of working periods exerts a profound influence upon the personalities of the individuals concerned" (p. 25) are left without supporting data. The authors present a typical analysis of the occupational structure of the rural community (pp. 58-61); while accurate as far as it goes, a more comprehensive measure of the impact of the rural enterprise in a complex social system needs to include occupations directly related to agriculture, such as processing plants for farm products, wholesale and retail distribution outlets, and farm input production (e.g., commercial fertilizers and farm implements).

The material on social ecology, organization, and structure in rural society represents both the major contribution of the book and the most useful insights about rural society. Throughout the book the contrasts between the family farm and the large estate holdings are used in analyzing forms of settlement, land tenure, social differentiation, and rural institutions. The authors have updated family farm concepts and definitions (p. 186) and used the data from comparative studies of farm holdings

to support their conclusions on many of the topics. For example, trends in social stratification are related to the substantial decrease in the extent to which land ownership is the basis for wealth, power, and social prestige, the general disrepute of *latifundia* in Latin America, the impact of urbanization on the landed estate and the two-class social stratification system, the mechanization of agriculture, and the voices of agrarian reform (pp. 284-288). A strong case is made for the social benefits of the family farm as an economic unit. What may not be justified, at least on the basis of the data presented, is the optimistic judgment made about the future of the family farm: "During the last century, various factors have been chipping away at the entrenched position of the large landed estate; and in the decades ahead, the forces promoting a transition to family sized farms are likely to be even more potent than they are at present" (p. 205). Whether or not changes in customs and tradition, mechanization, increases in levels of education and living, and all the other related variables will act in concert to produce the results predicted is still a matter of conjecture.

In the area of rural institutions, data are made current; but trends and interpretations of those data follow closely the conclusions reached much earlier by Taylor, Kolb, deS. Brunner, and Smith himself in his earlier works. This appears to suggest that there is considerable consistency in the developmental changes taking place in rural institutions. However, it might also indicate a need for new questions and new data in institutional analysis itself. Within the institution of marriage, the authors treat romantic love under the topic of "marriage by capture," certainly a unique suggestion. One of the best analyses is found in the area of political institutions and local government, where the interrelationships between large land holdings, illiteracy, and debility of local government are explored.

The final material in the book deals with social processes within rural society. Competition, cooperation, conflict, homogenization, accommodation, and other processes are discussed in reference to economic, racial, institutional, town-country, and social class relationships. The work concludes with observations about the future of agricultural people throughout the world. The dominant theme is "what is perhaps the single most important feature of rural areas everywhere—sociocultural change" (p. 491). In order to direct this change to the well-being of rural people, the authors state that "in the last analysis, only comprehensive

development of sociological fact and theory . . . can form the basis upon which adequate state and national policies can be established" (p. 493). A comprehensive bibliography of over 600 books and articles adds to the work's usefulness as a reference volume.

*Automation and Behaviour: A Social Psychological Study*, by J. K. CHADWICK-JONES.  
New York: Wiley-Interscience, 1969. 168 pp.  
\$8.95.

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This book reports a study, using 1956 and 1967 data, of the attitudes and behavior of a select group of English steel workers, many of whom voluntarily shifted in the interim to a new steel mill which made use of the latest in automated equipment. Five of the fourteen chapters describe working conditions and social organization in the pre-automated plant. The remaining chapters analyze the reaction of the workers to their new plant facilities.

Open-ended interviews and observational methods were used to gather data for stratified samples of 47 operators before the move to the new plant and 51 workers in the post-automated phase of the study. Data analysis consisted primarily of percentage distributions based on responses to questions dealing with variables such as degree of boredom, job satisfaction, and degree of informal worker contacts outside the factory. Chadwick-Jones was able to establish that the workers experienced a shift in attitudes as a result of the new plant setting. Unfortunately, no theory or hypotheses were proposed to give direction to the study. Thus, the research could best be characterized as a case study in technological change.

Social organization in the pre-automated factory setting was structured around work teams seeming to have high morale, acceptance of the incentive system for personal advancement, and understanding of the work process. These factors were diminished in the automated factory with an apparent rise in boredom and alienation. The author is reluctant to speak of the value issue related to sacrificing one well-defined set of human work relationships in favor of a seemingly more shallow set of interpersonal work relations. His study demonstrates all too well the lip service paid to the view that technology serves man. At one point the statement is made that:

Interviewees' opinions revealed that their established expectations and behavioural standards were inappropriate to the present situation. Less

commitment was required to team exchanges and relationships but less support was afforded, and the latter might have been especially useful to the individual in conditions of large changes in the job environment. Following on from this assumption it could be questioned whether the technical reorganization should have been permitted to disrupt existing group behaviour. This raises the general question of whether such forms could be included for design forecasting in explicit and measurable terms. If this could be done a significant contribution to planning might be the formulation of an optimal structure for the primary group (p. 128).

However, the author is reluctant to follow through and speak to the theoretical and practical issues involved in minimizing personal and social costs of technological reorganization.

This book would be useful for supplementary reading in sociology courses with an industrial and organizational emphasis, though it is inconceivable that many copies will sell at the price set.

*Automation, Alienation, and Anomie*, edited by  
SIMON MARCSON. New York: Harper and  
Row, 1970. 479 pp. Paperbound. \$6.95.

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The flood of literature on the effects of automation has been reduced considerably since the scare of the 1950's, when social and economic analysts were forecasting massive technologically induced unemployment and dehumanized "automatic" workplaces. From the fields of sociology, social psychology, economics, and business administration, Marcson has assembled a reader containing 28 relatively sober selections on the nature and consequences of automation, calling attention to a subject often neglected in a prospering economy and documenting the moot nature of assertions regarding the dire economic and social consequences predicted for the "new technology."

The book's title, which suggests a focus on the social-psychological effects of automated technology, misrepresents its content. The readings are arranged under six general topics, only the last of which corresponds to the implied theme. In the first five sections various important topics receive attention: the nature and development of automated technology; the impact of automation on manual workers, office employees, and the organization; the implications of automation for collective bargaining and white-collar unionism; and the relationships between automated technology, employment levels, and manpower planning.

Only one selection (Faunce, "Automation and the Division of Labor") links automation and

alienation, and it does not appear in the concluding section. This may be due to the emphasis of the final section on the thesis that technological development intensifies alienation. By contrast, "Automation and the Division of Labor," which contains distinctions among craft, mechanized, and automated technologies, suggests that, while mechanization increases alienation, automation reduces it. Two segments from Robert Blauner's *Alienation and Freedom* appear, but the part of his study demonstrating that workers in automation production systems are less alienated than those under mass production technology is omitted. A relationship between technology and alienation is not difficult to find in the literature; but it is mass production technology, not automation, which appears to be the culprit. Marcson is able to establish alienation as a by-product of "automation" by failing to distinguish between mechanization and automation, a differentiation he makes in other places in the book, as do several of the authors. It is unfortunate that a book on automation and alienation, when it touches on the subject, focusses instead on mass production technology and alienation, especially when some additional literature on this question exists.

At the outset Marcson offers a reason why this volume could not be entirely devoted to the social psychological consequences of automation: those most likely to investigate this area (i.e., sociologists, psychologists, and social psychologists) have left the field open to economists, who have provided a wealth of literature on the impact of automation on such matters as unemployment, management innovations, and occupational changes. As a result, relationships between automation and its social psychological correlates have not been widely researched. Nevertheless, Marcson's book will be an asset to anyone interested in the nature of automation and its diverse consequences; it clearly fills a gap in the literature which has been widening.

*Organizational Behavior: Theory and Application*, by WILLIAM FOOTE WHYTE. Homewood, Ill.: Richard D. Irwin, 1969. 807 pp. College price, \$10.50.

*Organizational Behaviour*, by JOE KELLY. Homewood, Ill.: Richard D. Irwin, 1969. 666 pp. College price, \$9.95.

J. REX ENOCH  
*Memphis State University*

Despite the amount of material published in the social sciences today, there are relatively

few instances of books of the same title. To have two books of the same name put out by the same publisher in the same year is even more unusual. But the similarity of these two books scarcely extends past their titles. Since both books are concerned with the same general area of investigation, they of necessity make reference to some of the same significant sources in their area of analysis, but the ways they use the information vary considerably. Kelly, in concentrating on theoretically significant empirical research relevant to an understanding of organizational behavior, succeeds in integrating this information with his own research and in developing meaningful empirically based generalizations. Whyte mentions the work of others when it relates to his own, since the bulk of his book reports case studies from his own research. For this reason (as well as others to be mentioned), Kelly's book more nearly accomplishes the goal of assembling the knowledge available in this area so that the reader can grasp where we are, where the gaps are, and what we need to know.

Kelly, an organizational psychologist, has skillfully managed to introduce policy concerns without the book's disintegrating into a treatise on "how to have a happy organization." The audience for which much of this is intended is indicated in the section of the book entitled "Applications," which deals with "Executive Recruitment and Selection," "Sensitivity Training," and "Management Education. . . ." The value of this for sociologists depends on particular orientations and interests.

For most sociologists, the significant contributions of this book lie in the first six or eight chapters. The first three chapters trace the movement from industrial psychology to human relations to organizational behaviour. The object of his approach is an understanding of "man-in-organizations-in-a-social-environment." He states: "Thus this field of inquiry about business behavior, which started as industrial psychology with the emphasis on improving the efficiency of the operator, which then in turn gave way to human relations within which the need to provide a *satisfactory* social environment became supreme, is now largely concerned with developing the optimal organization where the efficient can be reconciled with the human" (p. 23).

Kelly emphasizes the necessity of an interdisciplinary approach to the study of organizational behaviour, and develops a "Framework for the Study of Organizational Behaviour" in Chapter 6 which brings together materials developed in the preceding chapters. This conceptual framework is consistent with traditional

concerns of organizational sociologists (e.g., roles, small group behavior, bureaucratic structures, formal and informal organization). Also included at the end of each chapter is an analytical glossary of major terms introduced within that chapter.

The book is comprehensive, readable, and academically sound. Its relevance for social scientists in general extends its applicability. For an industrial sociologist, its weakness lies in its omission of any real concern with labor-management relations. The author contends that unions are losing support in both the United Kingdom and the United States "mainly because of the rise of meritocracy but also because of the trade unions' abuse of their powers" (p. 126). Given the orientation of the book, it is understandable why the author does not devote extensive time to an analysis of the labor movement, but to make such a broad generalization and then virtually ignore the topic weakens the author's recognition of the need to see the "organization-in-the-social-environment."

On the other hand, Whyte, well-known to industrial sociologists, devotes a significant segment of his rather extensive book to the analysis of labor organizations. This section suffers from the same problems which plague the rest of the book—i.e., a too-detailed account of numerous case studies concealing rather than enhancing the point being made.

Whyte's contributions to the field of sociology are not at issue here; the question is whether this book makes any significant contribution to the analysis of organizational behavior? The book is a revision of his *Men at Work* (1961). Nineteen chapters are completely new and seven others are substantively new or major revisions of the earlier work, leaving only seven of the thirty-three chapters in this latest volume that appear to be similar to chapters in the original work. However, one who has read very much of Whyte's work would probably feel that he has read some of this before. Whyte not only continues to draw on his classic studies of *Street Corner Society* and the restaurant industry, but his conceptual framework à la Homans is well-known (and is the same as that used in *Men at Work*). His style makes for rather laborious reading because of the extensive description of particular situations and personalities, most of which were investigated by Whyte himself. Whyte suggests, as the first characteristic of a useful theoretical system, the importance of parsimony (p. 87); it is interesting and unfortunate that the elaboration of his "parsimonious" system in this

book is so tedious that it often takes away from what he is trying to emphasize.

On the positive side, the book is most comprehensive and covers a wider range of topics of concern to sociologists than does Kelly's. The first three chapters are cogent and concise treatments of historical, theoretical, and methodological concerns. Whyte gives considerable attention to the dynamics of industrial organization—i.e., its adaptation to its social and economic environment and changes within the organizational structure. As reference material in these and other areas, the book could be very useful, though its utility as a college textbook is questionable. For this purpose Kelly is more applicable despite its limitations. Kelly's limitations are a result of his academic orientation, while Whyte's are a result of his style.

*Organizations*, edited by JOSEPH A. LITTERER. Second Edition. New York: John Wiley and Sons, 1969. Volume I: *Structure and Behavior*. 498 pp. Volume II: *Systems, Control and Adaptation*. 417 pp. Clothbound, \$9.95 each. Paperbound, \$4.95 each.

CHARLES L. MULFORD  
GERALD E. KLONGLAN  
*Iowa State University*

Litterer explains that this edition is presented in two volumes in order to examine a number of topics more intensively and to include "control" and "systems" as separate topics. The topics and readings in Volume I are designed to focus upon the basic structures of organizations and upon the way "the parts (structures) move and do something in a fixed and determined way," i.e., focusing upon organizations in the simple steady state. The topics and readings in Volume II are concerned with cybernetic control, i.e., the capacity to control operations to achieve an end within a "closed" system, although there is no neglect of the organization as a semi-open system which adapts to its environments. Most of the readings retained from the first edition now appear in Volume I, while the additions are presented primarily in Volume II.

This collection of the "most important writings on organizations" is intended to be used in "advanced undergraduate or graduate level courses, or by the interested business or government executive." Apparently more of the readings were selected for the latter reason, as there is a strong emphasis on readings which

relate to business organizations, knowledge for executives, and "human relations." The quality and depth of Litterer's introductory passages are uneven, but good enough to enable one to use one or both of the volumes as the primary reading for an advanced undergraduate course.

It would be preferable if more of the 49 readings in Volume I related more specifically to those Etzioni calls "lower participants." The study of lower participants is a relatively neglected task, especially in non-business type settings and in formal voluntary organizations. Few of the readings relate to the impact of recruitment selectivity on the organization or to factors which may affect the commitment of lower participants, such as socialization and communication. We share Etzioni's view that it is at the level of lower participants that organizations differ most, and that a comparative analysis of organizations will be most fruitful in the long run.

We are more pleased with Volume II. The "external" aspects of organizations are currently receiving much attention. Litterer has selected 31 readings which relate to (1) general systems; (2) systems properties of organizations in the steady state; (3) organizations as open systems; (4) equilibrium, feedback, and control; and (5) adaptation, growth, and conflict. These topics are applicable to most organizations, and the readings seem to be more generally applicable and less oriented toward the business firm.

Either of these excellent volumes can stand on its own merit. We expect that they will be widely adopted and come to be known as basic readers.

*Power in Organizations*, edited by MAYER N. ZALD. Nashville, Tenn: Vanderbilt University Press, 1970. 336 pp. \$10.00.

EDNA E. RAPHAEL  
*Pennsylvania State University*

This book is composed of the papers and comments prepared for a conference held at Vanderbilt University in 1969. A title such as "Working Papers on Power in Organizations" would have been more appropriate, since it would clearly convey the particular contribution made by the volume and thus differentiate it from numerous currently available sociological readers and treatises on this and related subjects. As an encounter with problems and

models for the study of power in organizations, this volume also has an exploratory quality, one of its important merits. Within the bounds of a few hundred pages, contrasts in levels of theoretical perspective and methodology draw attention to many issues and problems and to some of the hazards that attend study in this area. The volume thus tends to sharpen the reader's critical faculties.

Among papers which report findings of studies currently in progress, those of Bucher, Goldner, Blau, and Perrow seem particularly interesting. The Bucher and Goldner papers are reports of case studies. The Blau and Perrow papers report comparative studies. Divergent findings in the reports of Bucher and Goldner suggest new lines of inquiry and direct attention to difficulties which may attend interchangeable use of concepts specifically designed for either inter-organizational or intra-organizational studies, but not for both. While Bucher refers to an inter-organization approach, she is concerned primarily with the distribution of power within the structure of the separate professional medical units of a large medical school. Goldner looks more closely at relations between the industrial relations staff and other units which compose a multicompany industrial corporation. One wonders if the marked collegial structure characteristic of the distribution of power within units of the medical school faculty is not to some extent a function of the great power available to the elite position in an hierarchical organization of health-education services. Goldner's industrial relations staff, clearly not an elite but instead bound on one side by obligations to numerous units higher in the management hierarchy and on the other by the unpredictability of demands from labor unions, resorts internally to a bureaucratic rather than a collegial structure of power. The bureaucratic structure is a defense against incursions upon its distinctive organizational position as a staff with some autonomy due to expertise. A bureaucratic hierarchical structure assures uniformity of rulings across dispersed industrial relations units and increases the security of the position of staff expert.

The papers by Blau and Perrow are instructive for different reasons. In the Blau paper this reader is struck not so much by the findings, part of a major effort in the comparative study of large samples of organizations, as by the intriguing fact that it portends a time when the field of organizational studies will be both theoretically and methodologically rigorous. In his report of findings from a preliminary analysis of survey data, Perrow displays amazing candor in describing some of the difficulties

which attend a comparative study of the distribution of power among basic units of industrial firms. Indeed, his study almost founders over such a detail as the small size of his sample of organizations.

The more theoretical papers in the volume are as diverse as the research papers. Zald's discursive think-piece is as illuminating in its own way as Pondy's limited mathematical model. Each of these papers offers the reader new insights and demonstrates the potential existence of new avenues of inquiry. Zald's paper is an ingenious exploration into combining the frames of reference of the political and economic sciences with the interactional approach of sociology. Because Zald rather freely mixes theoretical materials at various levels of generalization with more empirical findings, the theoretical issues developed are sometimes blurred. Unfortunately Pondy's paper tends to perpetuate that formal organization theory which already is heavily weighted by business models of operations. Some of the papers by commentators are as instructive as the main papers of the conference.

*The American Corporation: Its Power, Its Money, Its Politics*, by RICHARD J. BARBER.  
New York: E. P. Dutton and Co., 1970. 309 pp. \$7.95.

JOSEPH HARRY  
Wayne State University

The primary thesis of this book is that the tremendous increase in the concentration of corporate stock ownership has created a new system of corporate relationships with which the legal apparatus of the federal government is ill-equipped to deal. Because that apparatus is designed to prevent limitations on competition within industries, it cannot deal with the growth in recent decades of conglomerates spanning many industries. Moreover, the conglomerate corporations, serving primarily as financial centers for their divisions, have become engines of investment on a massive international scale. To the extent that large corporations come to have their operations located in a variety of countries, they become independent of any particular government and may frequently act at cross-purposes with their "home" government's foreign policy.

The initial chapters document the growth of the conglomerate over the last two decades in the United States. Although successfully documented, much of this material is known to

any reader of the New York Times. Of interest is the fact that ownership of corporations seems to be drifting out of the hands of individuals and into the control of financial institutions such as mutual funds, insurance companies, and banks. The author presents these data in a harum-scarum fashion and tells the reader repeatedly that such concentrations of economic power have "fundamental," "significant," "momentous," and "far-reaching" implications. However, we are left completely in the dark as to what these implications are.

Part II of the book reviews recent structural changes in the economy, such as the rise of the service sector, the decline of organized labor, and the growth of ties to the university. Again, the reader would find most of this material in *Fortune*. Part III documents the massive commitment of industry to research, and argues that the technological innovations flowing from the research laboratory give rise to perpetual market insecurity for individual firms which must perpetually fight "the winds of technological and economic change" (p. 164). This argument and the expression, "the winds of change," constitute unreferenced references to Schumpeter's *Capitalism, Socialism, and Democracy*. Part IV describes the need for government to call on business for technical and economic assistance in dealing with social problems, while entertaining the idea that government-run programs to solve problems may well be less costly than business-run programs. This presents the entertaining idea that business, having created most of our social problems at an immense profit, will now solve them for a similar profit.

The last part of the book, concerning the internationalization of business operations and, to a lesser extent, ownership, stands as the major accomplishment of the book. The author points to the problems which arise when international corporations that owe allegiance only to profit and further investment must operate in a world of jealous nation-states demanding allegiance only to themselves. The firms must adapt to a diversity of national laws, and the nations find it difficult to control the operations of their own firms in foreign lands. The author cogently argues for the development of international agreements to regulate international corporations. On the whole, the book is quite uneven, being a mixture of new ideas, old ideas, and borrowed ones. Most of the data are presented dramatically but are not particularly new. Still, the marriage of data and ideas seems a fairly happy one. The book could certainly be used well in an undergraduate course in sociology or economics.

*The Business Enterprise in Modern Industrial Society*, by JOHN CHILD. London: Collier-Macmillan, 1969. 152 pp. Clothbound, \$3.50. Paperbound \$1.75.

WILLIAM D. HEFFERNAN  
University of Missouri

Unless readers are familiar with the work of John Child, the title may cause many students of complex organizations, industrial sociology, and social problems to dismiss this book as just another in the managerialist tradition. The series title, *Themes and Issues in Modern Sociology*, hopefully will cause sociologists to examine it more closely. In this small volume, which includes a thirty-page bibliography, Child has drawn extensively from literature concerning both business and non-business organizations in an admirable attempt to answer questions such as: What are the social costs of business enterprises? How and to what extent are the powers of business enterprises restricted? How does the environment influence the organization? How is size and technology related to the business organization? How is technology related to quality of life? What means are available for altering the effects of technology? The author's major emphasis focuses on the relationship between the business organization and the social environment. He is concerned with quality of life for the people of a society, without losing sight of the economic considerations of the business organization.

The major contribution of this book is the synthesis of a large body of information drawn from both sociology and economics which is relevant in analyzing the quality of life in an industrial society. The author has achieved his objective "to review concisely the major issues concerning the role of the business enterprise in contemporary societies, and to assess relevant sociological argument and research." His generous use of information from different cultural settings provides an opportunity to examine the operations of organizations in different political-economic settings.

One might disagree with some of the optimistic conclusions the author has drawn, and cite additional studies related to the issue. However, this book is designed to encourage further studies, so that empirical evidence might be used to answer questions currently remaining open to debate. Child has pointed the way by both explicitly and implicitly suggesting propositions.

The book fails to suggest new measures or methods of obtaining some of the needed in-

formation requested. For example, the author emphasizes quality of life but relies on studies of alienation, job satisfaction, leisure time, and economic measures. He indicates that psychologists should pursue studies relating to mental health, wants, and needs. Perhaps sociologists should also attempt to define and measure those sociological aspects related to quality of life. A similar type of measurement concern is evident in the discussion of organizational structure. Although this structure is central to Child's elaborate model of "variables influencing quality of experience within business enterprises as indicated by job attitudes and behavior," his discussion includes traditional measures such as amount of decision making, supervisory style, integration into the group, and size. Since Child sees the organizational structure as mediating among environmental features, personality features, organizational features, and job attitudes and behavior, while orienting business organizations to be socially responsible to the society and economically responsible for organizational survival, more attention should be directed to elaborating what Child calls "revised structural arrangements."

These criticisms admittedly go beyond the author's original objective and are partially the result of the book's suggestions regarding the severe limitations existing in the study of organizations. The author is to be commended for the way in which he has systematically woven together a vast number of current as well as classical studies in an attempt to indicate what is known and to suggest areas of needed research. Sociologists interested in industrial sociology and complex organizations should become familiar with this book.

*Manpower Report of the President: A Report on Manpower Requirements, Resources, Utilization, and Training*, prepared by the UNITED STATES DEPARTMENT OF LABOR. Washington, D. C.: U. S. Government Printing Office, 1970. 329 pp. Paperbound. \$2.50.

JUDITH N. CATES  
American Psychological Association

The 1970 *Report* is the first of the Nixon administration; the 1968 *Report* was Johnson's last. A difference in tone is set in the Presidents' letters of transmittal to Congress: Johnson writes of personal dignity and hope, and Nixon of well-constructed and well-administered programs. The 1968 *Report* proceeds in a social

psychological vein in the initial chapter, "New Perspectives on Manpower Problems and Measures"; the first chapter in the 1970 *Report* is "Manpower and Economic Policy."

Manpower programs are viewed as a potentially valuable contribution to the government policy of price stability, economic growth, and full employment. In contrast to the traditional tools of economic policy, manpower programs can contribute to full employment and increased production without undesirable inflationary consequences. Frictional unemployment may be reduced by computerizing job information; job banks will also facilitate data collection and analysis relating to the labor market and to the operations of the employment service. Youth unemployment may be relieved by reducing the uncertainties relating to the military draft, a full-year academic calendar year, and programs aiding the transition from school to work. Safety legislation may reduce loss of productivity attributable to accidents; financial incentives to encourage winter employment in construction could increase productivity in that industry. Unemployment insurance cushions the impact of unemployment; in periods of economic slowdown the benefit period could be extended. Coverage should also be extended to additional employment categories: state and local government, domestic service, non-profit organizations, and farm workers.

Two key words in the Nixon philosophy of government are *decentralization* and *evaluation*. In the proposed Manpower Training Act "primary reliance is to be placed on the elected heads of state and local governments for decisions on the allocation of manpower program resources" (p. 83). The proposed act would also stimulate the development of "measures of success and failure related to improvement of an individual's welfare rather than to the number of transactions in which he has participated" (p. 83).

The final chapter, "Manpower Demand and Supply in Professional Occupations," should be of personal interest to sociologists. The demand-and-supply situation in the professions is easily summarized: "Employment requirements will continue to rise faster than in any other major occupational group during the foreseeable future, as they have in recent decades. But the supply of college-educated personnel—the chief source of professional manpower—is mounting to unprecedented levels" (p. 161). The problem is essentially a demographic one; however, the writers confess to some uncertainty in their projections:

Because of the crucial relation of Government policy decisions to both the prospective supply



of Ph.D.'s and the demand for them in different specialties, future supply-and-demand relationships in this segment of the professional work force have peculiar uncertainty. Another imponderable factor is the capacity of these highly trained personnel to themselves generate new and added demands for their services, through their own scientific breakthroughs (p. 166).

Low growth requirements are projected for elementary and secondary teachers. Teacher training is the largest field in predominantly Negro colleges and the largest field of professional employment for women. In a society concerned with promoting opportunities for the disadvantaged, these facts may be viewed with some alarm. The health occupations remain an area of chronic shortage and, therefore, an exception to the characterization of the general manpower picture in the professions.

The 1970 *Manpower Report of the President* is, like its predecessors, an excellent buy at \$2.50. The Statistical Appendices alone may be worth the price.

*Incentives to Work*, by DAVID MACAROV. San Francisco, Calif.: Jossey-Bass, 1970. 253 pp. \$8.75.

*Clients or Constituents: Community Action in the War on Poverty*, by NEIL GILBERT. San Francisco, Calif.: Jossey-Bass, 1970. 192 pp. \$7.75.

*Community Control: The Black Demand for Participation in Large American Cities*, by ALAN A. ALTSHULER. New York, N. Y.: Pegasus, 1970. 238 pp. Paperbound. \$2.75.

MAYER N. ZALD  
Vanderbilt University

Harrington's *The Other America* appeared only seven years ago! The Fracas on Poverty and the Black Rights Movement captured the attention of the media and the intellectuals. Consequently, there has been an avalanche of books dealing with poverty, urban ghetto-life, and the civil rights movement. Although all three of these works are *au courant*, they differ enormously in the quality of mind that they reflect and their ability to inform about the past and future of key issues of social policy. They touch upon many of the main issues raised by our jousts with poverty and black power—welfare payments and the motivation to work, problems of involving the poor in decisions about services, and the political control problems that emerge if one takes seriously the rhetoric of decentralization.

*Incentives to Work* poses two questions: (1) Why do so many Americans fear that provision

of adequate income will sap the poor's willingness to work? (2) Do research findings provide systematic evidence of a disincentive to work accompanying income supplements? Macarov pursues answers by reviewing and synthesizing previous writing and research. He begins with neither a special intellectual slant nor theory nor an interest in re-analyzing data according to new criteria. In this situation Macarov would have to be especially hard-headed or insightful to make the review of real import. Instead, as soon as one reads the Prologue, one suspects what he will find. The author believes "passionately that poverty . . . can and should be completely eliminated in the United States, no matter what the costs or obstacles." Later in the same paragraph: "I believe poverty should be eliminated simply because it should not exist."

The first three chapters review the background of American welfare policies and current proposals for change (Schwartz, Friedman, Theobald), the definition of poverty and the occupations of the poor. Nothing new. Chapter 4, "Motivation to Work," may be the most useful in the volume; it pulls together material on the meaning of work in American society in order to isolate why many have such an ambivalent attitude toward work, a feeling that work is important to the definition of self and a fear that the poor, if given subsistence, might opt out. The material reviewed ranges from instinct theory to the Protestant Ethic. Even this chapter is unsatisfactory; for one ends up with a set of opinions and interpretations, not a coherent explanation. Chapters 6-11 review research in several areas that might show disincentives to work—the effects of relief payments, unemployment insurance, and disability payments. As Macarov notes, these research findings must be carefully weighed, for there have been few experimental designs employed, and a host of contaminating factors could have influenced findings. The author usually finds no evidence of disincentives. The whole study could have benefitted from analytic forethought. Rather than asking "Is there evidence of work disincentives?" the question might well have been "Under what conditions do subsidies lead to work incentives or disincentives?" The bibliography is useful.

Gilbert's *Clients or Constituents* and Altshuler's *Community Control* deal with the participation of blacks and the poor in the control of political and institutional decisions. Gilbert's book is a very solid case study of the evolution of Pittsburgh's Community Action Program, which seems to have been one of the more successful ones; it had low conflict with the

Mayor's office, it received financial allocations far beyond those that would have been expected from OEO guidelines, and it received favorable reviews from Sargent Shriver and the press.

Gilbert attributes Pittsburgh's initial success to its domination by a small group of like-minded welfare professionals located in key institutional positions, including the Mayor's office. They started to work on the program before OEO legislation was actually drafted, and they had access to key agencies and groups. At first, little attention was paid to community involvement. Later, however, as the neighborhood organizations developed internal vitality and as the neighborhood coordinators (professionals) developed greater commitment to social change than to service delivery, mobilization of the community and of discontent would become more important functions. Indeed, the Mayor's Committee on Human Resources (the official CAP agency) moved from clandestine support of special purpose action groups to explicit and official support. Gilbert does a fine job of tracing the evolution and dilemmas of involvement of the poor, the changing role of the professionals as they teach laymen how to bargain with agencies, the changing allocation of funds away from educational programs toward community organization and other programs. The main limitation of the book is its failure to exploit opportunities presented for comparative analysis. For instance, a typology of the potential power of the eight poverty neighborhood organizations is presented without showing any consequences for financial allocations or conflict. More important, a chapter comparing the Pittsburgh experience with that of other cities would have given insight into broader issues of how to organize effectively community action programs.

Altshuler's long essay is an informed speculation about the possibilities and values of radical decentralization of the central city. I approached Altshuler's book with a heavy bias against it. In his "Acknowledgements" he tells how much he learned from black activists; and, though I had read other fine works by him, I was prepared to find more heat than light—a glorification of power to the people. Instead, here is a careful weighing of the pro's and con's of decentralization, the possible financial structure, the criteria for representing city and organizational interests in the new governmental structures, the value of having a neighborhood chief executive who might also be the district councilman, the problems posed for racial integration of heavily black-dominated local governments, and others. Although Altshuler is clearly in favor of decentralizing

the inner city, he gives the "anti's" their due.

Altshuler's position rests upon several key assertions and assumptions. First, he notes that in 1960 only 22% of the population lived in local jurisdictions larger than 250,000. What blacks are demanding is what most whites already have: local governments that they can affect. Second, he notes that there are few economies of scale above the quarter million mark. Third, and most important, he argues that the ghetto crisis is not one of effectiveness but of legitimacy. As long as the police are an occupying army, as long as the schools are an externally imposed custodial institution, legitimacy will not be restored. His argument is of course more complicated than this, and skillfully buttressed with survey and other data. Sometimes, however, he uses pretty weak evidence. For instance, he argues that the Mayors came to believe that CAP's were useful because they gave an entré to the ghetto, possibly helping them control the ghetto in times of crisis. It is at least equally plausible that the Mayors would find almost any program useful that showed them to be progressive fund raisers. Moreover, the control of ghettos is extraordinarily complicated: CAP programs may help create the problems the reactions to which they also help control. But this is minor. Ultimately what Altshuler is calling for is the suburbanization of the city. If it ever comes, the forms of community conflict will change as blacks conflict among themselves, new forms of ideologically oriented urban machines emerge, and the operations of professional bureaucracies change. Meanwhile, Altshuler's excellent book combines informed and critical speculation with liberal social values.

*The Engineers and the Social System*, edited by ROBERT PERRUCCI and JOEL E. GERSTL. New York: John Wiley and Sons, 1969. 344 pp. \$9.95.

EDWARD E. KNIPE

*Virginia Commonwealth University*

According to the editors, this book examines the "present and future role of the engineer in American society." Part I, "Historical and Occupational Setting of the Profession," (a) deals with the rise of the engineering profession in relation to technological change and the rise of giant corporations; (b) compares, through the use of a "functionalist model," engineering with other professions; and (c) treats the problems of professionalization from a cross-cultural perspective.

The selections in Part II, "Recruitment and Socialization," take a close look at the engineer-

ing student and faculty, discussing student selection factors, the student's experiences in college, and his expectations for the future, and focusing on the authoritarian and prejudiced attitudes of engineering faculty relative to their colleagues in other disciplines.

Part III, "Work Roles and Organizations," includes three selections. The first deals with women in engineering and suggests that they do not fare well because feminine norms are different from the norms of engineering. The second reviews the failure of unionism and suggests that concern with the professional aspects of engineering has prevented the emergence of collective bargaining units, traditionally associated with non-professional occupations. The third is concerned with obsolescence in engineering in the face of increasing technical knowledge. Part IV, "Careers and Society," deals with the relationship between engineering and social class phenomena and with engineering and family roles.

Although the authors intend this volume to present a coherent picture of the past, present, and future of the engineer, each chapter stands as an independent work relatively unconnected to other chapters. For example, the question of unionism emerges in several chapters. In the Introduction the authors review each chapter but never link one with another. Whatever linkages exist among selections could have been made through transitional comments by the editors, while closer editing would have made the book less repetitious. Nevertheless, because this volume does bring together a great deal of material hitherto unpublished, it is a contribution to the sociology of an occupation which promises to play a greater role in determining the human uses of technology.

*Sociological Approach to Religion*, by LOUIS SCHNEIDER. New York: John Wiley and Sons, 1970. 188 pp. \$5.95.

CHARLES A. HILDEBRANDT  
Keene State College

If there is a question about whether or not Louis Schneider is a Parsonian structural-functionalist, this book should provide a conclusive affirmative answer. The book is not only *about* religion, but at times reads almost like a religious celebration of this particular approach. Almost half of this relatively short book consists of an enthusiastic summary of structural-functionalism *à la* Parsons. This is not to say that Schneider is unaware of the limitations of the approach; he is careful to note the major criticisms of Parsonian thought. He seems most particularly conscious of and

defensive about the abundant charges of conservatism leveled against functionalism, and of its inability to account adequately for social change. With these criticisms apparently uppermost in his mind, he eventually launches into a description and analysis of selected aspects of religion.

Because so much of the book is devoted to a painstaking—even laborious and repetitious—elaboration and defense of structural-functionalism, one would expect and hope that, when the focus shifts to religion itself, Schneider would tie everything together. As he turns to the substantive, however, he allows us to do most of the application ourselves—if we are up to it, that is. There does indeed seem to be a need to make more explicit the linkage between the theoretical fabric he has recounted and his engaging analysis of religion.

Schneider's view is that within the Parsonian systemic scheme, religion can be seen as especially relevant to the two functional imperatives of pattern-maintenance and integration. Some of the key variables in his analysis are the relationships between the cultural and structural components of religion; the normative and distributive features of structure; structural differentiation; and the hierarchical ordering of "upward" and "downward" controls exerted among values, norms, collectivities, and roles. Schneider falls prey to some of the ambiguities common to the functionalists; for example, he does little if anything to clarify the amorphous concept of "structure." And what is the difference between a structural value (or social system value) and a cultural value?

The specific aspects of religion which Schneider uses for his purposes seem to be arbitrarily selected, but do include some always intriguing to sociologists. (Much to his credit, Schneider is very generous in giving examples. No one can accuse him of the usual parochialism of sociologists: his examples encompass the globe and his grasp of many varied cultures is sure). His careful discussion of the interrelationships of belief and ritual leads smoothly and clearly into an overview of the functional relationships between social system and culture. Structural differentiation is also neatly apparent in his demarcations between folk and universal religions. There is the almost inescapable discussion of the church-sect typology. One whole chapter is devoted to "The Case of the Protestant Ethic"; his qualifications of what Weber had in mind and his arguments against the critics of Weber are cautious and excellent. His elaboration of the idea of "functional alternatives" is good, especially his skillful use of the life and character

of Weber himself to illustrate nationalism as a functional alternative to religion. Schneider makes a real contribution in his analysis of "levels of religion," the idea that within any particular folk or universal faith there are levels of beliefs and practices which differ sharply with one another and yet may influence each other profoundly. A similar advance in conceptualization may be present in his distinctions among religion, magic, and instrumentalization. Schneider's offhand avowal that much is magical in modern man's non-rationality is refreshing. Schneider's use of "the tragedy of the idea" may hold promise for a better coping with the idea of social change. He illustrates this concept by discussing the impact of the Protestant Reformation upon economy, science, literature, and polity, and the subsequent "undoing" of Protestant intentions by the very forces Protestantism unleashed in these areas.

Clearly, there is much of value in this book, but there are some annoyances. It is always a chore to read a text so frequently interrupted by gargantuan (though sometimes interesting) footnotes and long parenthetical asides. There are also those banalities for which sociologists are always being criticized, such as Schneider's observation that "Were there no physical world and no organisms there would be no human personality or society or culture" (p. 67). The last chapter directs itself "Toward the Future" and ends up being little more than a listing of five definitions of secularization and a rehash of all the confusion that word dependably evokes. The index is none too good. Though the book is being promoted as a textbook, it is not a survey of the field. Its real value is likely to be supplemental reading and a discussion springboard for advanced students.

*The Sociological Interpretation of Religion*, by  
ROLAND ROBERTSON. New York: Schocken  
Books, 1970. 256 pp. \$6.50.

GEORGE W. BARGER  
*University of Nebraska at Omaha*

Although Robertson's book communicates not only the basic issues and substantive content, but also the "feel" of the sociology of religion, it is a frustrating book. The author confesses to an "overriding concern with theoretical problems" (p. 3). The result is a rather unfortunate "on the one hand . . . on the other hand" kind of narrative that at times lacks clear direction. What is one to make, for example, of the distinctions and redistinctions

which follow upon one another in Chapter 4, the least satisfactory section of the book?

Weber stressed the *acceptance* of the world in Confucianism, and the Puritan tendency to regard the world as inherently evil. . . . But since Weber appeared to regard Confucianism as *fundamentally* inimical to a doctrine of radical salvation, we may consider also the relationship between Islam and Christianity in terms of the distinction between other-worldliness and inner-worldliness. For Weber, Islam was less inner-worldly than Christianity. Yet the argument, advanced by Hodgson, that relative to Christianity Islam has been more this-worldly is a persuasive one, Hodgson maintains that Islam, Judaism, and Zoroastrianism are distinctively more this-worldly than Christianity and Manichaeism. Christians have traditionally conceived of their religion as consisting in a sacramental church with 'a sacred dogma *acting upon* a profane world,' whereas Muslims have regarded theirs as 'a body of universal law and a community which is bound thereto and which is the world at its best.' (p. 90)

Typologies are juxtaposed at such a level of abstraction that the substantive issue is sometimes lost. Indeed, the major typology, whose polarities give rise to the principal chapter divisions of the book, is so gross as to be virtually barren for empirical reference.

No doubt these criticisms point beyond the book itself to the discipline. Sociology of religion has moved scarcely at all theoretically since the formulations of Durkheim and Weber; empirically, it is plagued by a "religious sociology" rather than a sociology-of-religion emphasis, and by serious methodological weaknesses. One hopes that things are in the process of improvement, but only time and subsequent output will substantiate a conclusion that the current change is indeed positive.

Still, it is a fascinating field and this book has real attraction. There is not a major problem in the sociology of religion that does not receive discussion. Such questions as definition of religion, assessment of religiosity, and the relationship of religion and social change are treated in depth. The work is anchored squarely on a sociological framework. Only in the penultimate chapter does the author allow himself an extended digression into essentially theological and philosophical concerns. Robertson's theological sophistication is a pleasant overplus.

In sum, the book underemphasizes the empirical possibilities and needs of the sociology of religion. Nevertheless, Robertson has raised basic issues and their implications within an uncompromising sociological framework. His book certainly may be counted among the positive developments in the field.

*Millennial Dreams in Action: Studies in Revolutionary Religious Movements*, edited by SYLVIA L. THRUPP. New York: Schocken Books, 1970. 229 pp. Clothbound, \$6.50. Paperbound, \$2.45.

JOSEPH F. ZYGMUNT  
University of Connecticut

This book is in keeping with the recent revival of scholarly interest in millenarian movements which has begun to yield a renewed appreciation of them not merely as exotic specimens of religious life, but as fairly common articulations of social unrest deriving from a multiplicity of sources. Previously published as a Supplement (1962) to *Comparative Studies in Society and History*, it is a report of an interdisciplinary conference on millenarian movements held at the University of Chicago in 1960. Included are revised drafts of the major papers presented at the conference, as well as several notes and commentaries written subsequently by participants. The editor's introduction provides an informative summary of the conference proceedings, together with a brief evaluation of the progress made in achieving the general objectives of the symposium.

Students of millenarian movements will recognize the major contributors as scholars who have done outstanding work in the field. Drawing upon his research on medieval millenarism, Norman Cohn attempts to define the distinctive properties of millenarian movements, to identify some of their more important variations, and to venture some hypotheses concerning conditions favorable to their development. George Shepperson's critical paper on "The Comparative Study of Millenarian Movements" invites attention to several relatively unexplored dimensions of such collective enterprises which future research might usefully try to clarify. In an additional paper, Shepperson presents a more specific analysis of some modern millenarian groups in Nyasaland, focussing especially on various segments of the Watch Tower movement and considering the factors which have contributed to their differential politicization. René Ribeiro compares the messianic movements which emerged in Brazil during the pre-colonial, colonial, and modern periods, derives a typology of such movements, and offers a set of explanatory hypotheses. The role of millenarian ideas in the Taiping Rebellion of mid-nineteenth century China is examined by Eugene Boardman. Justus van der Kroef presents a comprehensive analysis and systematic comparison of three messianic movements which arose in different parts of Indonesia during

the early decades of the twentieth century. Jean Guiart re-examines Melanesian cargo cults in the broader context of the efforts of Christian missionaries in the South Pacific, while Mircea Eliade invites attention to the role of certain indigenous mythical-ritual themes as factors predisposing Melanesian groups to develop such cults. George Simpson advances a socio-functional interpretation of the Ras Tafari movement in Jamaica. A classic case of chiliastically inspired revolution (the Taborite movement of the fifteenth century) is discussed by Howard Kaminsky, who emphasizes the medieval cult of the "Free Spirit" and its institutionalization as sources of influence upon this Bohemian experiment. The millenarian dimensions of the Savonarola movement in fifteenth-century Florence are explored by David Weinstein. David Aberle contributes a brief but suggestive essay on the applicability of relative deprivation theory to millenarian movements.

While the sample of millenarian movements treated in this book is quite impressive, scarcely any consideration is given to European and American cases of the last three centuries. A comparable symposium dealing with the latter would be most worthwhile. In general, the individual papers are of high scholarly quality. Firmly rooted in empirical research, predominantly analytical rather than merely descriptive, constructively critical in tone, and venturing conclusions with salutary tentativeness, this collection is a valuable contribution to the ongoing dialogue on the subject. Judging from this report, the conference came closer to realizing the objective of charting the basic similarities and differences among movements of the millenarian type than arriving at a unitary theory that accounts for their occurrence and variations. But some of the conceptual and propositional ingredients of such a theory do emerge from the deliberations.

*Student Freedom in American Higher Education*, edited by LOUIS C. VACCARO and JAMES THAYNE COVERT. New York: Teachers College Press, 1969. 165 pp. \$5.95.

*Black Power and Student Rebellion: Conflict on the American Campus*, edited by JAMES McEVoy and ABRAHAM MILLER. Belmont, Calif.: Wadsworth Publishing Co., 1969. 440 pp. Paperbound. \$3.95.

CHARLES E. RAMSEY  
University of Minnesota

These two volumes, on the same general subject, are quite different in nature. Vaccaro

and Covert do not state in any definitive sense their editorial intent nor give the criteria for the selection of readings, except to say that the volume "presents an array of views by individuals closely associated with college and university life in order to delineate significant issues, to expose certain tendencies and characteristics of students, and in general to offer the reader some mature and dispassionate insights concerning the question of student freedom in American higher education" (p. viii). This quotation is a good description of the book; for the chapters vary greatly in style, approach, the way they view problems for analysis, and quality. Some of them present an in-depth analysis of an important problem (e.g., the senior editor's chapter on "The New Student Subculture and the Search for Meaning"). Others present a superficial pulling together of a vast array of historical facts and current events, all mentioned only in passing, apparently selected to point up something completely escaping this reviewer's cognition (e.g., one essay skips glibly from Aristotle to Clark Kerr to *Time* magazine in a style characteristic of *The Saturday Review*). There is a balance in value position ranging from a chapter by an "angry student" to a historical development of the concept of apprentice and master, with the obvious implication that apprentices (students) cannot be involved in the decision-making process—all in extreme functionalist language. Most of the chapters provide numerous footnote references.

The McEvoy-Miller volume, on the other hand, is highly integrated, although it too consists of a series of readings by several authors. Part I consists of case studies of campus disorders: San Francisco State, Columbia, the University of Chicago, Duke, Stanford, and Berkeley. These valuable descriptions attempt to be as empirical as possible, but few generalizations can be gleaned from them. Various possibilities may help to explain this lack of attention to theory or to presumed describable processes: Is the theory of conflict inadequate to describe the actual happenings in campus revolts? Do reporters depend only on first-hand observation and therefore not report other events? Do observers choose to ignore conceptual frames which might lead them to look for events confirming or refuting ideas of the processes of conflict? Judging from the reports, the most likely explanation is the first one: leaders had a few simple notions, but most of the events were either out of their control or were planned on the spur of the moment.

Part II is a series of "position papers" by participants: S. I. Hayakawa, Max Rafferty,

Robert Chrisman, Nathan Hare, Roy Wilkins, Stokeley Carmichael and Charles Hamilton, Melvin Posey, Bill Barlow and Peter Shapiro, and Todd Gitlin. Many of these papers are dispassionate analyses, although clearly written from a "position." In future research they will become excellent documents. Part III is made up of a series of analyses of conditions leading to student unrest.

The McEvoy-Miller work is a well-organized set of readings, most of which are rich in content and many of which will be of inestimable value in further research on the student and black rebellions.

*The Unionization of Teachers: A Case Study of the UFT*, by STEPHEN COLE. New York: Praeger Publishers, 1969. 245 pp. \$8.00.

JOHN B. HUDSON  
Trent University, Canada

This study reports an example of substantial social change: an organization which moved from a "conservative and acquiescent group" to a militant striking group. Cole uses the New York City UFT to examine four sociological conditions that brought about change: (1) dissatisfaction, (2) channeling dissatisfaction into an action movement, (3) rank-and-file predisposition to action, and (4) social control or resistance to change by authorities.

In Chapter 3 ("The Growth of Dissatisfaction"), Cole documents the effects of inflation and other economic factors in influencing the growth since 1939 of dissatisfaction and a sense of relative deprivation among New York teachers. He makes the reasonable assumption that "by locating the places in the school system where there was greatest support for the union movement, it is possible to find those internal weaknesses that nurtured dissatisfaction and provided motivation for participating in action aimed at changing working conditions" (p. 35). The junior high school turned out to be the location that provided the bulk of early dissatisfaction.

Chapter 4 discusses "The Strike Ideology." New York City teachers were dissatisfied all during the 1940's and 50's, a period during which a strike ideology failed to take hold. After the union as a matter of policy decided to use the strike as a weapon and had some successes, the movement began to attract substantial membership. Cole concludes that the factors necessary for the strike ideology to succeed were (1) intense dissatisfaction by teachers, (2) union leaders with a militant pro-

gram of action, and (3) attitudes on the part of teachers favorable to striking. Teacher attitudes, initially unfavorable to strikes, were changed significantly by the civil rights movement, which legitimated unlawful activity if it was directed toward changing "immoral laws" (pp. 73-74). The militant union became more successful than its competitors in solving the financial problems of teachers so that it succeeded where the others failed.

Cole analyzes in Chapters 5 and 6 the attitudes of the rank-and-file teachers with respect to militancy. He shows that people with militant characteristics increased as a proportion of the teacher population in the 1950's, thus increasing the base of support among teachers for militant action in the 1960's (pp. 93-98). He then analyzes the structural conditions of the teaching job that influenced teachers' propensity to strike, concluding that attitudes toward the job influenced only those not already militantly disposed by background.

In analyzing the forces opposing change, Cole discusses, for example, several actions which the school board could have taken, which union leaders agree would have seriously impeded the growth of union militancy, and which the board had taken in earlier years. He suggests that these procedures would undercut "any movement that arises out of intense dissatisfaction with working conditions" (p. 110). Theorists, consultants, and executives may be interested in what Cole has to say about the four actions: "concerned paternalism, token concessions, co-optation of leaders, and sanctions" (p. 110). Cole believes that such factors as the political context, the legitimacy of the union's goals, and lack of communication with the union resulting in lack of knowledge account for the board's failure to act appropriately.

In Chapter 7, Cole discusses how the reactions of outsiders to the growing militancy of the teachers union mediated the link between predisposition and action. In Chapter 8 he continues by focusing upon four factors which were proximate causes in changing predispositions into actions: "social support, cross-presures, structurally determined fear of sanctions, and the significance of reference groups" (p. 131). In Chapter 9 the general question of the conditions for successful "unionization" of a profession is discussed. In Chapter 10 and in the Epilogue, Cole returns to his case study of the New York situation and further illuminates that situation with the theoretical apparatus developed in earlier chapters.

Cole has written an interesting book of value to those with an interest in the historical spe-

cifics of the New York case, as well as to those interested in the development of a theory of sociocultural change.

*Woman's Place: Options and Limits in Professional Careers*, by CYNTHIA FUCHS EPSTEIN. Berkeley, Calif.: University of California Press, 1970. 221 pp. \$6.95.

VIRGINIA OLESEN

*University of California, San Francisco*

American women's rising expectations have been accompanied in recent days by a flood-tide of popular and academic literature which on occasion has been redolent with sour feminism, tired Marxist clichés, warmed-over biological determinism, and, not unimportantly, poor scholarship. None of these qualities, however, describe *Woman's Place*, an urbane and solidly grounded work which confronts the issue of women's access to and progress in American professions with a careful analysis based on recent cross-cultural, statistical, and sociological materials, including the author's own research on women lawyers.

How this book will be read in and out of sociology is difficult to say, given certain rapid and dramatic Women's Liberation activities over the past year which have already altered consciousness of this question in ways which at this writing are only dimly understood, even though some consequent actions and responses seem clear enough. For some readers, Epstein is discussing and analyzing "that which everyone already knows"—for example, that working women often disparage themselves and other working women, that mass media depict the working wife invidiously, that very early sex role socialization channels female infants firmly but subtly into eventual domesticity, that unseen but stringent social barriers, formal and informal, exist inside and outside the professions for women in general and married women in particular. That these observations are well and dispassionately documented will strengthen the analysis and the argument for others less familiar with the issues.

How the book will contribute to sociology is more easily anticipated. The author's use of "status set," "status set typing," and "sociological ambivalence" to conceptualize and integrate various dimensions and levels of the problematic aspects of women's recruitment to, and careers or lack of careers in, the professions carries the analysis through critical questions of sex role systems and into the related theoretical areas of the sociology of occupa-

tions and professions, the family, and the social psychology of deviance. Those readers interested in comparative analysis will find these materials and conceptualizations profitable in the scrutiny of other themes where an incumbent's status characteristics do not accord with presumed attributes of the occupational status in question, e.g., male nurses, black military officers of senior rank, married nuns or priests, female politicians, physicians who are jazz musicians or sports figures.

This analytic stance leads the author to scrutinize institutions which are implicated in the status set of the married woman in a profession—for example, institutions such as child care facilities which relate to the statuses of wife and mother, and those which are attached to professional status, for instance, the organization of medical or legal practice. Epstein explores the implications for study of the professions themselves with respect to the structure of practice which would utilize women more fully, either by diluting the strength of sex-typing (these professions being largely male) or by altering the role networks wherein access to and ascent within professions are shaped.

Not the least of the volume's virtues are the many links to be made from this work to other salient inquiries—the extensive family time budget studies of Madeleine Guilbert and other French sociologists, Richard Hall's work on professions and their organizational context, Margot Jeffery's analysis of women physicians' practice in Britain, Robert and Rhona Rappoport's studies of two-career couples, Jean Bui-Dang-Ha Doan's analysis of the feminization of certain French professions, and, of course, the earlier studies of Alice Rossi, Jesse Bernard and Mirra Komarovsky. *Woman's Place* is, in short, to use that fatigued word of this era, "relevant" in the best sense—both concerned about an issue which merits attention and informed by a sophistication that maintains concern with the accumulation of sociological knowledge.

*Marriage and Work in America: A Study of Motives and Roles*, by JOSEPH VEROFF and SHEILA FELD. New York: Van Nostrand Reinhold Co., 1970, 404 pp. \$8.95.

HARRY GYMAN  
Bowling Green State University

This volume deals with three interrelated themes. First, the authors examine three roles—marriage, parental, and job—with respect to

three dimensions found in all roles: role demands, ambiguities, and gratifications. Data on respondents' perceptions of roles—"role reactions"—are analyzed with respect to several relevant variables: e.g., the marriage and parental roles in relation to sex, age of children, and level of education; the job role in relation to prestige of job, age, and other variables. In addition and of greater importance, each role is analyzed in regard to three "motives": achievement, affiliation and power. Thus, the roles investigated are merely intended as case studies with respect to the effects on role perceptions of these three motives. These relationships are used as points of departure for assessing and redefining what the authors call the "sensitization principle" and the "congruence principle," a statement of the conditions under which the strength of the motives may vary in role relations.

The *sensitization* principle is defined in this way: "in motivationally ambiguous roles, the strength of a man's motives will effect the quality of the demands, frustrations and gratifications that he perceives in these roles" (p. 12). The *congruence* principle is: "to the extent there is congruence between the strength of a motive and possibilities for the motive to be gratified in the role, there will be satisfaction and ease in role performance, and to the extent there is a poor meshing, there will be problems in the way individuals react to the role" (pp. 13-14). Thus, *sensitization* deals with a person's perceptions of roles, while *congruence* deals with "role performance, role choices, and role feelings" (p. 14). And both deal with the relation of motives to gratifications and possibilities of gratification in roles.

Thus, the scope of the book is quite broad; unfortunately, the analysis does not come off. Trying to compress all three elements (the roles, roles-plus-motives, and roles-plus-motives-yields-the-two-principles) into one work weakens all three efforts. The authors consider so many variables and so many dichotomies of these variables that the analysis quickly begins to suffer from an attenuation of cell sizes. For example, Table 4-2 (p. 159) contains 7 role reactions items, the three motives, and three levels of education, all by sex (mother) and age of children (school age). There are thus a total of 63 cells, not counting 21 cells of each role reaction by each motive by sex and education. Clearly, more refined analyses are precluded simply on the basis of number of cases. For example, the critical variable, strength of motivation, is not included in the analyses except in an *ad hoc* fashion. No such data are reported. Also no data are presented



showing any interaction effects among the three roles and/or among the motives.

The attempt to redefine concepts in terms of data is one to be applauded; that the attempt is unsuccessful is due in part to the nature of the study—one of those wide-ranging, shot-gun surveys. Hence, there is little precision in the development of specific hypotheses and in the development of specific measures through which the task of reconceptualization could be more directly attacked. Indeed, it is through the process of conceptual clarification itself that the research process is made easier. In this instance the authors had to use a very roundabout mode of data analysis in order to deal with the conceptual problem. Unfortunately, there are very few significant findings, and these are either so low (gammas of .20 or so) or so high (.80, say) that the first cannot be taken too seriously and the latter are too good to be true. In brief, the data do not support the burden of interpretation which they are called upon to bear.

Despite its defects, this book is worth reading, particularly by specialists in role theory and social psychology.

*Protest, Reform, and Revolt: A Reader in Social Movements*, edited by JOSEPH R. GUSFIELD. New York: John Wiley and Sons, 1970. 576 pp. \$9.95.

HAROLD A. NELSON  
University of Houston

Thirty-three articles, five previously unpublished, have been organized around seven topics: (1) differential responses to social change; (2) rejections of the social order and their direction; (3) structural sources of protest, reform and revolt; (4) the development of collective meanings; (5) ideology, myth and belief; (6) the organization of collective action; and (7) some contingencies of collective action. The selections include studies of a number of historical periods and societies as well as cross-societal comparisons.

The assets of the volume are several: (a) Gusfield's skilful editing will permit students to become acquainted not only with the authors' conclusions, but also with the structure and logic of their arguments and approaches to data analysis; (b) the various frameworks should encourage readers to try their own comparative analyses (Bayley, for example, in his "Public Protest and the Political Process in India" advances a series of propositions potentially applicable to politics and protest in the

United States); (c) the selections frequently complement one another (e.g., Moynihan's "The Professionalization of Reform" and Hofstadter's "What Happened to the Antitrust Movement"); (d) the authors' conclusions may well provide the basis for informed discussion and debate (Moynihan's comments on the decreasing need of mass rallies to persuade governments that popular demands exist may prove to be a case in point).

Gusfield's Preface and section introductions raise several issues, the most important being the role of "human purpose, deliberate design and organization in the direction and redirection of societies." The selections should permit the student at least to see the relevance of the issue to the theory of the discipline and also, by implication, that the study of social movements is not a distinct entity so much as one variation of the sociological theme.

In American sociology the study of social movements has been confined largely to the domain of collective behavior. Gusfield's volume contributes to the healthy trend away from this confinement. Though not all sociologists will agree with his analytical framework or with his choice of selections, Gusfield has provided a useful volume for serious students of social movements. Careless proofing results in occasional annoyance.

*Poverty: New Interdisciplinary Perspectives*, edited by THOMAS WEAVER and ALVIN MAGID. San Francisco, Calif.: Chandler Publishing Co., 1969. 221 pp. Paperbound, \$3.25.

*Poverty and Wealth in America*, edited by HAROLD L. SHEPPARD. Chicago, Ill. Quadrangle Books, 1970. 279 pp. Clothbound, \$6.95. Paperbound, \$2.45.

*The Block*, by HERB GORO. New York: Random House, 1970. 191 pp. Clothbound, \$10.00. Paperbound, \$3.95.

ALFRED M. MIRANDE  
Virginia Polytechnic Institute

These three books reflect the wide variation of approaches to the study of poverty. The three approaches represented are the interdisciplinary (*Poverty*), journalistic (*Poverty and Wealth in America*), and ethnographic (*The Block*).

*Poverty* is a product of a symposium conducted at the University of Kentucky. Since the proceedings were tape-recorded and edited,

the book follows the symposium format: (1) a basic position paper is presented by eight participants representing the various disciplines, (2) a discussant comments on the position paper, (3) the author responds to the comment, and (4) finally, there is a general discussion.

This collection of essays promises much but delivers little. Few would dispute the editors' contention that "the literature on poverty has traditionally tended to reflect a narrow perspective (p. 1)" and that there is a need "to encourage both the expansion of disciplinary horizons and a holistic perspective on poverty" (p. 2). Yet the book suffers from the same narrowness which it seeks to overcome. Most of the position papers are characterized by pretentious and narrow discussions of broad topics. The most blatant example of this tendency is "The Political Scientist's Perspective on Poverty," which begins with the disclaimer that "political science has no unique contribution to offer in analyzing the causes of poverty" (p. 134). The paper turns out to be a detailed discussion of how the Economic Opportunity Act of 1964 was politically motivated, rather than a general analysis of the contribution of political science to the study of poverty. "The Psychiatric Perspective on Poverty" is limited mostly to a discussion of case studies from a child-psychiatry clinic which serves four eastern Kentucky counties. Another major criticism is that the controversy over the "culture of poverty" is virtually ignored. This omission is revealing, since many of the papers unwittingly employ the ideology of the culture of poverty. The "Psychiatric Perspective," for example, characterizes children of the poor as coming "from socially disorganized and psychologically as well as economically impoverished families . . ." (p. 112). On the positive side, the discussants generally provide badly needed correctives for the position papers and help to develop a few interesting controversies. There is also an excellent paper by Fuchs which critically examines the culture of poverty concept and suggests that the school helps perpetuate poverty and inhibits vertical mobility.

What was probably a fair symposium makes a mediocre book. The papers appear to be hastily thrown together, lacking the polish expected of published materials. Statements which may appear reasonable in oral presentations are nakedly exposed in print. With the exception of a brief introduction to the volume, there is no attempt by the editors to integrate the selections into a "holistic" perspective. The result is a series of unrelated position papers.

*Poverty and Wealth in America* is a collec-

tion of readings from the *New York Times*. It is surprising that a fairly good reader can be composed from a single source. The facts on poverty are presented clearly and persuasively, but there is little in the way of theories or even conceptual frameworks. The selections are concerned with poverty in different periods of American history, from the depression to the present. Several excellent points are made recurrently: (1) It is misleading to think that poverty is being eliminated simply because the minimum incomes of the poor are increased. Since poverty involves relative deprivation, the gap between the poor and the rest of society must be considered. (2) Concern with poverty as a social problem goes beyond income to other areas, such as giving power to the poor and control over their institutions. (3) Overall images and average statistics for the country as a whole obscure the "Other America" and make the poor invisible. (4) Many people are not born poor; they become poor after they retire from the labor market or become disabled.

Of the six sections in *Poverty and Wealth*, the last is probably the best. It presents alternative views and solutions. Two of the most important issues discussed are guaranteed income and community action programs designed to increase the power of the poor. The book may serve as a useful supplement in undergraduate courses. However, it should be clear that it is more concerned with describing the nature of poverty and suggesting short-term solutions than with understanding the causes.

*The Block* is an unpretentious little book which presents an excellent description of life among the poor through photographs and tape-recorded interviews. Herb Goro is a photographer who for one year lived with and studied the residents of a slum area in New York's East Bronx. The technique of combining photographs (120) and the residents' views of life in the ghetto is successful in creating a coherent and moving account of life among the poor. The book focuses on three adolescents, depicting their hopes, fears, accomplishments, and disappointments. It is a corrective for the many pathological accounts of the poor. Although the respondents are amid poverty, crime, illegitimate children, and rats, their life is not pathological. They share the American Dream and aspire to leave *The Block*, but are hopelessly trapped in a life of poverty. The residents reveal a keen understanding of their situation and insight into the structure of the society. A teen-age girl with two illegitimate children (different fathers) gives this analysis of the status of blacks: "The Muslims and the Panthers and the Five-percenters, they fighting

for freedom. But we got freedom—we're just slaves: we can do what we want without asking some master, but we don't got power" (p. 119).

*The Block* should be a useful supplement for courses in poverty or race relations. It provides the raw data on poverty which can be used to illustrate more abstract sociological principles. The major weakness of the book is that questions of methodology are ignored. The reader does not know what questions were asked to elicit the responses, whether some of the tape-recorded materials were excluded (and if so, why?), and the criteria employed to select respondents. However, these questions may not be relevant since *The Block* is more a work of art than of science.

*Mothers in Poverty: A Study of Fatherless Families*, by LOUIS KRIESBERG. Chicago, Ill.: Aldine Publishing Co., 1970. 356 pp. \$9.75.

MARVIN R. KOLLER  
Kent State University

This thoughtful book is a model research report that not only deals with a wide range of topics related to fatherless families—such as public housing, neighborhoods, socialization of the young, discriminatory practices, employment, public welfare, and racism—but also provides an underlying theory of poverty that links these topics.

Kriesberg is concerned with the conceptualization of poverty as self-perpetuating because poverty may be viewed as a subculture (inter-generationally affected) or as being essentially situational (contemporary circumstances alter or shape behavior regardless of shared values). Acceptance of the former would mean that long-range social controls may or may not eliminate or modify the conditions under which impoverished families endure. Acceptance of the latter would mean that there would be more confidence that changes in present social policies would markedly affect the lives of the poor. Kriesberg's findings validate "the situational" much more than the "culture of poverty" construct. However, as Kriesberg notes, the influences of generations, particularly in the inculcation of values, are present to a limited extent.

Data were derived from detailed interviews with a cross-section of households in four low-income public housing projects as well as their surrounding environs in Syracuse, New York, supplemented by interviews and follow-up interviews a year later with applicants for pub-

lic housing and by participant observation. The schedule of questions does not appear in the appendices, but perhaps its extensive nature precluded its full citation. Precise questions are cited in the footnotes, however; and tabular information derived from them, together with tests of statistical significance, are provided.

Kriesberg is concerned about the representative nature of the four housing projects and cautions about over-generalizations from limited, heterogeneous data. The projects and their neighborhoods varied by architectural structures, prior residency, ethnic, racial, religious, and socio-economic characteristics of their resident populations; school-home liaison, and social mobility patterns—all of which were demonstrated to have measurable effects upon findings.

For those specifically interested in fatherless families living in poverty there may be some disappointment. Fatherless families are not Kriesberg's real concern; rather they are the means by which the author seeks to understand overall questions about the durability of poverty patterns. The psychological interior of fatherless families is not delineated, but external factors sustaining the inadequacies of fatherless families are detailed. Low income, the lack of child-care facilities, and the difficulties of employment and housing for female-headed families are among the circumstances with which the project families learned to cope.

Each of the ten chapters is summarized, and the concluding chapter brings together the key findings and conclusions of the study. Kriesberg argues quite persuasively for expanded social security programs, more generous public assistance programs, negative income tax programs, and family allowance programs. His expertise rests upon the ability to distill the essence of masses of data that bear upon the circumstances in which poor families live, particularly fatherless households, and to suggest to the "nonpoor" that there are ways and means at hand to create happier lives for them—and for ourselves, since we are inseparably linked.

*Disability and Rehabilitation: Legal, Clinical, and Self-Concepts and Measurement*, by SAAD Z. NAGI. Columbus, O.: Ohio State University Press, 1969. 329 pp. \$10.00.

NORMAN K. DENZIN  
University of California, Berkeley

Sociologists have been slow to study America's social welfare institutions. Their interests have

focused instead on the more dramatic and, therefore, more public socialization and care-taking institutions. While there is a rather well-developed literature on mental hospitals, prisons, and colleges, nothing similar exists for welfare, public aid, and rehabilitation agencies, although the names of Blau, Wilensky and Lebeaux, Hughes, Beck, Zald, R. Scott, and Sussman come to mind. Until most recently the study of social welfare had largely fallen into the hands of politicians, journalists, economists, social workers, and an occasional psychiatrist.

Nagi's research, sponsored by the Social and Rehabilitation Services Administration, begins to fill this gap. His is perhaps the first large-scale sociological study of the Social Security Administration (SSA) and its related programs. It must be judged in this light.

But, in the main, this is more a methodological-epidemiological-evaluational study of how the SSA evaluates clients than an examination of the experiences of either clients or professionals, within the organization or in society at large. Nevertheless, Nagi does attempt—in Chapters 2 and 5 especially—to incorporate portions of the labeling and functional theories of deviance into his analysis. He reviews the literature on the sick role, stigma, public stereotyping, and the effects of rehabilitation on families. He also indicates the importance of the physician as a decision-maker in the rehabilitative career, and draws out the structural strains inherent in the physician's role. In these senses his study will become a point of departure for future analyses of disability, rehabilitation, and welfare practices.

His basic purpose (pp. 7-8, 27) is to clarify the concepts "disability" and "rehabilitation" and to assess and compare the evaluation decisions reached at the local, state, and national levels of the SSA. Clinical teams, whose composition and perspectives are never clearly revealed, were sent to three target Standard Metropolitan Statistical Areas: New Orleans; Minneapolis-St. Paul; and Columbus, Ohio. Their objective was to compare their clinical evaluations with those reached at the three levels of the SSA.

In Chapter 2, Nagi offers a conceptual framework for the analysis of disability and rehabilitation, and distinguishes disability from pathology and impairment, stressing that his model is most relevant for physical problems. Three definitional types of disability and rehabilitation are presented: legal, clinical, and self. No attempts are made to study self-definitions. On p. 16 Nagi states that: "These assessments were believed to be beyond the technical

competence of persons included in the sample." Just why this judgment is made is unfortunately never elaborated. It is also misleading to have self-definitions contained in the book's title. Rehabilitation programs do more than evaluate client applications. They shape and alter self-conceptions, personal careers, and social relationships. By failing to include data on the subjective experiences of disabled persons, Nagi's study largely passes over the human tragedy contained within our society's welfare systems. That people lie, cheat, and rewrite their biographies to beat or get into the system is a reflection on the system. Conversely, it must be noted that welfare and clinical workers are cast into positions of making moral—not just statistical—judgments about social selves. Their ideologies for passing along responsibility, for letting the system speak for them, must be uncovered.

In Chapter 3, Nagi speaks to the problem of decision-making in the SSA and suggests some of the recent research by ethnomethodologists on organizational perspective (Sudnow, Cicourel). However, the negotiated, compromising, and often self-serving character of interactions in welfare agencies is never directly confronted or pursued. Instead, Nagi suggests that evaluations require information, criteria, and judgement. Chapters 5 and 6 present data from the clinical teams on the role of these variables in disability evaluations. In Chapter 9, Nagi briefly reviews the work of Scheff, Friedson, and Goode on decision-making errors, stereotyping, referral systems in medical organizations, and professions as communities. This is the most theoretical chapter, but it fails to offer a systematic framework for future research. The book lacks a summary of the study's findings.

I have indicated what I regard as the main weaknesses of Nagi's report. The self is omitted from study, and the moral issues of rehabilitation are overlooked, as are agency compromises and negotiations. Problems of stereotyping and stigma are *singled out* for future research, but remain unexamined in this study. A nagging feeling remains. If SSA personnel are making few judgmental errors (as Nagi indicates), if things are working so well, why are so many people unhappy with the present system? Until Nagi's unanswered questions are more directly attacked, an important feature of everyday American life remains unexamined. How do the maimed, crippled, disabled, rejected, and unwanted make out; and, last, how do their caretakers, the keepers of the welfare system, manage to absolve themselves of responsibility in the face of such events?

*The Daily Needs and Interests of Older People*, edited by ADELINE M. HOFFMAN. Springfield, Ill.: Charles C Thomas, 1970. 493 pp. \$16.50.

RUTH ALBRECHT  
*University of Florida*

This book includes selections from a variety of fields. Seven of the 26 contributors are sociologists who have written extensively on aging. Others are specialists from medicine, psychology, home economics, adult education, architecture, business administration, social welfare, political science, or government agencies dealing with older people.

The book is divided into seven sections. The first is a presentation of the discipline of gerontology, from its philosophy to a review of events and developments in recent years. Section Two includes four chapters which cover demography of aging, the elderly and politics, employment, retirement, and income maintenance, as well as the Federal role in consumer protection.

The third section is concerned with the psychological and sociological aspects of aging. The chapter on understanding the aging process includes an analysis of how experiences in early life affect the individual in later years. The social relations chapter covers social role concepts, the disengagement theory, group memberships, and takes a long view of the future, when social situations may be manipulated or managed to make the older years better. Leisure and meaningful use of time are reported for a variety of personality types. Religion, religious institutions, and their functions are covered in a concise, sensitive presentation. Part Four deals with the biological aspects of aging, including a brief but inclusive discussion of the functions and changes of major body organs and glands and a review of health, illness, preventive care, and comprehensive programs designed to maximize wellness.

Basic needs of the aged are discussed in Section Five. The chapter on nutrition has charts to show nutrient requirements, but also goes beyond basic food intake to nutritional assistance needed by older people. Housing problems are discussed from an architectural standpoint, giving due recognition to sites, social class differences, group and individual living plans. Sociological and practical problems of providing suitable wardrobes for older persons include clothing cut, comfort and style, and the fact that the very active social life in some retirement communities requires a variety of costumes. The chapter on family relations centers on the family as a social system, intergenerational help patterns, subgroup variations, and theories associated with aging. Sec-

tion Six looks at voluntary organizations, the responsibility of the Federal government, functions of community health services, and educational programs for the aged.

By the time the reader reaches the last section he should be ready for the three chapters which consider future developments in this area. Research is analyzed in a comprehensive approach, which should encourage even the people who shy away from investigating aging or the aged. There is a chapter on training in social gerontology in institutes, job training centers, and academic settings. The final chapter covers perspectives for the future and the effects of social change.

The title of this publication does not seem quite accurate, since it suggests more attention to individuals and their day-by-day living needs than is actually given. It is intended for home economists but certainly is not limited to that group. The contents can be recommended for courses in gerontology and related fields. Hoffman has produced a well edited and useful book full of basic facts and information.

*Comparative Perspectives on Race Relations*, edited by MELVIN M. TUMIN. Boston, Mass.: Little, Brown and Co., 1969. 312 pp. Paperbound. \$3.95.

FRANKLIN J. HENRY  
*McMaster University, Canada*

Dealing with race relations in various countries and regions, most of these readings are general surveys, with a few on some specific aspect of race relations such as the perception of skin colour in Japan. Tumin has written an introductory paragraph for each section and an introductory chapter summarizing some of the basic concepts and propositions in this field. Six of the selections are from previous collections: Romanzo Adams on Hawaii; Ralph Beals, Spanish America; Elaine Burgess, United States; John Donoghue, Japan; Albert Hourani, Near East; and T. H. Silcock, Malaya. Six of the selections are from monographs: Michael Banton on Britain; Kingsley Davis, India; Richard Thompson, New Zealand; Tumin and Arnold Feldman, Puerto Rico; Pierre van Den Berghe, South Africa; and Charles Wagley, Brazil. Two selections are from the spring, 1967, issue of *Daedalus*: David Lowenthal on the West Indies and Hiroshi Wagatsuma on Japan. The two other selections are Michael Banton on Africa south of the Sahara from the *International Social Science Journal*, and Herbert Hyman and Paul Sheatsley's second article

on attitudes toward desegregation from *Scientific American*.

The central question around which the editor has organized this reader is "What are the conditions under which people who differ in some significant identity . . . (specifically) race, live at peace or compete or conflict with each other?" This idea is not elaborated, conflict is not defined, the paragraphs preceding the selections are not oriented to this analysis, and the readings are not ordered along a peace-conflict dimension. Instead, the articles are arranged alphabetically: Africa south of the Sahara, Chapter 1; South Africa, Chapter 12. Actually, most of the selections are basically descriptive and make little attempt to explain why race relations are as they are. When explanations are given they are sometimes unconvincing. For example, Hourani asserts that there is little prejudice against black-skinned people in the Near East, and that one reason for this (the only one given) is that Islam teaches that race distinctions are of no importance in the eyes of God.

For the most part, the selections are rather superficial. Areas as vast as Africa south of the Sahara or the Near East are covered in 15-20 pages. Lack of research on race relations in the particular areas is another difficulty, a fact frequently lamented by the authors. In part, the superficiality is due to the almost complete divorce of the research presented here from the mainstream of sociological theory. For example, Michael Banton advances a "stranger hypothesis" to help explain white-coloured relations in Britain without reference to the classic ideas of Simmel concerning the stranger. In part, the superficiality results from the editing. For instance, under the general heading of forces tending to diminish caste, only one sentence in Davis' paper has been retained under the subheading "urbanization": "The anonymity, congestion, mobility, secularism and changeability of the city makes the operation of caste virtually impossible." The succeeding sentences in which this lead sentence is explained have been cut.

There are a number of small annoyances—in Tumin's own selection we are asked to note a "startling fact" based on four cases; 2 out of 11 cases are expressed as 18.3% and we are told that 67.98% of the college group are white; we are also informed that some definite relationship exists between educational level and skin colour, but that it is "hardly a matter for serious concern."

The beginning student of race relations reading this book will acquire some appreciation of the varieties of such relations existing in

the non-communist world a decade or so ago. The two more or less extreme cases of Hawaii and South Africa are presented clearly and concisely, and Davis' *Population of India and Pakistan* is by way of being a classic. In general, Tumin might have made a greater contribution to the discipline had he performed the analysis and himself answered the question which he regards as the central focus of his reader.

*Race and Class in Latin America*, edited by MAGNUS MÖRNER. New York: Columbia University Press, 1970. 309 pp. \$10.00.

PIERRE L. VAN DEN BERGHE  
University of Washington

This is yet another collection of papers spawned by yet another topically specialized conference. (Roughly half of the books I have recently reviewed have been of this type, and at least one third of my recent printed output has the same dubious origin in a *morceau de circonstance*, so the disease has obviously reached epidemic proportions.) The very first thing which struck me in the book was the misspelling of a distinguished Mexican anthropologist's name in the Table of Contents, and the mistake is repeated in one of the two entries about him in the Index. This creates an unfortunate impression of sloppiness in a book which contains several very valuable pieces.

Mercifully, the editor took his tasks seriously: he did not simply put his name on the cover of the book; he actually edited the papers, eliminated some, shortened others, and, thank heaven, cut out the discussions altogether. So the final product is above average for the *genre*. The book is organized in four parts consisting of three or four papers each. Part I is on "The Abolition of Slavery and Its Aftermath," with papers by Gonzalo Aguirre Beltrán, Carlos Rama and Richard Graham on Mexico, Uruguay, and Brazil respectively. Part II deals with "Immigration, Stratification and Race Relations" with contributions by Mario Vazquez on 19th century Peru, Harry Hoetnick on the 19th century Dominican Republic and Florestan Fernandes on 19th and 20th century São Paulo, Brazil. Part III is devoted to "Change in Indo-America During the Nineteenth and Twentieth Centuries" and includes pieces by Moises González Navarro on Mexico, Manning Nash on Middle America, and François Chevalier on Peruvian *indigenismo*. A concluding Part IV is concerned with "The Present State of Knowledge and the Interdisciplinary Tasks Ahead." Magnus Mörner writes on the historiography of race relations, Charles Ander-

son on the concepts of race and class in relation to politics, Octavio Ianni on race relations research in Brazil, and Hilgard O'Reilly Sternberg on the geographer's approach to race and class.

Having gone over the Table of Contents there is little space left to say anything meaningful about the book, which is the trouble about reviewing works like this one. Perhaps the main strength of the contributors is that they all share an acute sense of history which is so often lacking in the treatment of North American race relations. Scholars from Latin America are well represented; and as several of them have not written extensively in English, this book will have the merit of introducing monolingual English-speakers to their work. The book lacks a bibliography, other than the footnote references, a failing common to edited collections; but there is an index.

This leaves room for one conceptual criticism. Several of the contributors use the term *mestizaje*, which is doubly confusing. It is of course derived from *mestizo*, the term used in Colonial times to designate a person of mixed European and Amerindian parentage. In the last century or so, *mestizo* in Spanish American countries has acquired a cultural connotation to mean a native speaker of Spanish (who often happens to be genetically mixed as well), as distinguished from a native speaker of an indigenous language who is an *indio* or *indigena*. Thus, now, it is anything but clear whether the term *mestizaje* refers to miscegenation or acculturation. Even as a synonym for miscegenation, *mestizaje*, is a bad term to use in English because historically it referred only to Euro-Amerindian mixtures, not to Euro-African or Afro-Amerindian ones. One confusing term, and a foreign one at that, is indeed a poor analytical substitute for two clear ones. In addition, the word has the limitation of having currency only among Latin Americanists, and thus its use is a case of gratuitous esoterism. As several of the articles were originally written in Spanish, however, the fault may sometimes lie with the translator rather than the author. *Mestizaje* is much less objectionable in Spanish than in English.

*Politics and the Ghettos*, edited by ROLAND L. WARREN. New York: Atherton Press, 1969. 214 pp. \$7.95.

SEYMOUR SPILERMAN  
University of Wisconsin

This slim volume, a collection of 13 essays and comments, presents alternative strategies for

contending with the existence of urban ghettos. The papers, presented at the 95th annual forum of the National Conference on Social Welfare (1968), cover the range of intellectual disputations presently being digested by the social work establishment in developing a coherent *Weltanschauung* on ghetto social policy, with an accompanying set of action programs. Thus, diverse policy alternatives are considered, from the dispersal of Negroes to a more equitable allocation of political and economic resources among existing neighborhood groupings.

As is often the case with a collection of essays prepared for a conference, there is considerable variation in quality and originality. In this instance, because the authors have diverse formal affiliations—in the academic disciplines, government, social work, and activist organizations—substantial differences in ideological perspective are apparent. Thus, the ghetto is examined from the vantage of an exploited colony in chapters by Norton Long and Tom Hayden. Using this model, Long discusses the difficulty of finding competent "native" leadership in the transition period as whites move to the suburbs. Hayden stresses other considerations derived from the colonial model via Franz Fanon: The importance of struggle and rebellion for stimulating group-consciousness and overcoming the apathy of the "culture of poverty." In contrast to these perspectives, society-wide economic trends toward greater efficiency and profit enhancement, requiring large scale and highly technical production systems, are taken as a given by Martin Rein, who presents a compelling critique of "decolonization" strategies which, in practice, are likely to mean the elaboration of marginal enterprises and not a viable ghetto economy.

A second theme in the volume concerns the performances of different governmental units as agents for social change in urban areas. Charles Adrian reviews the very modest contributions of state governments in past years. Daniel Elazar presents the most interesting essay in the collection, a historical review of federal-local relations with emphasis on the limitations imposed by a particular structural relationship (the federal system) for the development of new levels of government. He stresses the tension between increasing operational efficiency (as represented by pressures toward metropolitan government) and achieving greater community (pressures toward neighborhood groupings).

In fact, the stress on community, in the *gemeinschaft* sense, ties many of these essays together. This theme is more prominent than even the assigned topic of the conference, "the

configuration of political forces as these forces affect social welfare policy-making with respect to the ghettos" (p. 7). I take this as indicative of the relevance of this collection to the issues of 1968, if not 1970.

This book is recommended to sociologists who are professionally concerned with the development of social welfare policy. It is not, however, a contribution to our understanding of the dynamics of ghetto behavior or of the interplay among neighborhood units in stimulating or preventing social change.

*Yiddish in America: Social and Cultural Foundations*, by MILTON DOROSHKIN. Madison, N. J.: Fairleigh Dickinson University Press, 1969. 281 pp. \$10.00.

JONATHAN LASKOWITZ  
*Queens College, Cuny*

A study of ethnic unity and social movement expressed through and produced by a national language and culture, this book focuses on the social and cultural role of the Yiddish language in the community of Eastern European Jewish immigrants. Its methodology aims at revealing the significance of the Yiddish language by analyzing the cultural institutions created by Eastern European Jews. *Yiddish in America* proceeds on the premise that today "American Jews" means Eastern European Jews. Doroshkin's principal sociological theme is that there was a developed and organized community of Jews in the United States during the period of Eastern European immigration (1800-1924), represented by cultural institutions that reflected the needs of the people and with which individual Jews identified and affiliated. Moreover, Yiddish culture was the vehicle and Yiddish language the tool with which Jews shaped and formed their institutions: "This factor differentiated the community of Eastern European Jews from the Sephardic and German communities" (p. 24).

It has become increasingly evident that the dynamics of inter-ethnic group relations and group identifications cannot be viewed as self-contained subsystems. Rather, they exist in interaction with the social, cultural, and personality systems of the larger society. Doroshkin's book historically systematizes the factors which facilitated for Jews both community cohesiveness and subsequent movement into the larger society. He reveals—through an historical-sociological analysis of the development of Yiddish language and literature, the Yiddish press, the Landsmanshaften and fraternal organizations—the state of identification and community character of the American Jews of

the period of Eastern European (Yiddish) immigration. The Jewish immigrants are portrayed as a cultural community whose Yiddish institutions fulfilled a broad range of social functions, not the least of which was unification.

The Eastern European Jewish community in the United States was composed of many elements: different countries and provinces of origin, conflicting social attitudes and temperaments, and various shades of orthodoxy. Yet, in this book they emerge as a community of similar experience and common problems, one that effected a clear identity and left a strong impact on the host culture as a homogenous group. They were united by their large numbers and common needs, and the Yiddish language and the Yiddish cultural institutions that the language gave rise to one seen as the primary cultural mechanism for their unity.

This is a perceptive historical-sociological account of language as a social and cultural product serving not only to define, express, and transmit culture, but also as a mechanism for the creation of ethnic unity and subsequent social movement.

*The Tenacity of Prejudice: Anti-Semitism in Contemporary America*, by GERTRUDE J. SELZNICK and STEPHEN STEINBERG. New York: Harper and Row, 1969. 248 pp. \$8.95.

SOLOMON POLL  
*University of New Hampshire*

This is the first extensive analysis of anti-Semitism based on nationally representative data. Selznick and Steinberg are not trying to explain the historical basis of anti-Semitism, nor are they interested in the personality or the psychopathology of the anti-Semite. Their primary aim is, rather, to measure the differential presence of anti-Semitic beliefs in the United States. Their concern is with the anti-Semitism found in the "normal" course of life, i.e., routine anti-Semitism in American society. Their 1,913 informants were asked to answer questions that dealt with traditional negative beliefs about Jews, i.e., statements characterizing Jews as monied, clannish, prideful, unethical, power-hungry, pushy, and intrusive.

As measured by the authors' Index of Anti-Semitic Belief, they found continued adherence by many to anti-Semitic beliefs and stereotypes. Over one-third of the population is anti-Semitic, and that one-third which is virtually free of anti-Semitic beliefs does not oppose anti-Semitism either. This third strongly defends the right of social clubs to exclude Jews. In other words, today, only 25 years after the holocaust, during a



period of unprecedented economic prosperity, anti-Semitism is found in every segment of American society.

Upon the closer examination of the differential presence of anti-Semitic beliefs, Selznick and Steinberg show that Negroes are more anti-Semitic than whites; people in the rural West and rural South are more anti-Semitic than elsewhere in the country; older people are more anti-Semitic than younger people; foreign born are more anti-Semitic than native born; Protestants, particularly "conservatives," are more anti-Semitic than Catholics; blue-collar workers are more anti-Semitic than white-collar workers; lower classes are more anti-Semitic than upper classes; and less-educated people are strongly and consistently more anti-Semitic than educated people.

Selznick and Steinberg clearly pinpoint the roots of anti-Semitism: it is part of the American culture and individuals acquire it through the normal process of socialization. It should not be astonishing that people are anti-Semitic; it should be perhaps surprising when people are not. There are, however, countervailing forces to anti-Semitism which are also part of the American culture, especially in its official and ideal values of democracy and equality. There is a vast difference between the two traditions. Anti-Semitism and the ideal values are not equally transmitted in the culture. Whereas anti-Semitism is acquired by most individuals through the normal socialization process, the countervailing forces are acquired only by those who are receiving a more sophisticated education in which the beliefs and attitudes of the common culture are scrutinized and evaluated. Because of "lack of cognitive sophistication, existential isolation, and paucity of rewards," say the authors, "the uneducated have little reason to understand or identify with the [ideal] norms of the larger society."

Selznick and Steinberg focus attention on what they call an "ironic balance" of anti-Semitism. Whereas the uneducated non-anti-Semite is susceptible to political anti-Semitism in as much as he would support an anti-Semitic political candidate, the educated non-anti-Semite defends social club discrimination. In fact social discrimination finds its strongest support among the college-educated. Some college graduates try to justify social discrimination as one's freedom of personal association. But so does the bigot!

The authors forcefully warn that anti-Semitism among the college-educated cannot be ignored. The college-educated are not only in key position, but they "constitute a cultural elite whose responsibility is to form and main-

tain the ideal values of society." In 1906 even Hitler sounded like a liberal. "I came to Vienna . . ." he said. "The Jew was still characterized for me by nothing but his religion, and therefore on grounds of human tolerance I maintained my rejection of religious attacks in this case as in others. Consequently the tone of the Viennese anti-Semitic press seemed to me unworthy of the cultural tradition of a great nation" (*Mein Kampf*, p. 52). As it was in Vienna so in the United States: "Social club discrimination," the authors say, "helps to create and maintain the image of Jews as socially undesirable and casts the Jew into the role of the outsider. It reduces the moral authority of the educated to speak out against prejudice, and implicitly legitimates the virulent expressions of anti-Semitism that the educated disavow from themselves."

*Tearoom Trade: Impersonal Sex in Public Places*, by LAUD HUMPHREYS. Chicago, Ill.: Aldine Publishing Co., 1970. 180 pp. \$5.95.

IRA L. REISS  
*University of Minnesota*

My overall reaction to this book is rather mixed. It is a study of male homosexual behavior in public restrooms, conducted in a metropolitan area and centering around the use of public restrooms (called "tearooms" by the people studied) for male homosexual contacts—usually of a mouth-genital (fellatio) variety. The attractiveness of these public restrooms for such trade seems to be their closeness to major transportation routes, the quick and impersonal sexual satisfaction they offer, and their relative isolation. They are frequented usually at the close of the work day by men of all social classes going home from work. This unintended use of a public facility is itself a valuable contribution to the study of the urban scene. Humphreys posed as a participant lookout and thereby gained access to what was going on. The majority of homosexual patrons are not members of the gay subculture, but rather are men who desire to keep their homosexual identification invisible. The sexual interaction occurs in almost complete silence through an accepted set of symbols. The interested male must first display an erection before he will be accepted as one who belongs. This cautiousness helps to avoid police arrests, but is of course no guarantee against entrapment.

The original sample was of 134 license numbers from those cars parked more than 15 minutes in front of 10 public restrooms in four parts of the metropolitan area. The fact

that the men who drove these autos participated in homosexual behavior in the public restrooms was verified by Humphreys' observations. The final sample consisted of 100 participants, of whom 50 were eventually interviewed. However, the bulk of the analysis of homosexual behavior was based upon 50 systematic observations made in the tearooms themselves. Humphreys had intensive interviews with 12 participants. He sought to interview the basic sample of 100 individuals under the pretence that they were selected in a social health survey. He waited at least a year after seeing the men in the tearoom before interviewing them, so as not to be recognized. The fifty interviews were completed in this way, and a control sample of 50 men was randomly drawn from the overall social health survey at the same time and matched on certain key traits such as occupation, race, area of residence, and marital status. Surely one can raise questions about the sample of 50 "deviants" eventually arrived at, but Humphreys adds to this information with his more elaborate length of experience and his 50 systematic observations in the tearooms. One can thus judge the work more as an impressionistic, participant-observation type of research.

Humphreys agrees with Evelyn Hooker regarding the ease with which men seem to shift from the so-called active to the passive roles. It seems that age is a key factor, with the young men as "insertors" and the older men more likely to be insertees. Men shift roles, although Humphreys himself states that many of them face a crisis when they age and must change to the insertee role. The behavior in the tearoom is seen as noncoercive and non-committal. The homosexual-pandering that one notes in certain city streets is not encountered in these tearooms. From the description of the sexual encounter in Chapter 4, one is left with the impression that Humphreys exaggerates the importance of each move the participants make. He describes every encounter as consisting of the "basic steps of positioning, signaling, contracting, and payoff . . . no two of them are alike" (p. 78). Of course, there are subtleties in tearoom encounters where men show erections, play with themselves, and move into insertor or insertee positions in the stalls; but it may be that Humphreys has exaggerated the importance of specific features which could be played down in the interest of simplicity. There is also excessive use of jargon, e.g., he speaks on p. 79 of the "interaction membrane" and of its "penetration" by outsiders and of "reactive purposes" and "proactive measures." Technical terms are fine when they aid our understanding; when such terms merely add

another layer of complexity, they become needless jargon. One must judge terms by the use they serve for a study, and Humphreys' use of jargon doesn't seem to add to the reader's understanding.

Humphreys presents evidence of the futility of police raids on tearooms and of the frequent use of blackmail and payoffs by the police. A good deal of the book focuses on the need for new attitudes toward homosexuality and on the pointless harassment by police. The large amount of space utilized for this detracts from effort that could have been devoted to theoretical relevance. The book is heavily moralistic—in this instance on the side of tolerance for tearoom activities. For example, Humphreys states on p. 163: "if there is any threat to society from tearoom action, my data do not indicate it. The only harmful effects . . . result from police activity." On p. 165 he speaks of "healthy potential" of some associations and on p. 164 of the "dangers" of condemning covert deviants and of the "welfare of individuals." I am not one who would argue that such moral implications should be ignored, but I would argue that one should recognize the tentative basis upon which one makes moral assumptions. Humphreys seems to be taking the naive position that he has demonstrated the correctness of his moral position. A more careful analysis of the moral issues is surely needed. The strong polemical position he takes detracts from an overall understanding of the situation.

Humphreys classified his 50 interviewees in the deviant sample into four subtypes: two subtypes which include 31 of the 50 cases are composed of men who want to keep their homosexual behavior completely separate from the rest of their lives—one subtype of an experimentalist nature, and one subtype of men who belong to the gay subculture. One interesting finding is that the secret homosexuals seem to be rather conservative in other areas of life. This compartmentalization of deviance is analogous to Bartell's findings on the political conservatism of his Chicago suburb mateswappers. The wide range of homosexual subtypes fits well with Michael Schofield's careful study of London homosexuals. One wonders why Schofield's work is not even cited by Humphreys. In fact, one is rather disappointed at the low level of analysis of notions regarding causes of homosexual behavior, such as types of father and mother roles. Humphreys does give some data on this in Chapter 6, but systematic comparisons of his deviant and control samples are largely lacking. Perhaps the small number of cases prevented this; but, in any case, one is left with the feeling that the careful

matching of control and deviants, the random and other sampling techniques, were not really of much utility. This is basically a participant observation study; and, as such, careful comparisons cannot easily be made. We are thereby left very much on the descriptive and impressionistic level, and little is accomplished on the systematic explanatory level. The author does go beyond description but basically in order to show the relevance for changing legal or police codes and not very far in terms of sociological theory.

When, in Chapter 8, Humphreys does get involved in causes, he uses speculations not based on his data. For example, he speaks of the importance of a disinterested or threatening father for the growth of homosexual behavior or the sexual isolation of a prep school as a factor in homosexual behavior. These ideas are not tested in his data, although they are common ideas in the works of men like Irving Bieber and others. He speaks of the old 20-minute houses of prostitution and argues that if they were still available many of these men would visit them instead of the tearooms, for they are simply seeking sexual satisfaction in a quick and non-committing fashion. On p. 115 he states: "I find no indication that these men seek homosexual contact as such; rather, they want a form of orgasm-producing action that is less lonely than masturbation and less involving than a love relationship." I view this as a rather presumptuous conclusion. First, no evidence from his data is offered to support this. In addition, he has quoted his male respondents as viewing other men as physically attractive in many ways. Why aren't these men visiting the "known" loose women who could offer the same orgasmic release? More importantly, in terms of this review, why didn't Humphreys ask these men and his control sample exactly this type of question? Instead of evidence from his interviews, one finds opinions impressionistically arrived at and untested and frequent reference to other people's impressions. Little in the way of evidence from other studies is cited, and both Schofield and Bieber have data that could have been compared, further tested, and refined.

Overall, I found the book interesting and descriptively informative. I learned how the tearoom operates, and this is valuable for sociological understanding of urban America and potentially for sex-role analysis. However, I feel I did not learn very much about the causes of this situation nor the motives of these men. These are topics discussed at some length, but the data are not well utilized for such ends. One is left with the impression that the heavily analyzed interaction in tearooms and the careful

matching of samples were epiphenomena. The key contribution of this book is descriptive and impressionistic, not explanatory.

*Nudist Society: An Authoritative, Complete Study of Nudism in America*, by WILLIAM E. HARTMAN, MARILYN FITHIAN, and DONALD JOHNSON. New York: Crown Publishers, 1970. 432 pp. \$6.95.

MARTIN S. WEINBERG  
*Indiana University*

The audience for most publishing sociologists is other members of the profession, but there are also sociologists who write primarily for the public. Hartman and Fithian fall into this latter category (Johnson, the third author, is a nudist who has published extensively on nudism). Due to the poor quality, polemicism, and limited coverage of other books which pertain to nudism, the work of the present authors is probably the best and most complete that is currently available on the subject. Nonetheless, whether or not the book really qualifies for review in a professional sociological journal is questionable. The book begins with a "scholarly" prolegomenon: "Prior to actual camp visits, hypotheses and probability statements were drawn up. Following preliminary analysis of the original data, additional hypotheses and probability statements evolved that gave direction in the development of further questionnaires." It does not proceed on this level, however. The hypotheses and probability statements do not appear, nor is a copy of the questionnaires presented. Some sociological jargon can be found—e.g., primary group, significant others (which is applied across the board to parents, children, aunts, and uncles)—but no sociological perspective is employed which might have provided valuable insights into nudist phenomena, nor do the authors make any attempt to show what the study of nudism contributes to sociological understanding *per se*. In short, no section of the book could qualify as an article in a professional journal of social science. Only the discussion of nude marathon sessions in the chapter on "Nudism as a Therapeutic Community" begins to reach that level of discourse.

This is disappointing when one considers the extensive fieldwork and approximately 2,000 questionnaire-respondents who are represented in the study. Much more is expected of professional sociologists than appears in this book, even if their work is directed chiefly to the lay public. The lack of a theoretical orientation

precludes systematic insights into any number of basic social phenomena: e.g., social organization, social movements and social change, the study of "politics" and conflict in marginal groups, "minority group" behavior, socialization, symbolic interaction, the study of deviance. With regard to the data analysis, the lack of a theoretical perspective also limits any inquiry into social process, covariation, etc. The "hard" data presented are all marginals. There are ample quotations from questionnaire respondents, but no systematic inclusion of the field data.

As a "popular" book on nudism it is quite adequate, however. There is a standard summary of the history of nudism; a review of studies of nudism; an enumeration of the social, psychological, and sexual characteristics of nudists; evaluations of nudist marriages and the benefits of nudism (for children, marital relations, etc.); a consideration of why nudists say they became nudists and why some drop out or are dissatisfied; the mass media's treatment of nudism; nudism and the law; non-nudist's reactions to nudism; and nudism as a therapeutic community. The book ends with certain personal impressions of the authors, and a statement about the future of nudism and nudist research.

By way of conclusion, it would appear that some persons outside of social science make more significant contributions to social science than do so-called professionals, and the certain social scientists, such as the present authors, provide popular works of higher quality than do journalists. Although one wishes for more from one's colleagues, such contributions are not entirely without merit.

*Social Stratification and Deviant Behavior*,  
by JOHN P. HEWITT. New York: Random  
House, 1970. 176 pp. \$5.95.

DAVID L. DODGE  
*University of Notre Dame*

This slender volume contains a "self-esteem theory" by which Hewitt explicates more fully than heretofore the causal pathway linking social inequality and deviance. His objective is "to develop a theory capable of accounting for diverse empirical findings" (p. 3), but he also scrutinizes closely numerous *theoretical treatises* that link inequality and deviant behavior. The latter exercise is conducted "not to disprove them but rather to indicate the points at which they agree and to demonstrate that, when stripped of elements contradicted by

logic or empirical evidence (where available) they converge in the direction of a self-esteem theory" (p. 80).

Hewitt is concerned with the system state-rate fallacy that lurks in many sociological works; he seeks to make explicit use of personality theory in conjunction with sociological theory and data to construct a more adequate sociological analysis of social inequality and deviance. In sociological theories that involve the system state-rate fallacy, one is pressed somehow to bring "the individual" into the picture so that an adequate causal sequence can be effected articulating some property or process with ensuing rates/careers of some behavior (or some other property or process of the system). When men are brought into the scheme of things, the property or process of the social system impinges on them so that they in turn experience a crucial psychological state subsequently manifested in common behavioral adjustments (rates or careers), given certain common conjunctive situational contingencies. The psychological state experienced by the individual becomes a necessary, intervening causal mechanism between the system state and the rates.

Deviant behavior is linked with social stratification as a function of the social inequality generated by the stratification process. However, adequacy of self-esteem is also socially governed (at least initially) by the stratification process and its inherent social inequality. Two auxiliary themes are (1) the premise that "deviant or conforming behavior is always a product of the motivations and capabilities of individuals and of the situations in which they find themselves" (p. 14), and (2) the assumption that the mechanism of self-esteem is the major, though not the sole, social-psychological variable intervening between the social structure and its behavioral consequences.

Despite its brevity, the book is so systematically and rigorously written that comprehensive summarization is impossible. In Chapters I and II Hewitt grapples with the problem of defining deviant behavior and distinguishes three mechanisms of behavioral motivation and control. Deviant behavior is that behavior which violates one or more of these three mechanisms, namely, the normative, coercive, or situational demands. One may or may not "elect" to conform, depending on his relative commitment to social norms, the coercive expectations of some power group, or some combination of psychological predispositions and external events in a situation over which he has little if any control. The situational demands mechanism has special appeal to Hewitt, for

he utilizes it to consider the problem of "psychic survival," later translated into the problem of creating and maintaining an adequate level of self-esteem. At times Hewitt confuses the problem of defining what deviant behavior is with the problem of what motivates and causes deviant behavior to occur. Most assuredly, to explain deviant behavior one must first define it; but the definition and explanation(s) are not synonymous. Nonetheless, Hewitt's creative examination of deviance should appeal greatly to those enamored with the labeling approach.

In Hewitt's scheme the process of socialization is equally important and not divorced from the stratification process. It is within the socialization process that a mutual interdependence between the self and sets of motivations and normative commitments arise. A "normal" socialization creates an adequate level of self-esteem and concomitantly a commitment to norms. In "non-normal" socialization, inadequate levels of self-esteem are associated with lack of commitment to norms, and subsequently with deviant behavior. Extenuating situations which influence the bargaining process for prestige are a part of the socialization process and may enhance or attenuate levels of self-esteem. People with low self-esteem and lack of commitment to norms and roles participate less effectively in the prestige-bargaining context, compounding their problems of self-esteem. Consequently, "deviant behavior occurs when social arrangements interfere with 'this normal' process of socialization . . . the development and maintenance of adequate self-esteem are inhibited. Our fundamental thesis is that any society contains at least some social arrangements in which participation is either normative or unavoidable, yet that so inhibit development of adequate self-esteem as to produce poorly socialized people" (p. 41).

Chapter III contains a self-esteem theory of delinquency; critical qualifications of major delinquency theories and examinations of numerous empirical observations of delinquency and associated factors in light of the self-esteem theory are cogently and amply provided. Chapters IV and V further elaborate the relationship between deviant behavior and self-esteem among adult members of the lower strata and juvenile and adult members of the middle social strata. Chapter VI considers the social implications of inequality and deviance.

It would certainly take a much larger volume than this to chart systematically the linkages of the self-esteem theory and the variegated forms of traditional deviance. It would have been preferable had Hewitt titled his work *Social Stratification and Delinquency*, for his

treatment is largely restricted to juvenile delinquency, though he gives considerable attention to mental illness and offers cursory comments on Black militance and citizenship role involvement. In any case, I have strong reservations regarding the request that, for purposes of expediency, the phenomenon of mental illness be considered as representative of deviance in general. It is surprising that Hewitt gave only two pages to drug addiction and altogether neglected suicide and alcoholism. Finally, one is left wondering how the etiology of different forms of sex deviation might be more fully explicated by the self-esteem theory. However, the self-esteem theory of delinquency is provocative and clearly warrants future theoretical formulations and empirical research regarding other forms of deviance.

*Crime and the Legal Process*, by WILLIAM J. CHAMBLISS. New York: McGraw-Hill Book Co., 1969. 447 pp. Clothbound, \$7.95. Paperbound, \$5.95.

ROBERT H. HUGHES  
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This book includes twenty-five articles reporting empirical studies of the legal system which are divided into three major sections reflecting the author's assessment of the three major questions of concern to the sociology of criminal law: (1) the emergence of legal norms; (2) the administration of criminal law; and, (3) the impact of legal sanctions. The second section—administration—is subdivided into three parts: arrest, prosecution, and trial and sentencing. The author assumes the responsibility for reducing the "blindness of facts without theory" by providing a theoretical framework in the form of an introduction to each section and subsection of the book.

The criteria for the inclusion of articles give an indication of the general orientation and scope of the volume. An effort was made to incorporate empirical studies from behavioral science disciplines other than sociology (including legal scholarship) when appropriate. Theoretical statements about the legal system were omitted in favor of empirical studies. Studies dealing with the legal profession *qua* profession were also omitted; and only a few studies of legal systems other than those predominately Anglo-American were included.

The "theoretical introduction" to Part I is more than adequate, cogently summarizing the areas of debate, areas of weakness, and those marked by some consensus in the explanation of the relationship of law and society.

This same theoretical introduction, however, raises some question regarding the representativeness of the articles included. "Two general explanations which compete with one another among legal scholars and social scientists as models to explain changes in the legal order deserve special attention: these may be referred to as the 'value-expression' and the 'interest-group' hypotheses—however, in *all* the studies incorporated in this volume the role of interest groups in determining the content of legal norms is paramount" (p. 8, emphasis added). The perception of the law as being concerned with, based on, or embodying "just ends" is fundamental to much of the present-day struggle in the area of civil rights and civil liberties and crucial to "conscientious objection" in terms of the "Nuremberg Principle." It is unfortunate not only that studies of this aspect of legal norms are omitted, but also that they do not figure more compellingly in the theoretical analysis.

In the general introduction to Part II, Chambliss summarizes his theoretical perspective regarding the administration of criminal law: "The activities of police and law enforcement agencies . . . are designed to have the consequences of maximizing organization rewards and minimizing organizational strain. This general principle is reflected in the fact that in the administration of criminal law, *those persons are arrested, tried, and sentenced who can offer the fewest rewards for nonenforcement of the laws and who can be processed without creating any undue strain for the organizations* which comprise the legal system. The remainder of this introduction and the research studies which follow are an elaboration of this general principle" (pp. 84-85). There we have it—the law enforcement bureaucracy "rat" in the socio-economic "maze." It does not seem as appropriate at this point to argue the validity, usefulness for prediction, or general utility of this major premise, as to consider briefly the extent to which the empirical studies included for its support and elucidation do in fact support and elucidate it.

In none of the three subsections do the articles lend much support to the theoretical premise, nor does the theoretical discussion do much to weave the hypotheses and tentative conclusions of the studies into a coherently patterned fabric. For example, in the subsection dealing with the police and arrest, Bittner's "Police on Skid-row" provides the test case, for he is purposely studying a domain of police practice that "does not seem subject to any system of external control," (p. 153)—an area of police practice falling outside the theoretical premise and hence not explainable by it. The

general conclusion of this study is, in turn, consistent with those found in other articles in this subsection, especially those of Westley ("Violence and the Police") and Piliavin and Briar ("Police Encounters with Juveniles"). These three articles concur in suggesting that police practice is a response to the meanings which they perceive in a consistent manner to inhere in complex and persistent (to them) social situations. In no way is the reported and observed behavior of police officers explainable as an outcome of directives or procedures of a police department interested in maximizing its rewards. Skolnick's "The Narcotics Enforcement Pattern" illustrates the importance of time, access to resources, and opportunities as crucial factors in determining enforcement practices, again a finding at best unrelated to the theoretical explanation offered.

I want here to emphasize that my argument is neither with the theory nor the empirical studies, but rather with the discrepancy between the two. The articles in Part II of this volume are well selected, interesting, well organized, and important contributions to the sociology of criminal law. They include studies based on quantitative methodology, participant observation, scholarship, and ethnomethodology, and provide a rich source of "insightful" and "factual" statements regarding the operation of the machinery of legal processing.

Happily, this problem of divergence between data and theory is sharply reduced in Part III, where the theoretical explanation moves from the data of Chambliss' "The Impact of Punishment on Compliance with Parking Regulations" to a general typology of crime and motivation which clarifies some important issues in the field of corrections and law enforcement and which in turn is further clarified by remaining articles in this subsection. The theoretical introduction to this section of the volume also contains succinct appraisals of labelling theory, the free will—determinism controversy, and the role of violence in the "national character." This section could have been strengthened by the inclusion of psychological studies of compliance, conformity, and alienation, and sociological studies of political crime and of anomie and behavior.

This seems to be an important book, for it clearly defines an area of interest to sociology which for too long has been ignored or studied in an unsystematic and inconsistent fashion. The organization of the book is clear and concise, and this in itself helps to clarify a number of issues. The effort to provide an interdisciplinary perspective on the study of criminal law and criminal behavior is equally overdue and commendable, although the dominant

thrust of the book remains sociological. The articles included are of very high quality, both in terms of their academic-methodological rigor and the extent to which they clearly and precisely reveal the social processes through which our society defines and disposes of undesirable persons.

The Epilogue opens with T. S. Eliot's "Between the idea and the reality/Falls the Shadow." This volume provides a rather precise picture of that reality, and few who see it clearly can be much pleased. Chambliss remarks that this book is a hesitant and tentative beginning toward illumination of the shadow, an effort to help laymen, students, and professionals to infuse it with clearly perceived ideals which may then be proposed as alternatives. It is, of course, impossible for any one volume to deal completely with the wide range of approaches to the problems in this field. By its scope, reach, and direction, this particular book does accomplish its major objective: to make a strong beginning toward reaching an understanding of how the shadow of process demeans the idea of justice and to seek, in the dialectic between the idea and reality, a more effective manner of insuring justice.

*Crimes Against Bureaucracy*, by ERWIN O. SMIGEL and H. LAURENCE ROSS. New York: Van Nostrand Reinhold Co., 1970. 142 pp. Paperbound. \$2.45.

ELMER H. JOHNSON  
*Southern Illinois University*

Seven articles were selected to address students and professionals in criminology, the sociology of organizations, and the sociology of law. In their valuable Introduction, the editors state their perception of the contributions of this small volume. The development of criminological theory would be incomplete without consideration of crimes against bureaucracy. In addition to alienation and depersonalization, criminal exploitation is a vital aspect of problematic relationship between individuals and organizations. Crimes against bureaucracies are particularly germane to the study of the informal procedures which produce case outcomes significantly different from what strict interpretation of the formal law would lead one to expect. Finally, crimes against bureaucracies provide data on the legitimation of crime as a prerequisite to its perception in a way similar to the application of norms to conventional behavior.

Two articles by Smigel deal with public attitudes toward stealing from bureaucracies.

Donald Horning and Donald Cressey, respectively, indicate some of the ways in which those who steal from bureaucracies manage to avoid self-definition as criminals. Harold Groves uses discrepancies between rents collected and rents reported by landlords to taxing authorities as data on the pertinence of visibility of the offense to this class of transgression. An excerpt from the study of Mary Owen Cameron discusses how a department store deals with prevention, detection, and disposition of the shoplifter. Gerald Robin analyzes how a department store handles the dishonest employee.

The editors' Introduction is particularly cogent for delineating concepts and reviewing the pertinent literature. It raises questions likely to make this book an effective teaching tool in advancing student understanding of bureaucracy, the relationship between attitudes and the imposition of the penalties of the criminal law, and the effect of the characteristics of a particular class of offenses on law enforcement. The very choice of the topic provides a freshness in approach to white-collar crime.

The weaknesses are largely endemic of the book of readings as a type of publication, aggravated by the volume's brevity. Four items written originally for specialized journals are necessarily characterized by a terseness in expression and inclusion of methodological explanations which are inconsistent with the new purposes the editors would have the articles serve. Granted that a segment of the book's intended market overlaps the subscribers of these journals, we speculate that these characteristics impede communication with the largest portion of the intended market for the book. Written originally for purposes not directly relevant to the editors' stated objectives, the articles drift too frequently and leave the reader on his own resources in relating the evidence and conclusions to the book's central theme.

The book is successful in capturing a reasonable portion of the criminologist's interest in offenses against bureaucracies, but is less adequate as a resource for demonstrating implications for the sociology of organization and the sociology of law.

*Crime in the City*, edited by DANIEL GLASER. New York: Harper and Row, 1970. 308 pp. Paperbound. \$4.95.

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*College of William and Mary*

Glaser, focussing upon metropolitan lawlessness, has organized the 23 papers comprising

this volume into five sections and written very brief introductions to each section. Many of the papers first appeared in journals ordinarily available to social scientists; two result from the President's Commission on law Enforcement and Administration of Justice; and four are published here for the first time. Although several selections have been abbreviated, Glaser has apparently retained any continuity present in the original.

Part I includes three papers on the complexities associated with the rapid growth that has characterized American cities; the most inspiring of these is the selection by Marshall Clinard on the patterns, social organization, and functions of slum life.

Part II treats the structural distribution of crime and delinquency, particularly by socioeconomic status and urban location. Predictably, Reiss and Rhodes' study of Nashville delinquents is included, as are the well-known studies by Clark and Wenninger, Sarah Boggs, and Gordon (a major overhaul of the empirical evidence for, and conclusions traditionally drawn concerning, socioeconomic status and delinquency). Other selections include a summary of relationships between urbanization and crime in Denmark (Karl Christiansen), a paper on crime and relative deprivation (Paul Eberts and Kent Schwirian), and a cohort analysis of four delinquency correlates—school retardation, apparent truancy, guardian's occupation, and father's presence or absence—by Lee Robins and Shirley Hill.

Part III offers the most readable and stimulating papers of the collection, in part because three are especially written for the book. But there is also an inherent attractiveness in the *theme* of urban social processes which are productive of, or responsive to, deviant behavior. The new papers by Gerald Suttles (on public housing and project dwellers), Michael Lewis (on "hustling" and organized gang behavior), and Glaser (on urban criminal violence) are tightly-written and informative; the other selections are Toby's depiction of the youthful hoodlum's repudiation of a "stake in conformity," Werthman's explication of the moral career of the lower-class delinquent gang member, and David and Sophy Burnham's portrayal of the behavioral styles of "normals" in a preponderantly law-violating block of Spanish Harlem.

Part IV's selections illustrate some problems encountered by urban police, courts, and jails. Robert Derbyshire's lucidly written paper on police adaptations to rapid urban change is sensitive to the strains generated from conflicting demands on law enforcement facilities by political and community sentiment. It is followed by a selection from Jane Jacobs' *The*

*Death and Life of Great American Cities* and by Abraham Blumberg's contention that judges in metropolitan courts become directors of a legal ritual which all but repudiates the adversary system. The fourth paper, an original piece by Glaser, draws attention to the urban dumping grounds that are euphemistically termed "detention facilities."

Part V highlights the responses that must be made by cities to lawless conduct. John Bollens and Henry Schmandt predict that wealth and political power will become more decentralized as SMSA's increase in scale. The reader is to infer that crime and other pathologies historically concentrated in inner cities will be redistributed accordingly. Glaser's paper on prisons of the future argues that, even if current goals of correctional systems are perpetuated, institutions will be geographically relocated, reduced in size, and increasingly oriented to a "treatment" ethos. The final selection summarizes recommendations for coping with crime made by the President's Crime Commission.

Glaser's editorial skill has propagated a diverse yet cohesive body of substantive findings on urban criminal and delinquent behavior; it offers compelling testimony to the perception held by the Chicago School in the 1920s of "the city tending to debility and decay."

*The Making of a Criminal: A Comparative Study of Two Delinquency Areas*, by PATRICIA ELTON MAYO. New York: Pica Press, 1970. 147 pp. \$6.50.

RUTH SHONLE CAVAN  
Northern Illinois University

This book is a study of two high delinquency housing estates (government housing projects): one in Marseille, France, and the other in Wrexham, Wales. Mayo formerly was a consultant sociologist for various British industries, labor groups, and government agencies; later she turned her attention to research in the social background of crime.

Mayo states that this book does not attempt to develop theories on crime, but is a description of two communities in which it is not considered reprehensible to commit serious crimes. She has sought to test hypotheses, not clearly stated but implied, and to point out areas in need of additional research. Regardless of her statement of intent, she does start with two theories: (1) that crime is an integral part of society and a social phenomenon as often as the result of emotional disturbance; (2) with some modification, the Cloward-Ohlin



theory that posits the development of either an area of criminal subculture with opportunities for criminal careers or an area in transition, marked by gang violence. Her study provides support for the first and partial support for the second theory.

A small community—Newtown, Wales, with a population of 6,000—offered a contrast to Marseilles and Wrexham. In this area, essentially rural and with law-abiding traditions, youth in a housing project were often involved in crime up to the age of twenty. With entrance into adulthood, they were absorbed into the law-abiding adult community. There seems to be some question whether this area contradicts or supports the theory of the transmission of a subculture, in that the noncriminogenic culture prevails over youth after a temporary age-related foray into crime.

Over a five-year period Mayo assembled data from interviews with prisoners from the housing estates, their families, and officials of social agencies and from various official records. She makes some comparisons of delinquents and nondelinquents within a country, but avoids the pitfall of comparing Wales with France, given the differences in definitions and methods of handling crimes. However, certain similarities may be noted. Thefts and related activities were by far the most frequent offenses of the boys in both Marseille and Wrexham; boys were more delinquent than girls; education was only marginally related to crime; most offenses were committed by two or more boys rather than one individual; and leisure facilities were inadequate. These same generalizations are also applicable to the United States.

Mayo is especially critical of Shaw and his associates for having placed too much theoretical emphasis in the 1920's on the adjustment problems of European immigrants. Delinquency exists in Wales and France in areas inhabited mainly by long-established families and where most other families come from nearby regions; only a low percentage are from other countries. A somewhat comparable situation to that of immigrants might have been noted—the disturbance to residential areas produced by the construction of new housing estates and by the influx of strangers. Nonetheless, the author prefers to emphasize the continuity of delinquent subcultures rather than the assemblage of strangers and the intermixture of cultures. She concludes that no one specific cause accounts for the development of criminogenic communities but that they grow spontaneously under certain interrelated conditions. In addition, she finds delinquency to be not an isolated phenomenon, but a contemporary symptom of alienation, along with industrial and student revolts.

Mayo's factual descriptions are valuable in view of the tendency of criminologists to confine their theories to one culture. But her contribution would have been much greater if she had provided an analysis of the mechanisms whereby the delinquent subculture is converted into individual criminal behavior.

*Juvenile Offenders for a Thousand Years: Selected Readings from Anglo-Saxon Times to 1900*, edited by WILEY B. SANDERS. The University of North Carolina Press, 1970. 453 pp. \$12.50.

NORMAN GOLDNER  
Wayne State University

After reading *Juvenile Offenders for a Thousand Years*, one will be struck by how little things have changed. Limiting himself chiefly to sources from England and the United States, Sanders has located historical material on juvenile misconduct from 688 A.D. to 1900. His stated intent is to "render more accessible rare and valuable books, pamphlets and other data" for college teachers, judges, social workers, social historians, educators, parents, and citizens. Sander's central theme is that "as far back as written records go, children who have broken the law have been treated on the whole more leniently than have adult offenders." (Our misconception that treatment was undifferentiated stems from reports that some juveniles were severely punished, even executed, and that they were regularly imprisoned with adult offenders under the vilest of conditions.) Source materials include legal documents, commission reports, public and private committee investigations, court opinions, news and literary accounts, the writing of reformers, books, essays, and other documents—all dealing primarily with themes related to causes, effects, treatment, conditions of and reform recommendations for juvenile delinquency and dependency.

The most common explanation for delinquency before 1900 was that some lower-class parents abandoned or failed to socialize their children and often even exploited them. (Sanders includes only a few brief allusions to non-lower class delinquency). This resulted in vagrant children whose illegal and immoral behavior was seen as necessitated by their struggle to survive. Parental failures were often attributed to moral turpitude and the genetic transmission of criminal tendencies; psychological and sociological explanations were notably absent from these early attempts to locate the causes of delinquency.

Several selections document attempts in the 19th century to develop special institutions and

programs to separate children from adult offenders, to inculcate them with religious morality, respect for authority, good habits and to teach them a means of earning a living. In time (1894) positive rewards for good behavior were added to the traditional recommendations for punishment. Some of these earlier recommendations to modify the treatment of offenders will seem strikingly contemporary to students of delinquency in the United States. For example, in 1844 a Connecticut survey of juvenile offenders revealed that legal reforms were deemed necessary in part because convictions were difficult to obtain owing to the recognition that sentences were inappropriately severe and held little promise of correcting the offender.

The main and continuing philosophy underlying the treatment of the delinquent, dependent and/or abandoned child was and is *locus parentis*, i.e., that the state would when necessary assume the duties and responsibilities normally held by the family. However, the student of societal reaction to the problem of delinquency will be impressed with what appears to be a long-standing failure of the political system sufficiently to support or finance programs aimed at treatment and reform. For example, as late as 1899, The Juvenile Court Law of Illinois provided for the appointment by the Court of probation officers. But no provision was made for either training or paying them or for facilities to house the children. In the period covered by Sanders, humane and innovative approaches to the handling of juvenile offenders and indigents depended on private philanthropic endeavor. This failure of public responsibility—withstanding public complaint—was noted in 1897 by Oscar Wilde, who observed that, if a parent treated the child as the state did in *locus parentis*, that parent would be severely punished by the state. Of course, the inadequate treatment given the delinquent and others who are wards of the public continues unabated.

While it is impossible to know whether Sanders has adequately sampled the available literature, his book is a rich source of illustrative material providing a valuable historical perspective. However, his chronological-historical approach may prove somewhat disconcerting to the reader because so many of the selections cover the same points. Also, a much-needed subject index was not provided, so that the reader is required to search laboriously for specific points of interest. This book will be of little use to those who seek theoretical insights, but it will demonstrate how little we have advanced in our understanding and treatment of juvenile delinquency in the last one thousand years. Beginning with Calvin's catechism in 1556, the reader will note the similarity of causal and

treatment approaches to deviant youth to those still imbedded in our folkways and mores, and he may wonder when, if ever, an appropriate social policy toward youthful offenders will be implemented.

*The Varieties of Delinquent Experience*, by  
BERNARD ROSENBERG and HARRY SILVERSTEIN  
Waltham, Mass.: Blaisdell Publishing Co.,  
1969. 165 pp. Paperbound. \$2.75.

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The research reported in this book was designed to challenge basic sociological theories about the etiology of juvenile delinquency. Anomie theory and its relationship to delinquency is questioned, as is the contribution of the "culture of poverty" to an explanation of delinquency. The basic research questions include the following: Can delinquency be accounted for in terms of the larger social structure in which it is found; and are these strategic cultural differences between urban groups characterized by similar economic circumstances and, if so, are they determinants of delinquency?

Lower-class areas in three cities—Chicago, New York, and Washington—were selected for analysis. Ethnic differences among the three communities were important to the analysis. In New York most of the people in the community studied were Puerto Ricans, in Washington most were Negroes, and in Chicago most were lower-class whites. Blocks from the three lower-class communities were selected. According to the authors "a social block focuses attention on a small, populous, constructed, lively, and rather mysterious area" (p. 21). In addition to demographic data from each community (e.g., crime records), subjective evaluations of the blocks also were obtained from samples of youth. Three categories of subjects were involved: one group without criminal records, one with records but no convictions, and one with both arrest and conviction records. A random sample was then chosen from each of these three categories, producing total N's of 52 in New York, 41 in Chicago, and 40 in Washington. These subjects were interviewed in depth.

There are differences among the poor in the three communities: "If our observations are at all accurate, cross-cultural variations among the poor are marked—and not only in delinquency patterns but in many other social activities as well. From a sociological point of view, failure to recognize the validity of this proposition results in treating all low-

income people as if they were a single social unit, which in turn produces frustration and despair" (p. 164). Some of the differences were (1) different patterns of delinquency among the three communities (e.g., offenses such as truancy were more common in New York and assault was much more prevalent in Washington); (2) "spiritism" or voodoo was prevalent in the New York community but not in the others; (3) the police were very visible in New York but not in Washington; and (4) sexual behavior, patterns of fighting and collective violence, and stealing also differed among the communities. There were no findings indicating a relationship between high aspiration and delinquency. As other research has shown, lower-class youth do not necessarily incorporate the American success goals and many blame themselves for failure to be mobile. The findings in this study were congruent generally with similar research.

The conclusions rely heavily on subjective evaluations by the subjects. It would have been helpful had the authors provided statistics on how many subjects had certain opinions rather than making statements such as "Girls in Chicago stress early marriage as a female adjustment" (p. 75). The authors chose to interview in depth, so their sample size was limited. The study would have been strengthened had some depth been sacrificed and more subjects included. It is difficult to have much faith in a comparison of three large urban communities when there are no more than 52 subjects from each community. However, one of the outcomes of research should be the development of new theories; and this is the major contribution of this research. The authors develop a general theory of anomie independent of socio-economic class, but "from the moral currents of contemporary social development" (p. 165). Their theory deserves further interest and research, and their book will be useful to both professionals and students. However, a serious impairment and irritation is the absence of an author-and-subject index.

*Delinquent Behavior*, by DON C. GIBBONS.  
Englewood Cliffs, N.J.: Prentice-Hall, 1970.  
276 pp. \$7.95.

HAROLD R. HUNTER  
*University of Florida*

This book is an excellent defense of the sociological approach to delinquent behavior. It contains a number of unique contributions, including discussions of working-class delinquency, hidden delinquency, the dangers of labeling delinquents

as such, and a rather idealistic plea for precision in delinquency causation research. The reader will derive much from the book if he can ignore the author's rather inconsiderate attacks on approaches that differ from his own. Many of the statements contained in the book are rather dogmatic and controversial, as will be clear from the following examples:

(1) Gibbons rejects the prevailing belief that delinquency is based on an ever-changing collection of factors. He contends that delinquency causation must isolate and define "some large but finite set of factors which bear an invariant relationship to delinquency" (p. 62).

(2) He further suggests that the gathering of those factors must be based on a theory, defined as "a systematically related set of statements including some lawlike generalizations that is empirically testable" (p. 65).

(3) In speaking of psychoanalytic approaches to delinquency, he states "we need not devote much space to psychoanalytic writings on delinquency. For one thing, psychoanalytic theories involve contentions about the workings of instinctual sources of psychic energy, and these are incapable of empirical verification" (p. 77). "Second, psychoanalytic arguments about law-breaking are relatively unfashionable at present" (p. 77).

(4) In regard to the psychological approach, he states: "Most juvenile offenders are relatively normal youths in terms of personality structure, in that they do not exhibit aberrant motives, deep-seated psychological tensions, or other marks of personality disturbance" (p. 98).

(5) In regard to the biological approach, he states: "Almost without exception, the biological theories that have been advanced have been scientifically naive, while the research that has been conducted has been flawed in one way or another" (p. 75).

(6) The sociological approach is left with a clear field: "Perhaps the search for the main-springs of juvenile misconduct should be directed at elements of the social order, such as social class patterns, the educational system, urban social organizations, or elements of middle-class life styles. This perspective places accent upon the sociological view, which supposes that behavior as widespread as delinquency probably develops out of conditions of social structure" (p. 100).

(7) Gibbons disdains the use of control and experimental groups: "Similarly nearly all would agree that there must be something radically different about gang delinquents, sex offenders, petty 'hidden' offenders, juvenile arsonists, hyper-aggressive delinquents, and the other lawbreakers who are lumped together

under the term 'delinquent.' It does not make sense to collect such a diverse bunch of youths and to compare them with 'nondelinquents,' in the search for etiological variables" (p. 93).

(8) He then contends that typologies based on common sense are better research tools than are experimental and control groups: "There is a readily apparent common sense basis for the typological orientation in criminology" (p. 93). "In general, these presuppositions argue that relatively discrete patterns among offenders can be observed when these persons are classified in terms of certain variables or dimensions. The dimensions in question have to do with offense behavior and with social-psychological characteristics of offenders" (p. 95).

(9) Gibbons' discussion of working-class delinquency is perhaps his most valuable chapter. He emphasizes two approaches—ecological distribution and delinquent subcultures. The ecological approach emphasizes inadequate socialization, institutional decay, and the presence of illegitimate means in the areas of high delinquency. In his discussion of delinquent subcultures class conflict, lack of opportunity, and the presence of a delinquent subculture are presented.

(10) His discussions of middle-class delinquency and female delinquency are rather traditional, and his handling of the aggressive delinquent is one-sided. He has offered only one viewpoint even of the research presented.

(11) He attacks the training schools for boys as treatment facilities and suggests that those institutions create deviant behavior that may not have otherwise re-occurred.

(12) The summary of conclusions contains statements which are controversial but are certainly steps toward the research precision which the author seeks.

*Freedom and Justice Within Walls: The Bristol Prison Experiment*, by F. E. EMERY. London: Tavistock Publications (Distributed in the U.S.A. by Barnes and Noble, New York), 1970. 119 pp. \$5.00.

JOHN R. HEPBURN  
University of Iowa

Conflict and accommodation processes in inmate-custodian interaction are common to prisons. When the inmates perceive the deprivation of incarceration and the degradation of status as unjust and unwarranted, a state of emotional tension arises. This monograph presents a field experiment in the reduction of such tension within a prison.

Bristol Prison is a Local Prison with a high rate of inmate turnover and an average inmate population of 360 men. Based on an array of theoretical propositions and the "favorable" results at other Local Prisons, the inmates at Bristol "were given greater opportunities to associate with each other in their leisure hours and an attempt was made to measure the effects of this change on the social atmosphere of the prison (p. xiii)." With the unrestricted opportunity of leisure-time association (whereas previously there had been no such association permitted), the following changes are hypothesized: (1) a reduction in the general level of tension; (2) an abstention by the inmates from use of the greater freedom to abuse the regulations or one another; (3) a reduction in the tension and an increase in the stability of inmate-custodian relations; (4) an increase in the feasibility and necessity of managing inmate-custodian relations.

The author gives consideration to those factors—such as the "Hawthorne Effect" and a decline in staff morale—capable of yielding a spurious relationship, but such possibilities are discounted. Data from inmates and custodians are collected before and, allowing an eight-week transition period, after the innovation of association. The findings which support the hypotheses are presented in a concise, elementary manner, largely with the use of percents in 2 x 2 tables, graphic representations, or differences in means tests. Lastly, the author discusses the problems to be incurred in supporting the system of increased association.

The research suffers severe methodological limitations which question the validity of the results. Emery's sample size is governed by the small number of guards employed (34) and his effort to draw from only the 101 inmates likely to be incarcerated during the length of the proposed measurement period. Apparently no thought was given to increasing the sample size or designing a panel study with a larger inmate sample available. The small samples (20 staff and 30 inmates) make it impossible to introduce controls for age, offense, prior record, length of incarceration, etc. A second shortcoming centers on the operational definition of the major concept. Assuming that tension is manifested in the frequency and intensity of conflict, the number and type of disciplinary incidents serve as an index of tension. No attempt is made to consider informal forms of conflict (e.g., ridicule and slow-downs) or to ascertain the source or level of tension with a scale for the measurement of such attitudes. While questions of attitude toward the association period were asked of inmates and staff, the interview schedule is not provided

for examination. Furthermore, on occasion one encounters statements of fact without the presentation of substantiating data.

It is clear that this research was conducted at the invitation of the Prison Commission; its pragmatic concern is designed to serve as a "guide to administrative action" (p. 90). However, action based on the reported experiment would be premature; a refined methodology, improved measurement techniques, and additional data are necessary. The work would be of wider significance had the author explored such issues as the effect of association on socialization to the inmate code, rehabilitation efforts, and recidivism rates. The value of this monograph is to raise, rather than explore and answer, issues concerned with the effect of greater association within prison walls.

*Social Interaction*, by MICHAEL ARGYLE. New York: Atherton Press, 1969. 504 pp. \$11.50.

FRANK D. BEAN  
University of Texas

This amplification of Argyle's earlier work, *The Psychology of Interpersonal Behavior*, does indeed constitute "something of a 'new look' in the study of social behavior" (p. 14). Argyle's approach is novel in two respects. First, drawing upon the findings and hypotheses of human ethology, he delineates the partially instinctual and evolutionary bases of human social interaction by noting parallels between human and non-human social behavior. Argyle also gives emphasis to the cultural roots of interaction by suggesting that innate tendencies in man require social experiences for their complete development. Second, Argyle devotes considerable attention to the only recently studied nonverbal aspects of social interaction and their close coordination with verbal aspects, the former serving not only as meaningful units of social behavior in their own right but also as cues which guide the course of verbal interaction. Consistent with this focus is Argyle's often critical stance regarding social psychological studies which, by dealing only with verbal interaction, overlook the essential interconnectedness of these two interactional modes, thereby producing incomplete and possibly incorrect findings.

The foundation for Argyle's treatment of social interaction consists of his analysis of the *elements* of interaction, both verbal and non-verbal. As noted, considerable emphasis is given the latter, e.g., bodily contact, posture, physical appearance, facial and gestural movements, eye contact, and the timing, tone, and errors of

speech. In terms of these, the actor's perceptions of other persons (who are viewed as other interactors making inferences about the actor) are analyzed and a model of social interaction constructed. Then in successive chapters Argyle shows how social interaction is influenced (1) by its frequent occurrence in small social groups; (2) by the fact that interactors often occupy positions within social organizations, and thus to some extent find their interaction "pre-programmed"; (3) by the personalities of the interactors, that is, by their tendencies to behave in similar ways across groups of situations; and (4) by the subjective notions interactors have of themselves, particularly as these influence their adoption of modes of self-presentation to induce others into seeing them in certain desired ways. In a separate chapter Argyle suggests several means whereby social interactional skills can be learned, developed, and modified.

Since the book is intended as a text in social psychology, the value of its treatment of social interaction in ways heretofore neglected must be weighed against its perhaps unavoidably narrow scope. Positing that "relations with others are the most important part of human life, and that most of the essential human characteristics cannot be manifested by a person in isolation" (p. 430), Argyle focuses his analysis on interaction, essentially that behavior which is involved *with* the behavior of other persons. Borrowing from stimulus-response theory and learning and cognitive theories, and employing the general notion of an equilibrium in dyadic systems, he constructs a model of interaction as a social skill, analogous to a complex motor skill, such that the interactor's "behavior, as when he is driving a car, is directed, adaptive, and far from automatic, though it may be seen to be built up of elements that are automatized . . . [we] have an individual carrying out a series of actions which are related to consequences that he has in mind to bring about: in order to do this he has to match his output with input available to him and to correct his output in light of this matching process" (p. 180). While Argyle admits to the need to elaborate the model to account for "special cognitive processes taking place during interaction, such as 'taking the role of the other'" (p. 186), and the fact that other interactors are involved in the same process, his nearly exclusive examination of social interaction leads him virtually to ignore whole segments of the traditional domain of social psychology. Among these are certain aspects of symbolic interactionism and the voluminous literature on attitude formation and change. There is little place in Argyle's model, for example, for the notion of taking the role of

the generalized other, since it may not be directly involved in the interaction process, or for dealing with the impact on social behavior of combinations of attitudes and objective circumstances involving the actor. Some who use this volume as a text may wish to supplement Argyle's approach with other material.

Two further features of the book deserve comment. First, the work is an impressive collection of findings and ideas that cross-cut several academic disciplines. However, a number of the research results and concepts are presented so briefly that the student who is not already somewhat familiar with the materials risks either over-estimating the definitiveness of the findings or not arriving at a full appreciation of how they support Argyle's argument. The uninitiated student, however, is provided with ample reference materials to guide his search for further understanding of a wide variety of topics. Second, Argyle is at times quite pointed in his criticism of traditional social psychological studies, particularly experimental studies. He thinks such studies often compel "people to behave not only like rats but like *solitary* rats" (p. 18). He especially objects to their "stripped-down" nature which often results in theories being constructed on the basis of "minimal social situations" that omit key elements and processes. Pedagogically, this position is especially noteworthy in that it involves criticism of traditional social psychology in terms of relatively recently acquired evidence. In a day where it is easy, if not fashionable, to attack social science for reasons that frequently have little empirical basis, Argyle's endeavor is to be both appreciated and held up to students as an example of good scholarly effort.

*Readings in Social Psychology*, by ALFRED R. LINDSMITH and ANSELM L. STRAUSS. New York: Holt, Rinehart and Winston, 1969. 357 pp. Paperbound. \$6.95.

*Personality and Social Behavior*, by KENNETH J. GERGEN and DAVID MARLOWE. Reading, Mass.: Addison-Wesley Publishing Co., 1970. 228 pp. Paperbound. \$3.50.

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Social psychology, a discipline in itself in some universities, continues mainly to be a course or a selected field of interest within anthropology, psychology, and sociology departments. Distinct disciplinary backgrounds, varied theoretical approaches, and diverse methodol-

ogies lead to a multifaceted social psychology, and what may be of interest to one brand of social psychologists may be considered totally irrelevant by another group. Therefore, judgment of a social psychology reader as valuable or not should be based upon the usefulness of the book to the audience to whom it is directed.

Lindesmith and Strauss in their text (*Social Psychology*) proclaim their position as symbolic interactionists, and focus principally on concepts and problems of special interest to sociologists. Their selection of articles for *Readings in Social Psychology* is in accord with the basic tenets set forth in their text, and the organization of the reader parallels that of the text. Attention given in the text to those who have laid the groundwork for and/or contributed to the development of symbolic interactionism is evident also in the reader. One finds articles by Mead, Dewey, Piaget, and Cassirer, and is not surprised to note that about half of the articles in the reader were published before 1960. In addition to the classical articles, there are selections representing current developments of the symbolic interactionist approach in medical sociology, deviance, sociolinguistics, mass communication, and interaction.

Social psychologists accepting the symbolic interactionist approach, and particularly those finding Lindesmith and Strauss' text helpful, will welcome this selection of readings for their undergraduate courses. The authors steer clear of explanations of social behavior based upon statistical associations, because of the possibility that such analyses may obscure the view of interaction as a process. Thus, even a student with little knowledge of experimental research methods should be able to grasp the message presented in this reader.

Gergen and Marlowe take an entirely different tack. These authors rely heavily upon experimental social psychology in their effort to demonstrate the relationship between personality variables and social interaction. The eleven articles selected to illustrate their position are highly empirical, presupposing a certain amount of statistical sophistication on the part of the reader.

The introductory essay is a revised version of a position paper written for the new *Handbook of Social Psychology*. At the time that of *Social Psychology*. At the time that Gergen and Marlowe prepared their original statement for the *Handbook*, they felt the need for a publication presenting their area of interest to the advanced undergraduate. For anyone who has been waiting for a new and elaborated article written specifically for the undergraduate, this "revised" version may be

disappointing. There is little difference between the introductory essay in *Personality and Social Behavior* and the chapter of the *Handbook* which, the authors say, was directed toward a graduate and professional audience. The "revised" version follows closely the original presentation, with no more than a few simple editorial changes: skipping a paragraph, splitting an exceptionally long paragraph, or slightly changing the wording of a sentence. Efforts at mere literary simplification hardly seem necessary considering the sophistication of the selected articles. Anyone who appreciatively

reads the accompanying articles should be able to read easily the original presentation of the position paper as it appeared in the *Handbook*.

The articles are carefully selected to cover the historical progress in the field and also to acquaint the reader with a wide range of problem areas and methods for studying such diverse topics. This book should be well received by those who up to this time have looked in vain for a single volume containing both a unified treatment of the relation between personality factors and interaction and an intelligent sampling of research in this area.

## DELINQUENT REVIEWS

*The following books, published between 1963 and 1969, were all selected for review and sent to members of the profession who agreed to review them. Regrettably, these individuals have never submitted their reviews, despite repeated reminders, nor have they returned the books so that others could review them. We want to inform both the authors of the books and the entire profession that the ASR did not ignore these books, and would have reviewed them if the reviewers had cooperated. Indeed, we will still publish these reviews if we ever receive them.—M.E.O. & A.T.T.*

*Interpersonal Dynamics: Essays and Readings on Human Interaction*, edited by WARREN G. BENNIS, EDGAR H. SCHEIN, FRED I. STEELE, and DAVID E. BERLEW. Revised Edition. Homewood, Ill.: The Dorsey Press, 1968. 766 pp. College price, \$9.25.

*African Rural-Urban Migration: The Movement to Ghana's Towns*, by JOHN C. CALDWELL. New York: Columbia University Press, 1969. 257 pp. \$10.00.

*Conflict and Conformity: A Probability Model and Its Application*, by BERNARD P. COHEN. Cambridge, Mass.: The MIT Press, 1963. 186 pp. \$7.50.

*Readings in Industrial Sociology*, edited by WILLIAM A. FAUNCE. New York: Appleton-Century-Crofts, 1967. 599 pp. No price indicated.

*Industrial Relations and Social Change in Latin America*, by WILLIAM H. FORM and ALBERT A. BLUM. Gainesville, Fla.: University of Florida Press, 1965. 177 pp. \$6.50.

*Turner and the Sociology of the Frontier*, edited by RICHARD HOFSTADTER and SEYMOUR MAR-

TIN LIPSET. New York: Basic Books, 1968. 232 pp. \$5.95.

*Sociology and History: Methods*, edited by SEYMOUR MARTIN LIPSET and RICHARD HOFSTADTER. New York: Basic Books, 1968. 423 pp. \$7.95.

*Philosophy of World Revolution: A Contribution to an Anthology of Theories of Revolution*, by FRANZ MAREK. New York: International Publishers, 1969. 141 pp. Clothbound, \$5.95. Paperbound, \$1.95.

*Migrants, Urban Poverty, and Instability in Developing Nations*, by JOAN M. NELSON. Occasional Papers in International Affairs, No. 22. Cambridge, Mass.: Harvard University Center for International Affairs, 1969. 51 pp. Paperbound. \$2.25.

*Attitude, Ego-Involvement, and Change*, by CAROLYN W. SHERIF and MUZAFAER SHERIF. New York: John Wiley and Sons, 1967. 316 pp. \$9.75.

*Man-Machine Simulation Models: Psychosocial and Performance Interaction*, by ARTHUR I. SIEGEL and J. JAY WOLF. New York: Wiley-Interscience, 1969. 177 pp. \$9.95.

# CURRENT FOREIGN-LANGUAGE SOCIOLOGY BOOKS

## FRENCH

- Sociologie de Saint-Simon*, by PIERRE ANSART. Paris: Presses Universitaires de France, 1970. 213 pp. Paperbound. 12 F.
- École et société au Québec: Éléments d'une sociologie de l'éducation*, by PIERRE W. BÉLANGER and GUY ROCHER. Montréal, Canada: Editions HMH, Ltée, 1970. 465 pp. Paperbound. No price indicated.
- Protestantisme et Capitalisme: la controverse post-weberienne*, by PHILIPPE BESNARD. Paris: Librairie Armand Colin, 1970. 426 pp. Paperbound. No price indicated.
- L'Analyse des processus sociaux*, by FRANÇOIS CHAZEL, RAYMOND BOUDON, and PAUL LAZARSFELD. The Hague, the Netherlands: Mouton and Co., 1970. 413 pp. Paperbound. 25 Dutch Guilders.
- Aspirations et transformations sociales*, by PAUL-HENRY CHOMBART DE LAUWE. Paris: Editions Anthropos, 1970. 385 pp. Paperbound. No price indicated.
- Des hommes et des villes*, by PAUL-HENRY CHOMBART DE LAUWE. Paris: Petite Bibliothèque Fayot, 1963. 267 pp. Paperbound. No price indicated.
- Images de la culture*, by PAUL-HENRY CHOMBART DE LAUWE, et al. Paris: Petite Bibliothèque Payot, 1970. Paperbound. No price indicated.
- Organisation sociale des Peul: Étude d'ethnographie comparée*, by MARGUERITE DUPIRE. Paris: Librairie Plon, 1970. 624 pp. Paperbound. No price indicated.
- La Psychologie sociale: Une discipline en mouvement*, by DENISE JODELET, JEAN VIET, and PHILIPPE BESNARD. The Hague, The Netherlands: Mouton and Co., 1970. 470 pp. 22 Dutch Guilders.
- Essai d'une technique projective en sociologie de la religion: Le jeu de construction*, by NGUYỄN THỊ THANH-TUYẾN. Brussels, Belgium: Editions Lumen Vitae, 1970. 347 pp. Paperbound. No price indicated.

## GERMAN

- Österreichisches Jahrbuch für Soziologie, 1970: Mit Bibliographie 1960-69*, by ERICH BODZENTA. Vienna, Austria: Springer-Verlag, 1970. 260 pp. \$9.50.
- Zweispaltige Eliten: Eine sozialpsychologische Untersuchung über administrative Eliten in Tailand*, by ERNST E. BOESCH. Berne, Switzerland: Verlag Hans Huber, 1970. 333 pp. Paperbound. Fr./DM 28.
- Entwicklungsbewusstsein und wirtschaftliche Entwicklung in Indien*, by WERNER DRAGUHN. Band 28. Wiesbaden, Germany: Verlag Otto Harrassowitz, 1970. 288 pp. DM 58.
- Industrie und Gemeinde: Die Beziehungen zwischen der Stadt Wolfsburg und dem Volkswagenwerk und ihre Auswirkungen auf die kommunale*

*Selbstverwaltung*, by HERMAN HILTERSCHIED. Berlin, West Germany: Berlin Verlag, 1970. 248 pp. DM 50.

- Soziologische Aufklärung: Aufsätze zur Theorie sozialer Systeme*, by NIKLAS LUHMANN. Köln, Germany: Westdeutscher Verlag, 1970. 268 pp. Clothbound, DM 38. Paperbound, DM 24.
- Methodologie der Sozialwissenschaften: Einführung in Probleme ihrer Theorienbildung*, by KARL-DIETER OPP. Reinbek bei Hamburg, Germany: Rowohlt Taschenbuch Verlag, 1970. 332 pp. Paperbound. No price indicated.
- Tendenzen der Sexualforschung*, edited by GÜNTER SCHMIDT, VOLKMAR SIGUSCH, and EBERHARD SCHORSCH. Stuttgart, Germany: Ferdinand Enke Verlag, 1970. 146 pp. Paperbound. DM 25.
- Gewaltlose Politik und kulturelle Vielfalt: Hypothesen entwickelt am Beispiel der Schweiz*, by JÜRGE STEINER. Berne, Switzerland: Verlag Paul Haupt, 1970. 365 pp. Paperbound. SFr/DM 48.
- Kerala: Politisch-soziale Struktur und Entwicklung eines indischen Bundeslandes*, by MANFRED TURLACH. Band 26. Wiesbaden, Germany: Verlag Otto Harrassowitz, 1970. 386 pp. DM 78.
- Sexualunterdrückung: Geschichte der Sexualfeindschaft*, by JOS VAN USSEL. Reinbek bei Hamburg, Germany: Rowohlt Taschenbuch Verlag, 1970. 248 pp. Paperbound. No price indicated.
- Erkenntnis und Veränderung: Zu einer liberalen Sozialwissenschaft*, by ERHARD R. WIEHN. Tübingen, Germany: Verlag Ely Huth, 1970. 179 pp. Paperbound. No price indicated.

## ITALIAN

- Classi e generazioni*, by FRANCESCO ALBERONI. Bologna, Italy: Società editrice il Mulino, 1970. 211 pp. Paperbound. L. 2.500.
- Partito e comunità locale*, by GIANFRANCO BETTIN. Bologna, Italy: Società editrice il Mulino, 1970. 118 pp. Paperbound. L. 1.500.
- L'immaginazione sociologica di C. Wright Mills*, by GIORGIO MARSIGLIA. Bologna, Italy: Società editrice il Mulino, 1970. 150 pp. Paperbound. L. 2.000.
- Dati su ottanta casi di omicidio*, by MARIO SIMONDI. Serie Ricerche Empiriche n. 5. Florence, Italy: Università degli studi di Firenze, 1970. 135 pp. Paperbound. No price indicated.

## SPANISH

- Los Universitarios opinan*, by DEPARTAMENTO DE ESTUDIANTES Y GRADUADAS DE LA SECCIÓN FEMENINA DEL MOVIMIENTO. Madrid, Spain: Ediciones Almena, 1970. 135 pp. Paperbound. No price indicated.
- El oficio de las letras: Estudio sociológico de la vida literaria*, by HERNÁN GODOY. Santiago, Chile: Editorial Universitaria, 1970. 258 pp. Paperbound. No price indicated.



# PUBLICATIONS RECEIVED

(Listing of a publication below does not assure or preclude its subsequent review.)

- ABCARIAN, GILBERT and JOHN W. SOULE, editors. *Social Psychology and Political Behavior: Problems and Prospects*. Columbus, O.: Charles E. Merrill Publishing Co., 1971. 209 pp. Paperbound. \$2.95.
- ABT, JOHN J. *Who Has the Right to Make War? The Constitutional Crisis*. New York: International Publishers, 1970. 64 pp. Paperbound. \$.95.
- ADAMS, BERT N. *The American Family: A Sociological Interpretation*. Chicago, Ill.: Markham Publishing Co., 1971. 378 pp. \$7.95.
- ADLER, ALFRED. Edited by GEORGE H. LAPORTE. *Sex, Personality and the Establishment: Guidelines for Social Re-education*. New York: George H. LaPorte, 1970. 69 pp. Paperbound. \$2.25.
- ALBERONI, FRANCESCO. *Classi e generazioni*. Bologna, Italy: Società editrice Il Mulino, 1970. 211 pp. Paperbound. L. 2.500.
- ALBRECHT, MILTON C., JAMES H. BARNETT, and MASON GRIFF, editors. *The Sociology of Art and Literature: A Reader*. New York: Praeger Publishers, 1970. 752 pp. \$15.00.
- ALISKY, MARVIN. *Guide to the Government of the Mexican State of Sonora*. Tempe, Ariz.: Arizona State University Center for Latin American Studies, 1971. 48 pp. Paperbound. \$1.00.
- ALTMAN, ROBERT A. and PATRICIA O. SNYDER, editors. *The Minority Student on the Campus: Expectations and Possibilities*. Berkeley, Calif.: University of California Center for Research and Development in Higher Education and Western Interstate Commission for Higher Education at Boulder, Col., 1970. 219 pp. Paperbound. \$3.50.
- ANDERSON, NELS. *The Industrial Urban Community: Historical and Comparative Perspectives*. New York: Appleton-Century-Crofts, 1971. 438 pp. \$10.95.
- ANGLIN, JEREMY M. *The Growth of Word Meaning*. Cambridge, Mass.: The MIT Press, 1970. 108 pp. No price indicated.
- ARGYRIS, CHRIS. *Intervention Theory and Method: A Behavioral Science View*. Reading, Mass.: Addison-Wesley Publishing Co., 1970. 374 pp. \$9.50.
- BALANDIER, GEORGES. Translated by A. M. SHERIDAN SMITH. *Political Anthropology*. New York: Pantheon Books, 1970. 214 pp. \$6.95.
- BALL, JOHN C. and CARL D. CHAMBERS, editors. *The Epidemiology of Opiate Addiction in the United States*. Springfield, Ill.: Charles C Thomas, 1970. 337 pp. \$15.50.
- BANKS, J. A. *Marxist Sociology in Action: A Sociological Critique of the Marxist Approach to Industrial Relations*. Harrisburg, Pa.: Stackpole Books, 1970. 324 pp. \$11.50.
- BAR-ADON, AARON and WERNER F. LEOPOLD, editor. *Child Language: A Book of Readings*. Englewood Cliffs, N. J.: Prentice-Hall, 1971. 477 pp. \$11.95.
- BARBA, HARRY. *How to Teach Writing in the Time It Takes to Consume a Glass?—A Quari?—A Barrell of Wine*. Saratoga Springs, N. Y.: The Harlan Press, 1969. 16 pp. Paperbound. \$.50.
- BARBA, HARRY. *Love, in the Persian Way*. Saratoga Springs, N. Y.: The Harlan Press, 1969. 45 pp. Paperbound. \$.50.
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